COLUMBIA

ACCIDENT INVESTIGATION BOARD

Note: Volumes II – VI contain a number of conclusions and recommendations, several of which were adopted by the Board in Volume I. The other conclusions and recommendations drawn in Volumes II – VI do not necessarily reflect the opinion of the Board, but are included for the record. When there is conflict, Volume I takes precedence.

REPORT VOLUME VI

OCTOBER 2003

On the Front Cover

This was the crew patch for STS-107. The central element of the patch was the microgravity symbol, µg, flowing into the rays of the Astronaut symbol. The orbital inclination was portrayed by the 39-degree angle of the Earth's horizon to the Astronaut symbol. The sunrise was representative of the numerous science experiments that were the dawn of a new era for continued microgravity research on the International Space Station and beyond. The breadth of science conducted on this mission had widespread benefits to life on Earth and the continued exploration of space, illustrated by the Earth and stars. The constellation Columba (the dove) was chosen to symbolize peace on Earth and the Space Shuttle Columbia. In addition, the seven stars represent the STS-107 crew members, as well as honoring the original Mercury 7 astronauts who paved the way to make research in space possible. The Israeli flag represented the first person from that country to fly on the Space Shuttle.

On the Back Cover

This emblem memorializes the three U.S. human space flight accidents – Apollo 1, Challenger, and Columbia. The words across the top translate to: "To The Stars, Despite Adversity – Always Explore"

The Board would like to acknowledge the hard work and effort of the following individuals in the production of Volumes II – VI.

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VOLUME I

VOLUME II CAIB TECHNICAL DOCUMENTS CITED IN THE REPORT

Volume VI
Appendix H

Transcripts of Board Public Hearings
Reader's Guide

In the course of its inquiry into the February 1, 2003 destruction of the Space Shuttle *Columbia*, the Columbia Accident Investigation Board conducted a series of public hearings at Houston, Texas; Cape Canaveral, Florida; and Washington, DC. Testimony from these hearings was recorded and then transcribed. This appendix, Volume VI of the Report, is a compilation of those transcripts. The transcripts are also contained on the DVD disc in the back of Volume II. The video recordings of these hearings are included in the records of the CAIB, held at the National Archives and Records Administration.

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March 6, 2003
Houston, Texas

Columbia Accident Investigation Board Public Hearing
Thursday, March 6, 2003

10:00 a.m.
Bayou Theatre
University of Houston at Clear Lake
Bay Area Boulevard
Houston, Texas

Board Members Present:
Admiral Hal Gehman
Rear Admiral Stephen Turcotte
Major General John Barry
Major General Kenneth Hess
Dr. James N. Hallock
Brigadier General Duane Deal
Mr. Roger E. Tetrault
Dr. Sheila Widnall

Witnesses Testifying:
General Jeff Howell
Mr. Ron Dittemore
Mr. Keith Chong
Mr. Harry McDonald

ADM. GEHMAN: Good morning, ladies and gentlemen. The first public hearing of the Columbia Accident Investigation Board is hereby in session. We are going to begin our review this morning by talking to two officials of NASA who work here at JSC. We're going to be talking about organizational and lines-of-responsibility kinds of matters so we have a clear understanding of who does what and how you get it done and who answers to whom.

We're delighted to be able to start right at the top here at JSC with the Center Director, General Howell, Jeff Howell.

Thank you very much for taking time to be here. We also are aware that you've got duties that are going to call you away here; and those duties, of course, are related to this accident, for which we are understanding and appreciative.

Before we begin, the way we'll conduct this public hearing is Jeff Howell, Director Howell, will make an opening statement, which we'll be delighted to listen to. Then we will just simply ask questions as the Board sees fit.

Before we begin, though, Mr. Howell, let me first ask you to affirm that the information that you will provide to this Board at this hearing will be accurate and complete to the best of your current knowledge and belief.

THE WITNESS: I so affirm.

ADM. GEHMAN: All right, sir. The floor is yours.

JEFF HOWELL, having been first duly affirmed, testified as follows:

THE WITNESS: Thank you, Admiral. I'm pleased to appear before the Columbia Accident Investigation Board. It's now 33 days after the tragic loss of the courageous crew of Space Shuttle *Columbia*. We are deeply appreciative of the efforts of the Board to determine what caused the loss of *Columbia* and its crew, and we pledge to continue to cooperate and support your efforts in every possible way.

I'd like to begin by describing Johnson Space Center's role in our nation's space program. Originally named the Manned Spacecraft Center, JSC has served as a focal point for human space exploration since the early 1960s. The core capabilities resident at JSC since the beginning and continuing today consist of the design, development, and test of human spacecraft and human robotics interfaces; planning, execution, and control of human spacecraft; selection, training, and assignment of astronaut crew members; extravehicular planning of hardware development and training; life science research related to human space flight and associated biomedical research; the program management of large-scale human space flight hardware development programs; the study and curation of astro-materials; and last but not least, the safety, reliability, and quality assurance expertise to support all of these activities.

Within this context, as the director of the Johnson Space Center, I am responsible for providing the Shuttle Program with the institutional support needed to execute the Space Station Program's mission. The center is accountable for the hardware and software it delivers to the program as well as the quality and technical content of the analysis products it delivers to the program. Center management works closely with the Space Station Program manager, Ron Dittemore; and I am regularly apprized of program status and issues, as well as personnel and other matters.

I will be happy to discuss my understanding of these roles and relationships. Thank you, sir.

ADM. GEHMAN: Thank you very much. I'll ask the first question since I'm the chairman. Would you describe for us the lines of authority and chain of command, as we say in the military, lines of authority that starts with Mr. O'Keefe, a couple of layers above you, one layer above you, and perhaps one layer or two layers below you. Describe it; but then, if you would, expand it to if there are any branches or sequels -- for example, if the money is done differently than hiring and firing or something.

THE WITNESS: Of course, under Mr. O'Keefe is his Deputy Administrator, Fred Gregory; and under the two of them, he has his enterprise Associate Administrators. Code M, which is the Office of Space Flight, is headed by Bill Readdy. He is my boss. I'm one of the Office of Space Flight Center Directors. We have four -- myself, Marshall, Kennedy, and Stennis. As the Center Director below me, I have an immediate staff of direct reports -- you know, Legal, HR, that type -- plus I have directors of our major activities, engineering, mission operations division, the flight crew operations, our extravehicular activities, space and life sciences, and then safety and mission assurance -- safety, reliability, and quality assurance.

So those are my major activities, and each of them has a director. Under them are their branch managers and so on. So the largest of those directors is our engineering and then our flight crew operations division. Those are the two largest ones I have.

ADM. GEHMAN: And Mr. Readdy also has various projects, a direct report to him also; and we're going to hear from one of those projects later.

THE WITNESS: Correct.

ADM. GEHMAN: So that means, then, the way the wiring diagram works out, that the projects and the centers operate in parallel to each other. Is that a safe way to say that?

THE WITNESS: That's correct. Of course, under him he has an Associate Administrator for these programs, General Mike Kostelnik, and he has both the Shuttle and Space Station Programs under him. So he's the direct line of authority to Mr. Dittemore. However, you know, down at our level, Ron Dittemore and I are literally joined at the hip in the way we function because a big portion of my center personnel support his activities and we are intertwined in a very complex organization in that regard.

ADM. GEHMAN: Thank you very much.

General Barry, you want to lead off since we're talking about Shuttle support?

GEN. BARRY: Sure. Could you go into more detail. A two-part question really. Responsibilities of the astronaut office in regard to your responsibilities. And then could you outline your role before and after the *Columbia* mishap.

THE WITNESS: Certainly. The astronaut office, the actual office is called Flight Crew Operations Directorate, and Bob Cabana is the head of that. Under him he has several different divisions; but the major one is the flight crew office, the astronaut office. So he is charged under me to recruit and select and then train our astronauts to get up to a level where they are designated astronauts. They go through a very vigorous almost two-year training program to qualify to go on to become a crew of either a Shuttle or a Station. So he's charged with that responsibility.

Under him are several activities to do that. He has, you know, an aviation division where he has aircraft that our military air crew have to stay current in, and he has the training aircraft for the astronaut pilots that simulate a reentry of a Shuttle. They have that type of capability, all those things. I am responsible for all of this. He is accountable. He does this for me in that regard. Does that answer your question?

GEN. BARRY: That's something I think few people understand, the difference between Ron Dittemore's responsibilities and your responsibilities for the astronauts.

THE WITNESS: Now, Bob, you know, has to make sure, has to ensure that his astronauts are ready to perform their functions for Ron as members of a Shuttle crew. We share responsibility in that with our mission operations director, though, because under the mission operations director they're the ones who actually design the missions and build the whole milestone of activities to prepare for the missions and to conduct the missions. So the astronauts actually are trained by members of our Mission Operations Division. That's where they get their specific training for the missions they fly on.

So the MOD under John Harpold is really the directorate of mine that does that function for them. So it's the next step beyond being an astronaut now to train for a mission. You're basically directed and under the auspices of the Mission Operations Division in planning the mission, and they're the same ones who control them when they're in space.

GEN. BARRY: The second part of the question is could you explain your responsibilities so the Board understands what is the Center Director's role insofar as the Shuttle mission is concerned, what were you doing before, and just kind of a general outline of the responsibility that would be on any normal launch.

THE WITNESS: I don't have any direct responsibility over the Shuttle Program or the missions themselves. However, as I said before, we're so intertwined with our activities that I have members of my staff and members of my organization who support all of their activities. So I have a responsibility to make sure that they do their jobs correctly. Also as part of the budget process, we have activities that are defined by the program that they assign to us and, of course, we work out a budget with them and we are given tasks that we have to perform in support of the program. And, of course, I'm responsible for making sure that it -- it could be hardware products coming out of engineering, it could be software, and also the activities out of MOD. And I'm responsible to make sure those are done correctly. So that's the type of oversight I have in that regard.

Now, on a higher level, I'm also a member of the Office of Space Flight Management Council; and that is under Mr. Readdy. The members are the Center Directors and his Deputy or Associate Administrator, Mr. Kostelnik. We gather on a regular basis to discuss policy, discuss issues, and we all have a voice in that regard. That's another indirect oversight that we have in influencing what might occur or not occur in the Shuttle Program.

I am also a member of the Flight Readiness Review. We meet approximately two weeks prior to every Shuttle mission; and we have a very formal, extensive, comprehensive review of every aspect of the mission. I am a voting member of that Board. I sit at the table at the FRR that is chaired by Mr. Readdy and as a voting member, I can participate in questions and answers of any of the people who brief it and also I have a vote as more of, I guess, on the level of a Board of directors and I sign the certificate for flight. So I do have that type of oversight on a personal level, direct level.

GEN. BARRY: Thank you.

ADM. GEHMAN: Ken, do you want to be recognized?

GEN. HESS: One of the constant things that we see and hear about is talking about the debate about enough resources and staffing to conduct the mission that we have here. You laid out for us a pretty articulate description of a very complex, highly matrixed organization. Could you go into your personal feelings about staffing and resources?

THE WITNESS: I think we're in good shape. The majority of our people who work at the Johnson Space Center are contract employees. Just to let you know, on site on a daily basis, we have about 10,000 people working here every day. 3,000 are civil servants; the other seven are contracted people. Even in the surrounding area for support of our activities, another 6,000 or so contractors who support our activities. So it's truly a team effort. When I look at that team that we have right now, I am very pleased. I think we have a very highly qualified, gifted, dedicated, and committed team of men and women who support our activities and get the job done.

If I have a concern, it's always the balance between civil service and contractors. What's a critical mass of civil servants necessary to ensure that we have the proper skills to oversee our contractor activity. I am very confident that we have that at this time. The issue, of course, always is, within our 3,000 civil servants, our skill level, our experience level. We're in great shape right now. However, I have a concern because a very large number of our civil servants are at the age where they may retire in the next several years. So I have that challenge in the future ahead of me; but as we speak right now, I am very confident in the capabilities and skill levels of our `people and our ability to support the Shuttle Program.

GEN. HESS: As a follow-up, you mentioned that one of the direct reports you have is for the safety and mission assurance area. Could you explain to us how that functions and how that works in parallel during the flight readiness process?

THE WITNESS: Once more, it is complex; but I think it's very effective. Every activity that supports our human space flight program, each one of my directorates, each one of our contractors, United Space Alliance, Lockheed, Boeing and so on, they all have quality assurance, safety people and the like because everybody is totally intent on making this a safe activity at all levels and all the way to the end. However, because of the critical nature of our activity of having people exposed to this environment, I think it's imperative upon me to have a separate organization, a safety, reliability and quality assurance organization that is an added dimension for oversight to ensure that everybody's really doing their jobs and taking care of business.

There are several facets to this. One is we actually use them to support the program and have actual activities with the review boards and a report of the program team being with them and participating in the design and development, just to ensure that, from our point of view, everything is done according to Hoyle. But another aspect of it is I retain the right, since the astronauts belong to me, I have the right to have my own oversight in activities to ensure that everything is done to reduce the risk to the men and women who go in those machines, as well as the men and women who work with those machines. So that is another aspect of that organization. They work for me directly to do that.

So there's a combination. They work in concert with the program to assist them in what they do, but they also have the right to come to me with any kind of concerns about anything that might be going on and I can take that directly to Mr. Readdy or whomever.

GEN. HESS: Thank you very much.

MR. TETRAULT: Sir, did the Shuttle Program manager ever report directly to the Johnson Space Center?

THE WITNESS: At this time, no. He did.

MR. TETRAULT: Yes. And how long ago was that?

THE WITNESS: Just less than a year ago. I became the Center Director on 1 April of last year. So I've not been here quite a year; but right after Mr. O'Keefe became the Administrator, the decision was made to take the two major programs in Code M, both the Shuttle and Station, and move them under the direct leadership of the Johnson Space Center director and up to the Deputy Associate Administrator for space flights. So this was, I think, a result of the Young Committee's suggestions and recommendations. So that decision was made and we went through a transition period. The transition period had begun when I arrived in April, and by summer we had moved the total responsibility for those programs under General Kostelnik. So it's been fairly recently. If you look over the long term in the history of NASA, this authority has been moved back and forth from the center to the headquarters a couple of times, I believe; but this was the last iteration of that.

MR. TETRAULT: Thank you.

DR. HALLOCK: As I understand the Shuttle Program, there are four centers that really are very much involved with it -- your own, Kennedy, Marshall, and Stennis. I'm just curious what kind of interactions you have at your level with these other groups.

THE WITNESS: With the other centers?

DR. HALLOCK: Yes.

THE WITNESS: We communicate quite regularly. I think sometimes, given what the issues are, I might be communicating every day with Roy Bridges at Kennedy or Art Stephenson at Marshall. Other times we'll go a week or so without talking to each other. So really at our level we sort of hit the hot buttons and talk to each other over major issues.

At a lower level, we have a continuous liaison, communications and actual integrated work with the other centers with our engineers. We actually have a virtual engineering capability with Marshall where our engineers and their engineers sit down together and work out problems together on a regular basis. Our relationship with Kennedy is very close because, of course, that's where they process the vehicles and work with them and our astronauts are over there on a continuous basis for training and for familiarization. So below me, at a lower directorate level, there is a continuous flow of information and activity among the centers where they work with each other on a continual basis.

MS. WIDNALL: I actually have two questions. One is just a point of information. Who does the Mission Ops Directorate report to?

THE WITNESS: The Mission Operations Directorate reports to me.

DR. WIDNALL: Okay. So that report's to you.

The second question is that you spoke about the safety and mission assurance organization that works for you which, as I understand your description, is basically supposed to provide an independent assessment. Could you give me some examples of major program or mission changes that have occurred as a result of recommendations brought forward by the safety and mission assurance organization. Of course, I put in the word "major." I have no idea what major means; but if you can answer it now, I guess I would be interested if you could supply some examples for the record.

THE WITNESS: Right at this moment, I really don't have an example.

DR. WIDNALL: I understand.

THE WITNESS: I'll be happy to do that. Another aspect, just because of my capability of having leverage in these things, a lot of issues that they raise are worked out with the programs at a lower level. So it's a rare occasion when they would actually come to me.

I probably can't say it sufficiently, how important safety is to every person who works at that center. It's a way of life. You can say it's No. 1 first; but really if we were fish, it's the ocean we swim in. It's an attitude. So anytime anybody raises that flag at any level, it gets people's attention very quickly and people are going to take care of it. So since I have been the director, I don't really have an example. I do know that those things have happened in the past, and I'll be happy to get some examples.

DR. WIDNALL: I would be very interested.

GEN. BARRY: If I could have a follow-up question on your discussion about civil service. Since 1993, according to a report that we've got -- and I think you participated in this and I'd like your comment on it, the concept of privatization of the Space Station Program. One of the things that was stated in here is that since 1993, 50 percent of the civil service work force has been reduced at NASA. The specific wording is: "The NASA Space Station Program civil service work force has been reduced nearly 0 percent, resulting in significant loss of skills and experience." It says: "NASA's skill base continues to erode as more functions transition to the Space Flight Operations Contract." Now, some of that affects you obviously here. Could you give us some more information on your views on that and maybe some insight on your participation in this report.

THE WITNESS: I did not have any participation in that report. I think it goes back to my answer that I am very comfortable with the balance of civil servants versus contractors that we have at this time, and that has been a change from ten years ago. It has been a move toward contractors, increasing numbers doing functions that were done by civil servants in the days before.

I do have a concern for the future of, you know, once more, what is the critical mass necessary of civil servants in all the different disciplines that we participate with the programs to ensure that we have enough numbers to grow civil servants up to the expertise they need and so that we can have proper oversight on the civil service level. So at this time I'm very comfortable with it. I am very concerned about going any lower on our civil servants. However, I think it needs more study. I'm not saying we won't, but this is something that we need to really take a hard look at for the future.

ADM. GEHMAN: General Howell, we'll take turns here. You mentioned before in your description of your work force -- engineers, for example, the engineering department -- which are largely matrixed in support of a major project that you have here. Would you describe for us how that works as a practical matter? What I mean is if there's an engineering problem that must be solved or if one of the project people says I need more help here, I need more help there, who decides where the engineers work and how do you get reimbursed for that?

THE WITNESS: I guess it's a family matter, is the best way to put it. One of the extraordinary blessings we have here is that we have both the Shuttle and Station Programs located here and we support them with our Engineering Directorate and our other MOD and what have you. So every year they come out with guidelines on what type of support they want and we tell them what it's going to take to do it and we work out a budget. So we have a force of engineers -- we have two types. Each program actually has certain people who are assigned to them full time. For instance, in the Shuttle Program I have 700 plus full-time equivalent civil servants who work for the Shuttle. Now, I don't have the number for you; I'd have to get it for you. Not all 700 of them are full time. There are a certain number of those people are full time and the other number are matrixed work from several people who will take up one full-time equivalent. We have the same arrangement with the Station. We budget with them and work out an agreement on what type of work and what it will take to do it and I agree to the budget and then we go forward. And the budgets actually belong to the programs. So we make an agreement.

When something occurs, because of the family -- for instance, when we had the flow liner cracks, we went to full court press to figure out what had caused that, to do the metallurgy of that, all the what-ifs. So we set up a series of tiger teams to help analyze and come to agreement on what it took to fix that problem. I would say very easily that I took about 150, at least 150 engineers who were not part of the Shuttle Program who came to address that problem and work full time for several weeks to take care of that. Now, some of them came from our training people. Some came from Station on a loan. A legacy of Johnson Space Center is that, you know, you come on and you stay on when you get a problem and everybody turns to to do it. Everybody knows that that's the way we do it. How we work that out in the budget, I'll have to bring John Beall, my financial guy, in to tell you. I don't know, but we get it done and it is paid for. And there's such great cooperation between the programs that they're willing to, you know, do what it takes to help each other in that regard. I'll have to get you more specifics on really the details of how we do that.

ADM. GEHMAN: Good. I, for one, would be interested to know how that works out, not so much because I'm really interested in the accounting part of it but I am interested in the lines of authority considered. In other words, the cracks in the piping is a good example; and I certainly can appreciate the energy with which JSC attacks something like that, because it stopped the program for a period of months. I would like to know better whether or not that tiger team, for example, as you described it, was working independently, whether it was working for you, or whether it was working for the project manager.

THE WITNESS: They were working for the project manager. They asked for help. We offered what we could do to help them. They agreed to that, and those people worked for them. Ron Dittemore was in charge of that operation. We just provided him with a lot of expertise that he didn't normally have to help him solve that problem. Once more, though, I feel a responsibility that those people did the correct thing and came up with the correct analysis and testing. So I have something in this. However, they did belong to Ron Dittemore in that regard.

ADM. GEHMAN: I hate to jump around subjects here, but you said that you and other Center Directors, of course, are part of the COFR process, as I understand, the Certificate Of Flight Readiness?

THE WITNESS: Yes.

ADM. GEHMAN: And you actually sign on it. When you do that, of course, you are expressing your overall satisfaction in your sphere of knowledge, that you're ready to go; but except for that generality, what interests are you actually representing? Are you representing the interests of the astronauts? Are you representing the interests of the engineering department and the flight directors? More? Less? Have I overstated it? Would you characterize that?

THE WITNESS: Not at all. That's a very somber signing. It's very important. In my mind, I am representing every JSC person who has been involved in that, my confidence in them. It's also as a member of the management council for Bill Readdy. I feel that I am actually affirming everything that's gone on that I don't really have control over either. So I think I'm signing for the entire Office of Space Flight as a member of that Board with my approval.

Let me say, though, before I go to that FRR, every one of my directorates who are involved in the Shuttle preparation, MOD, the operations directorate, SR&QA, our flight crew operations directorate, engineering -- they all have their own separate FRRs where they go over every detail, every piece of paper that's been processed and every action that's been taken so that they are satisfied in their own mind that this thing is ready to go. Then they come brief me. I don't have a formal FRR per se, but we have a meeting and I am given a thorough briefing by all those heads so I can be confident when I go in that FRR that I can make that vote. They also raise, you know, issues that have come before, how they've been mitigated, and any issue that might be briefed at the FRR. So I am fully aware what issues might be raised and I'm ready to go be a participant in that.

ADM. TURCOTTE: Sir, following along the lines you described about the matrixed organization that is NASA and alluding to a little bit of Major John Barry's comments earlier about privatization of the process and then the recent organizational changes. As in any matrixed organization, one authority has lines of responsibility; and, more importantly, lines of resource flow in different directions. Are you in a better position now than you were a couple of years ago, as a result of the changes, in order to leverage that matrixed organization to get the work done that you need to do as both, 1, a director and, 2, as a signer on the COFR?

THE WITNESS: That's a good question. I don't know if I can answer it because I am a newbie. All I know is what I know since I've been there. Being a former commander in the Marine Corps, I'd like to be in charge of the whole thing. I am very comfortable with the way that it's organized.

Now, I think it was very timely, you know, if I can put on a NASA hat, a Sean O'Keefe hat, and look at why they decided to change that authority directly up to the office, when you look at what we're going to do in the future, looking at the SLI program that's going to come up, the orbital space plane, all those things are going to have to be intertwined and decisions are going to have to be made with all those things affecting each other. I think it's to NASA's advantage to have the heads of those programs up in Washington where all that can be worked together. So I think it was a very wise move, frankly, if I'm sitting in Sean O'Keefe's chair; and I'm very comfortable, because of the relationship I have with both him and with Bill Readdy and Mike Kostelnik, that any issues I might have on budget or what have you, I get a fair hearing and we get it resolved. So I really am very comfortable with our organization the way it is now.

The privatization, you know, the SFOC contract, I guess, was sort of a move in that direction. The organization I have now is what I inherited almost a year ago. So nothing has changed in that regard, and I'm very comfortable with the organization we have now.

GEN. BARRY: Sir, a lot of changes have occurred here at NASA during the last two or three years. You cited one of the Center Director responsibilities being shifted. The other one is the movement of the contractor support from Huntington Beach to JSC. Could you comment a little bit about that and your concerns, if any, about that move, particularly with regards to expertise, qualifications of the folks, and has that strengthened you. Advantages and disadvantages.

THE WITNESS: I think the move was really a very wise move by Boeing to get more central and get closer to the customer with the people who need to serve them. There is a concern, though, that he left a lot of expertise back in California. A lot of people didn't want to move. Mike Mott and I have had a lot of discussions about that. He's assured me that he is bringing in the right kind of expertise, that we can be confident in his products and what he's got to do. So I think what he did was the right thing to do.

There is friction and a hiccough anytime you do something like that. Things at first are not quite as good as they were before. But I think he has a very excellent plan to get back on step and be just as strong as he was and actually better because he's going to have a more centralized organization that can respond a lot quicker to the needs of the program.

GEN. BARRY: Just a follow-up. Did you have any involvement that you comment on and give us some help in trying to understand? I know we talked a little bit about this with Mr. Dittemore. When the OMM was moved from Palmdale to KSC, were you involved in that decision advice-wise with providing some counsel?

THE WITNESS: No, I really wasn't. That decision was made before I became the Center Director. So I had no involvement in it.

DR. WIDNALL: Could you describe your role in the issues like the resolution of in-flight anomalies, the design or material waivers that need to be granted, what I would refer to as escapements, which basically means approving hardware that for one reason or another just does not meet spec or a situation where something happened on a flight that maybe shouldn't happen, is not understood? Could you describe your role in that and then also indicate whether there is a formal process for resolution of those anomalies, in the general sense of anomaly?

THE WITNESS: We have a Mission Management Team which is tasked with, on a daily basis, having oversight over the mission and taking care of anomalies and mitigating any kind of problems they might have while they are on a mission or in space. I am not involved in that directly. However, I have several of my direct reports from engineering, from MOD, and other of my directors are on that team. So I have a sense of responsibility to make sure we have the right people on that team, but those are really experts at what they do. It's a very robust organization.

The MMT, you'll have a table and I'm not sure how many, let's say a dozen people at the table, but I assure you behind each one of those people are at least a dozen other engineers dedicated to help them resolve whatever issues they have. This is really serious business, and we commit a very robust engineering and operations team anytime we have a mission ongoing. It's at an expert technical level of our best technicians who do that. So I am not a part of that. I certainly don't have the qualities to be a part of that, but I feel responsible to make sure we have the right people.

DR. WIDNALL: Well, I'm not just talking about what happens in flight. I'm also talking about as the vehicle is certified as being ready for flight and some of the issues of hardware that doesn't quite for one reason or another meet some original specification and there's a waiver granted of some sort.

THE WITNESS: Well, because of our involvement with the program and participation in almost every aspect of these activities, I have people who are involved in all of those issues. I get regular briefings about that. Anytime there are any issues like that, I'm aware of them. And Ron Dittemore and I tag up every week and we'll discuss these things together. If I have any concerns, I'll let him know those things. So we work -- I am continuously apprized of any anomalies or issues that might be going on in preparation for a mission or anything like that.

DR. WIDNALL: But you're not part of a formal sign-on process?

THE WITNESS: No, I'm not. I'm not on that decision.

GEN. HESS: General, you've done a very good job this morning explaining to us how this highly complex organization comes together and talked a little bit about the structure and resources that we have. My question would be that here next year the Space Station is supposed to be core complete. I'd like you to talk to the Board a little bit about stresses to maintain schedule and impacts that you see in the future.

THE WITNESS: Well, we have been at Johnson -- I have to be very careful because I have been accused of being too success oriented, and that's sort of the nature of the beast at our center. One thing we have going for us, though, is we have an administrator who is just beating upon us how important safety is and that should be our first primary consideration in everything we do. He starts every meeting saying that and he ends every meeting saying that. We are very eager and excited about getting this Station assembled and the missions -- I better be careful; I'll get carried away.

You know, the complexity of these assembly missions is astounding. For me, watching how our people with the program put these things together and with the other centers and take this very huge, complex machinery up into space, get it connected and operating -- it's phenomenal. I think it's one of greatest achievements in the history of humankind, frankly. That's my opinion. So I'm excited about that, and we're eager to get on with it and get that done.

However, we understand the stakes and we are not going to do anything to impress anybody to put aside any kind of quality assurance or safety issue. I think that was very evident. You know, our administrator declared that we're going to have core complete by February of '04. We wanted to make that happen. However, we had a flow liner crack and we came to parade rest until we got that done. That was several weeks or a month. Then we had the BSTRA ball issue. Stopped. Never a peep or a complaint from our administrator or higher headquarters. They understood that those things have to be resolved before we're going to commit people to flight. So I'm just very comfortable with the attitude of our whole organization, from the top person down, of what is really important and what our priorities are.

ADM. GEHMAN: General Howell, I would with some degree of hesitancy speak for the Board here in which the general impression of the Board as we have traveled to all of the centers involved in the manned space flight, walked on the production floors and crawled over them and met with all levels of NASA and contractor employees that the Board generally agrees with your assessment of safety, what I would call tactical safety. If somebody finds something wrong, there's no pressure or hesitation to go fix it; but critics of NASA, while admitting that you have a very enviable and rigorous and mature safety program, also sometimes say that NASA misses the big safety issues -- that is, that the process of repeatedly fixing things and then certifying the fix to make sure that it's better than original, that sometimes you tend to miss the trends that you shouldn't have ever had to address that problem at all, that whatever it is that you're working on was not designed to fail and the fact that you're working on it is telling another story. Would you just give me your views of whether or not this is not well-informed advice or how you satisfy yourself that you've got the eye open not only for the snake that's right at your ankle but what's over the hill? I know it's a hard question to get at, but I would just like your personal views of whether or not these critics are telling us something.

THE WITNESS: On a daily basis when I have a chance, I've got such great team, we sort of gather together at the end of the day over a cup of coffee to talk about things; and we discuss these types of things. I think if the critics will look at us, you know, one thing is we are tactical because we're trying to support the programs and get their mission accomplished. So we focus on that, and I agree we're taking care of business today. At the same time I think people might fail to recognize that we have set up at NASA headquarters and we have people on teams right now, as we speak, working on future spacecraft, on modifications. You know, Mike Kostelnik has a very energized activity going on now, looking at what it would take to have a service life extension for the Shuttle to take care of these things that might be popping up that we didn't know about, safety modifications and reliability modifications we might do to this vehicle to keep it going to service the Station, because we realize our predictions are the Station, to be viable, is going to need a machine to carry a lot of stuff up there. And that's what the Shuttle does better than anything else that we've come up with. These are not things that are on the headlines. These are things that working groups are working on and, until we make decisions, they're not going to be in the press or in Aviation Week; but I assure you that there are some very good minds working on this. Some of our best talent from my center are up at headquarters working on these types of activities as we speak. We're working on an architecture for the future. So not only Shuttle people looking at what we can do to our present machine but the next machines to be better. We're also looking at what we'll be doing 20 years from now. So I would hate to think that people are claiming that we're too shortsighted.

As far as safety goes, you know if we had 200 more quality assurance people who could look over the shoulder of everybody, maybe we would be safer. There's that balance. As you know, on sailing ships, if you keep all of the ships in the harbor, you're not going to lose any of them. So you always have that terrible dilemma in a position of authority of how much is enough, are we ready to go. I think we have some very serious people and very well-educated and qualified people making these decisions, and I feel very fortunate to be on this team.

ADM. GEHMAN: Just a few more questions.

GEN. BARRY: Sir, if you could just comment and have an opportunity to talk about the budget. I know you've only been on the job for about a year or so but, you know, how it's transcended in your mind and what's the prospect for the future, particularly in any budget distinctions between the Shuttle and the Space Station that you might be able to comment on.

THE WITNESS: Right before I got here -- see, I can cast this stone because I wasn't here -- it was obvious to many people that our budget at NASA had gotten in disarray and it was very difficult for us to really identify what it cost to do things, to get things done. That was one of the first chores that Sean O'Keefe did when he got in here was to pin us down as an organization and find out exactly what it would take to achieve things that we said we were going to take and what it was going to cost to do those things and then to say do we have the budget to do those things. So we had to cut some things out because we really didn't have the money to pay for some things that we said we were going to do. That's just flat not good business. You can't do that at your household, and you can't do it at NASA either.

So he brought budgetary discipline to NASA. He came under a lot of criticism by people because he said I don't need any more money right now, I just want to see how I can spend the money I have. I think we have come through that and I think if you look at the President's budget submission, he actually modified it to ensure that the Shuttle and the Station both would have the adequate funding over the next couple of years to accomplish the missions that we've set out for them over the next several years.

Now, I think as we go forward, we are going to be in a lot better posture to predict proper budgetary accounts towards these things so that we will have credibility when we say we need this much for this and this much for that. I think we are in good shape because he adjusted. We were concerned over the next couple of years that we were really going to have adequate funding for both Shuttle and Station operations that we had projected; and he adjusted, within NASA, funds from other programs to ensure we could do that. So I think we are in good shape.

MR. TETRAULT: I have a continuing question on the budget, just to be sure I understand it. It was my understanding that the budget or at least the budget for the Shuttle and the Space Station are on a project basis with the engineers or Johnson engineers. So there must be some transfer of funds obviously that goes back and forth between the projects and the center. So that's my first question.

My second question is that it's also my understanding NASA is going to a full costing basis, which they haven't done before. I would like your ideas on what kind of a difference that's going to make in terms of how you operate.

THE WITNESS: The first question first. We have an institution with a certain number of engineers and we forecast a certain number of them will be working for the different programs and their budgets pay for their services for those people. However, there is a pool of talent in the different disciplines we have that is funded by institutional money. So I actually have an institutional budget that is separate from the program funding so that I can maintain certain core capabilities that would stand the give-and-take and the ups and downs of utilization or not. So that's sort of my buffer to ensure that I can maintain a certain skill level, whether or not the program needs them today or not, when they're going to need them tomorrow. So it's that kind of give-and-take that goes on. I don't know if I can be more specific than that.

However, a lot of budget decisions are made on what you did last year and the year before and what you project. So that's the type of -- you know, we match our full-time equivalents, our civil servants. Then, of course, the program, the contractors have to do the same thing with their own businesses, what's going to be needed by the programs. Then we have to bargain with them over what we need to do to do the task that's given us. Then they give us the money that's for that. Does that help on that at all?

MR. TETRAULT: Yes. It confirms what I thought.

THE WITNESS: On the other aspect, full cost accounting, that's going to be a great new adventure for Johnson Space Center and for all of NASA; and we are going to roll into that into our next budget year. I, frankly, am learning about it as quickly as I can, and I would hate to try to tell you what I know about it right now because I'm very ignorant. So I'm afraid I can't really answer that question for you.

ADM. GEHMAN: Thank you, General Howell. I've got one last question, and then we'll let you get on to your travel. As you have indicated, over the years a great majority of the work that's done on the space flight programs is now done by contractors, most of it under a great omnibus contract with this USA Alliance, this SFOC contract. What's the NASA mechanism for determining contractors' performance? Is there a board or a committee? Who decides whether they get bonuses or penalties and things like that?

THE WITNESS: Each of the contracts has a contract official who brings in and manages in the programs themselves, grade them on their performance. Of course, each contract is a little different, whether they get fee or whether it's fixed price or what have you. Each contract has a government official, usually with the program or project that they're contracted to, who actually grades them on their performance and determines their fee. Now, the fee determining goes up to the next level for approval. So we are involved in that because we have people on my staff, senior people who are actually reviewing officials to approve those determinations.

ADM. GEHMAN: So what you're telling me, it's really a series of smaller contracts?

THE WITNESS: Yes.

ADM. GEHMAN: It's a bunch of little contracts in each project; and does the center have a contract also, a support contract?

THE WITNESS: Yes, we do. We have several contractor people who do work for us on the center and we also have fee-determining officials and they are always reviewed. Of course, we have to get our headquarters to put a stamp of approval. So it's always the higher headquarters reviews things to make sure we made the right decision; and these folks, if they are upset about it, they can protest and have it reviewed.

ADM. GEHMAN: Thank you very much, General Howell. On behalf of the Board, we would like to thank you for rearranging your travel schedule to be here today. We would also like to thank you for the wonderful support that JSC has provided to us during the 2 1/2 or 3 weeks that we've been here in Houston. It has been wonderful and it continues. The Board is getting larger and we're digging deeper into your back yard and we appreciate your cooperation and the energy level with which all of your folks have supported us. I want to go on record in saying that. Thank you very much, and you are excused.

THE WITNESS: Thank you. We are at your service.

ADM. GEHMAN: Thank you very much.

Mr. Dittemore, if he's here, we're ready for him. If anybody needs a short, two-minute break, we will get started. I'm watching my clock.

(Recess taken)

ADM. GEHMAN: Our second witness this morning is Mr. Ron Dittemore, the Space Shuttle project manager. We'll follow the same procedure we just did.

Ron, thank you very much for coming over here and helping us through this. I'll start off by asking you to affirm your intention to tell the truth here, which I don't think is in question. I'll ask the first question, and then we'll turn it open to the panel here to continue.

I understand that for television purposes we need to stop at 11:29 or something like that in order that the NASA television network can do something else and then we'll resume -- I mean, 11:45, I think, and then resume again at 12:30.

Mr. Dittemore, before we begin, let me ask you to affirm that the information you will provide to this Board at this hearing will be accurate and complete, to the best of your current knowledge and ability?

THE WITNESS: I so affirm.

RON DITTEMORE, having been first duly affirmed, testified as follows:

ADM. GEHMAN: I'll start off with the first question. Would you describe for the Board the lines of authority -- as we in the military call it, the chain of command -- but the line of authority, starting with Mr. O'Keefe and then down to yourself and then perhaps one or two below you.

THE WITNESS: It's almost easier --

ADM. GEHMAN: Excuse me, I apologize. We invite you to make an opening statement, if you would like to.

THE WITNESS: I'm okay to do it whichever way you would like.

ADM. GEHMAN: It's my procedural error. You were invited to make an opening statement. Then I'll ask my question.

THE WITNESS: Since you had invited me, I had prepared one.

ADM. GEHMAN: We would like to hear it.

THE WITNESS: First of all, let me say to Admiral Gehman and the Board that I am very pleased to be here and to discuss the Shuttle Program management topics that you informed me of. I mentioned to you privately but I'll do so again in public that I pledge our unwavering support to you and the Board through the conduct of this investigation.

As the manager of the Space Station Program, I direct all activities associated with the program, including the overall program and project management. That includes integration and operations, directing and controlling schedule, planning and execution of design, development, tests, production, and operations. I am responsible and accountable for program safety, technical and operational performance, schedule, and costs. I report directly to Mr. Michael Kostelnik, the Deputy Associate Administrator for International Space Station and Space Station Programs, located at NASA headquarters in Washington, D.C. I meet regularly with Mr. Kostelnik, reporting daily to his support staff and apprizing them of topics of interest, issues, and concerns and general program status.

As you know, the Space Station Program office is located at the Johnson Space Center in Houston, Texas, where most of the program offices and staff reside. Additional program management reporting directly to me are located at the Marshall Space Flight Center in Huntsville, Alabama, and at the Kennedy Space Center in Florida.

I manage the Space Station Program through a combination of direct reports and matrix support at each of the human space flight centers, encompassing all the hardware and operational elements, including mission operations, flight crew operations and ground processes.

In the performance of these duties, I am strongly supported by the field center institutional management and support organizations. The relationship between the program and the field centers -- the field centers being the Johnson Space Center, the Marshall Space Flight Center, Kennedy Space Center, and the Stennis facility -- is outstanding, with exceptional human, physical, financial, technical, and other supporting resources provided as required to meet the highest expectations of safety and mission success.

I work closely with each of the human space flight Center Directors, Mr. Howell, Mr. Stephenson, Mr. Bridges, and Mr. Parsons, and their support organizations to accomplish the goals and objectives of the Space Station Program. I'm extremely appreciative of the work of the Columbia Accident Investigation Board and again commit to you our complete cooperation and all the resources at my disposal to aid you in your investigation.

ADM. GEHMAN: Thank you. You've already answered half of my question by describing the chain of command above you. Would you mention something about the direct reports under you, in particular if any of them are not located at JSC.

THE WITNESS: Let me go back and talk about those above me, just to make sure we're clear. I report directly to Mr. Kostelnik, who reports to Mr. Readdy, Office of Space Flight. Mr. Readdy reports directly to Mr. O'Keefe.

Starting at my level and working down, I have a management organization that is both direct reports and matrix support. Direct reports would include the vehicle engineering office, for example. That would be Mr. Ralph Roe. He would be accountable for the Orbiter itself as a vehicle, the software associated with the vehicle, the flight crew equipment, and the remote manipulator system, the arm that's physically located on the vehicle.

I have a manager for program integration, Ms. Linda Ham. She is accountable for basically the integration of the flight products, the flight preparation, the activities associated with the mission control center, both preparing for flight, flight design, cargo engineering and integration within the program. She also is accountable for the conduct of the flight during a particular mission.

Another primary direct report is the manager for launch integration, located at the Kennedy Space Center. This particular individual is accountable to me for the processing activities that are conducted in Florida. I delegate to that individual what we call a noon Board chairmanship where he takes care of the day-to-day processing paperwork that needs to come to the program for approval to continue processing, whether it be additional work or testing or any sort of processing.

The person at Marshall that is a direct report to me is Mr. Alex McCool. He is the director of the Marshall projects office, and I hold Mr. McCool accountable to have oversight into the Marshall propulsion projects. That would be the managers for the External Tank and the solid rocket motor, the Solid Rocket Booster, and the Space Shuttle management.

ADM. GEHMAN: One last question from me before I pass it to the Board -- that's clear. Thank you very much. Does the money essentially follow the same line? That is, when you decide next year's budget or your budget request or however you do it, that request comes from your direct reports and your estimate of what you're going to need for the next year in the way of matrix support and then it goes up to Mr. Kostelnik to Mr. Readdy?

THE WITNESS: Yes. It's even broader than what I just mentioned because I just mentioned just a small part of the program. We'd go on and on for a little bit more if we went to every one of the project elements that is a matrix support to me. For instance, the Mission Operations Directorate that Mr. Howell talked about is a direct report to the Center Directorate but a matrix support to me. When I make a call for a budget request, then the Mission Operations Directorate would come forward to the program and submit to me their budget request for the upcoming year. The Flight Crew Operations Directorate, even though a direct report to the center, would come into the program with their budget request. And that occurs throughout the program, whether it's at the Johnson Space Center or the Marshall or Kennedy space centers.

I have a business manager that reports directly to me. Once we receive guidance from headquarters on the particulars associated with the budget, we pass that guidance down to each one of the projects and program elements, whether they're direct report or a matrix support. Then we conduct a series of reviews, intensive in nature, that goes right down to the nitty-gritty, if you want to think about it that way, of their budget requirements for the year. And we develop a budget request that, once I'm satisfied that we have sufficient data to justify that request, then I would take that report to Mr. Kostelnik.

ADM. GEHMAN: Then the ISS program manager's doing the same thing?

THE WITNESS: Mirror image. In fact, the Station Program and my program will get together because there are some elements in our system where we share resources. They're very common in their function, and we would get together and make sure that we have the right split in appropriations.

ADM. GEHMAN: But the first time you meet a budget request then from the Center Director is when Mr. Kostelnik goes to Mr. Readdy?

THE WITNESS: I should tell you that even as I go forward to Mr. Kostelnik, one of the features that we like to do in the program is talk to the Center Directors and their staff before we go forward. We don't want to catch them in any surprise with the position the program's going to take. If we do have a position that the center feels strongly either pro or con, we want them to have the opportunity to talk to us before we go to headquarters. So I would utilize Mr. Hale in Florida to talk to Mr. Bridges; Mr. McCool at Marshall to talk to Mr. Stephenson; and we would transfer the information that we are considering to the Center directors and allow them any reclam of activities or any positions that we are taking. We at least acknowledge the differences before we go to headquarters.

ADM. GEHMAN: Thank you very much.

GEN. BARRY: Mr. Dittemore, thanks very much for coming before the Board. I'd like to afford you an opportunity to comment on the number of changes that have occurred, particularly big decisional changes on management and responsibility. We could probably spend hours talking about this on an individual basis, but if you can give a sense to the Board about your background and decision-making made in a number of areas. Let me just cite them, if I may. One was the decision to move the OMM from Palmdale to KSC, almost within the same time frame was a decision to move contract support from Huntington Beach to JSC, then as you have commented a number of times publicly but also have mentioned about the issue of contractors insofar as oversight to insight. And I cited a report with General Howell about since 1993, in your report that you put out about the concept of privatization, where it said: "The NASA Space Station Program civil service work force has been reduced nearly 50 percent, resulting in significant loss of skills and experience. The NASA skill base continues to erode as more functions transition to the Space Flight Operations Contract." I also would like to caveat that in our trips and our visits to different Centers, we've been very impressed with a lot of the professionalism, specifically with contract and contract support. There is some concern by critics, however, as we look into this that maybe that was too much all at one time to go on. So if you could give a sense to the Board of the rationale of some of those decision-making processes.

THE WITNESS: There are three major topics in there that I certainly understood. One was the Orbiter maintenance modification and the decision going from Palmdale to Florida. The other one was the transition of engineering from Huntington Beach to Texas and Florida. The third one would be in this privatization report.

Can you stop me when I start talking too long on these? 'Cause these are fairly meaty subjects and take a little bit of understanding as far as the background is concerned.

Let me just address the Orbiter maintenance modification first. It's not a new subject as far as us discussing where is the right location for that modification to take place. In the Nineties, I'm sure we did at least two or three studies. I'm familiar with a couple of studies that were internal to NASA in the '97 time frame, and certainly there was a study that was completed by the Inspector General's Office in 1998. All these studies were trying to understand where was the most cost-effective location for performing modifications or structural inspections on the Orbiter, what was the best location for the long term as far as the technical ability to maintain and sustain the Orbiter. Those types of questions, not to mention where was the best place as far as gaining the efficiencies that we were looking for in the future.

The study that was done in the late Nineties indicated that it was flight-rate dependent. If we had a flight rate that was greater than six flights per year and we desired to perform the modifications in Florida, we didn't have the necessary facilities to do that. The flight rate was going to be high enough that we needed the three Orbiter processing facilities that were in existence to just keep up with the flight rate and we couldn't afford to dedicate one to a maintenance period. The same report also indicated that if the flight rate were less than or equal to six a year, then Florida would be a viable option if the program so elected to consider that.

The conclusion at the time, because the flight rates were projected to be eight or so, was to leave the facility at Palmdale active and do our modifications there; and we moved and conducted the *Columbia* modifications in the Palmdale facility. The report also noted that if the assumptions changed, as I just rehearsed them at a high level, if they changed, they asked us to re-review the conclusions of the report.

Subsequent to the completion of the *Columbia* modification, we did just that. We went back and looked at the assumptions that were part of the report, saw that the flight rate was now six or less for the foreseeable future. In fact, even as you look at it now, we had flown two years with only four flights a year. We bumped up one year, I think, to six or seven; and we're flattening out to about a five steady state for the future. And we may bump up to six occasionally.

With that flight rate, we judged it was feasible to use the facilities in Florida as they existed today and that we did not have to provide additional facility. That's a significant finding because the cost of adding another facility and the timing of doing so was prohibitive.

We also looked at the work force. The Palmdale, just by nature of having the Palmdale facility out there and its entire function was modification, at the completion of a modification, we would essentially lay off the work force. So its methodology at Palmdale was to hire a large work force, complete the activity, and then lay off the work force for some period of months until it was time to do a subsequent modification, time for another vehicle to go to Palmdale, in which case you start the process all over again.

We were concerned that for the long term that the hiring-and-firing aspect of that facility would lose technical skills, would lose management knowledge, scheduling and planning knowledge, which we believe are extremely important when you're doing any type of overhaul activity on a very complex vehicle like the Orbiter.

We also looked at the Kennedy Space Center and recognized that we had migrated to a point where most of our technical expertise was in Florida. Day in and day out they're working on vehicles and they're scheduling, they're planning. It's just part of their everyday process. So that expertise was very strong in Florida, and it was a very stable work force.

So when we compared the two, Palmdale with the long-term turnover of people and losing some of the skills, that was a concern for us, and the fact that we were hiring and firing, comparing that to Kennedy with a very stable work force that we can maintain day in and day out who had the experience to work on the Orbiter, we opted for the long term for Florida because it looked to us in our judgment that that was the safest place to maintain the vehicle. The people most knowledgeable about the ins and outs of that Orbiter resided in Florida. Technically and from a safety point of view, we believe that's the right direction.

Looking at it from a cost standpoint, which wasn't our highest priority, but looking at it from a cost standpoint, we also believe there are significant synergies by allowing this activity to take place in Florida where they already have a large infrastructure associated with the overhead of operating a facility. So we can share some of that infrastructure with our modification period.

So those synergies effectively lowered the cost of the activity. So not only do we believe we can maintain a very superior technical work force, a stable work force over the long term, but we can provide synergies that will reduce the cost. So I'm safer and I have a reduced cost. It was hard for me to turn that down as an option.

So I recommended heavily that we move from Palmdale to Florida. Just as we have watched it over the last months, looking at the modification period for OV 103, it is coming along just wonderfully. They're on schedule. We don't see any technical issues, and we believe the cost is right in the ballpark of where we had predicted. So we're very happy with the activities so far in Florida.

I should mention it's not the first time that we had done a structural inspection in Florida. We had done one previously in, I believe, the early nineties. So it wasn't the first time.

I'm going to stop there and see if I answered you on that.

GEN. BARRY: That's good.

THE WITNESS: All right. Let me go on to the Huntington Beach transition. I'm going to spend about the same amount of time, if that's okay. The Huntington Beach transition of engineering skill was really a United Space Alliance contractor initiative and that initiative was brought to us at NASA as an initiative to really get the engineering closer to the customer, to get the West Coast and East Coast closer together because there was a tight correlation and a tight lash-up between engineering design center and operating Centers in Houston and in Florida. For the long term, we also believe that the contractor believed that there would be efficiencies that would lower the overall cost.

Remember the lash-up is that United Space Alliance is the prime contractor. The engineering work force, the Boeing engineering work force at Huntington Beach is a subcontract to United Space Alliance. So they have that relationship. So United Space Alliance was bringing to NASA a proposal to not only move the engineering closer to where the action is but also, for the long-term synergy and efficiency of the program, let's move some of those folks into areas where the costs could be decreased over time.

Boeing took on the challenge to consolidate the engineering functions; and I must admit to you that when we first looked at this initiative and talked to both USA and Boeing, we recognized that this transition was our top program concern. We briefed this as a top program concern to our senior management at headquarters and we subsequently briefed the status as we moved into this activity of how we were coming along in the transition at a number of flight readiness reviews, because we wanted all the Center directors and the chairman of the FRR and our senior management to understand the progress we were making and the risks that we saw in the system as far as making this transition from Huntington Beach to Florida and Texas.

In order to mitigate these concerns, we put together a plan that involved a very formal process of planning, identifying critical skills, trying to capture the critical skills that we believed were the most important, training of individuals that were perhaps new to the program, and then a certification process. So that formal process of identification of the skills, training of new hires, and then certifying them is well documented. If you want to see that, I encourage you to look into that, but we put that in place because we were concerned about those that we could not capture and those that we were going to hire and make sure we had the right technical balance remaining to sustain the program.

I would say overall that the transition has been very successful. We have very high confidence in the technical leadership that we were able to capture. We had NASA-USA-Boeing involvement. I met on a regular basis with the chief executive officer of United Space Alliance and also the general manager of Boeing Huntington Beach -- that would be Mr. Russ Turner and Mike Mott -- on a regular basis for some time. At the highest levels, we were reviewing the status of the transition. On a weekly basis all the senior management of Boeing and USA and the affected project offices, being the system integration office and the Orbiter office, met regularly to discuss the very details, by name, by individual, who was coming, who wasn't coming, critical skill, not critical skill, and the level or quality of people that were coming into the system, which was very important to us, especially since in the end we captured 24 percent of the incumbents out of Huntington Beach.

So effectively, if you looked at it on the surface, we had a large number of new people coming into our system. What was very gratifying to us was that the large numbers of new people weren't really new. They were people that were familiar with the Shuttle, familiar with Shuttle systems, had worked in the environment for 10 or 15 years, just happened to move from perhaps one company to another company. So we didn't get 70 percent fresh out of college, by no means. We got a large majority of very experienced individuals coming from different companies that had a lot of Shuttle foundation framework underneath them.

Even then, we still looked at the makeup of different groups of individuals. If we felt that we didn't have a sufficiently high number of experienced people in a particular group, we declared that group to be out of family. What out of family means to us is that it draws in additional expertise to look over their shoulder. An out-of-family group would require NASA and United Space Alliance technical expertise to validate products and to provide necessary oversight to make sure that we did not have any lapse in our technical ability to produce the products or in our analysis in response to any problems.

Some of the examples that were mentioned when General Howell spoke of over the last six months, the flow liner difficulty in the last July, involved the same lash-up of individuals -- some groups extremely well qualified, other groups requiring this out-of-family oversight. We found that this system worked well. The oversight provided just the necessary comfort that we needed and the skills, and we brought together the NASA expertise that still is available to us and the USA expertise to solve these very complex issues.

So overall the Huntington Beach transition has been a large success. It started out as a top concern, and it was organized and implemented in a way that managed the risk and resulted in a very strong work force. And I can't overstate the fact that it was a difficult activity all in all, moving a group of individuals, capturing only 24 percent, keeping your flight rate stable, and meeting the challenges of the problems that you face day to day. Just an excellent job by the management of NASA, USA, and Boeing to ensure a successful transition.

GEN. BARRY: Mr. Chairman, if I might cede the remaining part. We'll get to the privatization.

THE WITNESS: The privatization is going to go just like I just mentioned, that depth, if that's okay with the Board.

GEN. HESS: Mr. Dittemore, you gave us in your opening remarks some agree of description of your organization. I'm curious as to the SR&QA functions that are direct reports to you, the size of the organization, the scope of their responsibility.

THE WITNESS: I concentrate on safety and just that organizational responsibility all the time because it is our life blood. It is what keeps us safe. I need to give some background here also because to understand safety is more than just the word and just a high-level skimming the surface of what safety means.

You need to understand the relationships between in-line safety and independent assessment. It is a responsibility. For us in my program, the primary responsibility for safety is the in-line organizations. That is the design center reporting to a prime that reports to a project office that reports to a program office that reports to a flight readiness review. There is a primary path of accountability and responsibility, if you understand what I'm talking about. The design center is absolutely responsible for safety. The prime is responsible for safety. The project office is. I am. And we bring that to the flight readiness review.

It is very important that that primary path of safety is maintained in a robust fashion; and I watch that very, very carefully. There are ingredients of safety that are absolutely necessary, and these relate to the work force and relate to the skills that are important for us to consider. We have a term that we call checks and balances and healthy tension.

Checks and balances are making sure we have the right skills in the system, a safety officer that has the right background so that they can ask appropriate questions to challenge assumptions, technical results, et cetera. You have to have the right check and balance in the system.

Healthy tension is the way you set up an organization. There should be a healthy tension between an engineering design center and an operations center. The operator wants to use the hardware. The design center wants it used in a certain way. There should be a good healthy tension between the two of them to ask good questions, provide good technical answers; and if you set up your organization right, you have a very robust process and an ability to not let something slip through the cracks. All this is part of the primary path of safety. Appropriate checks and balances. Good, healthy tension.

Now, there's a second path that I think is also very, very important. It's a separate, independent organization. When you talk about safety, quality organizations, S&MA organizations that are outside of the program, I look at these organizations as being my secondary path of safety.

They watch, participate, and are involved in the activities of the direct path; but it's not the same people. It's a different contractor. It's a different set of NASA. So they are providing to me a separate, secondary, independent check. That's very important to me that I have someone that's looking over their shoulders and may not agree with the way the process has occurred or the way the technical answer has been achieved. I don't want the basis of both the primary and secondary path to be the same source. If it's the same source, I don't have value-added independent assessment.

So that's why I'm careful to tell you today that it's very important for me to have a primary path and then a secondary path that is an independent source. Different set of folks. Not the same contractor at the bottom. I don't want the same source. So when I talk about value-added independent assessment, that's what I mean. I mean a separate, independent assessment outside the program, outside the direct line. Then when they come to me -- and they do come to me, they sit on my boards and I ask them for their opinion -- I'm getting their opinion that is separate and distinct from my design primary path chain of command.

So when you talk about the organization for SR&QA, it's very important that we understand the primary path of safety that is direct accountability to me and a secondary path that's value-added independent assessment that is involved in our program. And we take both these paths forward to the flight readiness review.

GEN. HESS: Thank you.

GEN. BARRY: Next question won't be quite as cumbersome, but it may be. Let me ask the question on aging spacecraft in an R&D environment. We're entering an area here we've never really been in. Our space program, as glorious and as successful as it has been, really has been with vehicles that we've been able to use and then basically not use again. The Space Shuttle, of course, is the first one that now is, since the *Columbia* flew in 1981, over 20 years. Can you comment on the approach and significance of this new era that we're entering, if you could describe it that way? The Board's described it as aging spacecraft in an R&D environment, not an operational environment but an R&D environment.

THE WITNESS: I think we're in a mixture of R&D and operations. We like to say that we're operating the fleet of Shuttles. In a sense we are, because we have a process that turns the crank and we're able to design missions, load payloads into a cargo bay, conduct missions in an operating sense with crew members who are trained, flight controllers who monitor people in the ground processing arena who process. In that sense we can call that operations because it is repeatable and it's fairly structured and its function is well known.

The R&D side of this is that we're flying vehicles -- we're blazing a new trail because we're flying vehicles that are, I would say, getting more experienced. They're getting a number of flights on them, and they're being reused. Hardware is being subjected over and over again to the similar environments. So you have to be very careful to understand whether or not there are effects from reusing these vehicles -- back to materials, back to structure, back to subsystems.

To the best of our ability, we try to predict the interval for inspection, the interval for subsystem testing; and there's been pressure in the past and even today to decrease the amount of inspections that you do on these vehicles. It's very difficult for a design center who has the accountability to maintain and sustain these vehicles to want to back off on a level of inspection just so you can get more into an operational environment and turn them around faster.

So we have resisted and the design centers have resisted reducing the number of requirements from a processing point of view. The processors have to complete the requirements of the design center. So we have resisted backing off on the requirements the design center wants to pursue to maintain their insight into what you call the aging systems.

Our challenge is, as we effectively tear the vehicle apart in these maintenance periods, our challenge is to identify clues that either substantiate that we have the right amount of time between inspections or to give us a clue that says there's something happening here that we need to change or we need greater analysis or we need greater tests. And based on the experience over the last 20 years and looking at the vehicles and analyzing the results of tests and analysis, we're getting better at predicting that time interval. It was fairly arbitrary at the beginning. We said we needed to do a structural inspection, overall, every eight flights and three years. Structurally now we're looking at the structure and we're seeing that, because of the quality of the hardware from inspection to inspection, that perhaps I can even increase the interval on structural inspection in certain areas of the vehicle. There may be other areas of the vehicle where, because they're more exposed to the elements, I want to see them more often. So we look at all those different aspects both from a subsystem and structures. I just give you that by way of background to help you understand those are the areas that we really are trying to understand. And perhaps there's more that we do need to understand by way of tests and analysis as we continue to fly for another 20 years.

ADM. GEHMAN: Thank you, Mr. Dittemore.

Panel, our time is up. We're being governed partially by the broadcast of this hearing. We will continue with Mr. Dittemore at 12:30. We all have a couple more questions. So if you're still available, we'll resume at 12:30.

(Luncheon recess)

ADM. GEHMAN: All right. Members of the Board, we're back in session. Mr. Dittemore, if you're ready, we'll just resume right where we left off. I believe General Deal gets to go first since he wasn't here this morning.

GEN. DEAL: Mr. Dittemore, there has been some talk in the media and amongst people about potential trade-offs and budgetary trade-offs in the ISS program and the STS program. I'd appreciate your comment on that and giving us your views.

THE WITNESS: I think the first thing to note is that both the Shuttle and the Station are very complementary in that we work very closely together. I have a very strong relationship with Mr. Bill Gerstenmaier, who is the manager of the International Space Station Program; and that close relationship is necessary because, as I mentioned earlier, we share certain resources. The mission operations area, the flight crew operations, and even the space walk functions are common to both programs. We get together and make sure that when it comes to us spending our own resources individually as programs, we make sure that we have the right percentage split. Whether it's 50/50, 60/40, 20/80, we look at that and make sure we have the right appropriations between the two programs.

We also agree on who takes the lead. If the percentage of a particular activity is 80/20 in favor of Station, in many cases Bill and I will get together and I'll say, "Bill, why don't you take the lead? Because I'm basically a customer, you have more of an owner relationship here, we'll let you be the advocate for the budget and we'll just tag along." The opposite is also true. So there is a very strong relationship between the two of us and as we work those types of subjects together, we bring those forward to Mr. Kostelnik either to arbitrate where Bill and I can't get together -- which is very seldom -- or to bring Mr. Kostelnik up to speed on where we believe the right split is so that he can carry it forward in the budget process.

To date, there's no real sharing of budgets. There's no transfer from Shuttle to Station or Station to Shuttle. We both go forward with our requests for the given operating plan and the cycle that we're asked to present; and we compare notes to make sure that we are complementary, as I mentioned. So at this point, it seems to be working very well.

ADM. GEHMAN: Mr. Dittemore, one of the thing we forgot to do in the introduction here, would you tell us how long you've been the project manager, when you came into office.

THE WITNESS: Just six weeks ago, I would just have said generally, but now in the last six weeks it has made it very acute and I can tell you to the date almost. I was made program manager in April 1999, almost four years.

ADM. GEHMAN: Thank you very much.

DR. HALLOCK: I have two very different questions. I've been looking at the responsibilities of people and so on. I was just curious, as you come to a countdown, what are the responsibilities that you have at that time, your role and/or decisions that you get involved in as you approach the countdown.

THE WITNESS: I think it's important to recognize that once we get to a time frame that is launch minus two days and inward toward the count, the management of the activities, the launch countdown itself is really handed over to the launch teams, the operating teams, and the management of the program is delegated from an operational point of view to the manager for launch integration. As I mentioned earlier, he is my direct report to me, located in Florida. That particular individual chairs the Mission Management Team, starting from L minus two days down to the count.

As we get into the countdown and we're all in the Launch Control Center, then the final go for launch is given by the Mission Management Team and specifically by the chairman of the Mission Management Team. That would be the manager for launch integration. He will provide to the launch director his approval or her approval to go ahead and proceed with the count. That is done just before we come out of the hold at T minus 9 minutes.

My relationship to the manager for launch integration during this time frame, because we're both located in the Launch Control Center but not in the same general area, we are tied together via a phone. If there's anything unusual, we communicate with each other; and if there's nothing going on, we communicate. So both positive and negative reporting, depending on what time frame we are in the launch count. And we're co-located sufficiently close to each other that if there's any reason I need to get up and walk over and talk face-to-face, that's easily done. So the relationship is a very tight communication loop.

Where I am located in the Launch Control Center is in the senior management area, and I'm sitting right next to Mr. Bill Readdy, the Associate Administrator for space flight. We have the Center Directors in this general area and if there's anything that would come up that involves the agency, if there's anything beyond the operations team involvement that requires a senior management discussion, we're all there together and can feed that information back to the manager for launch integration who's chairing the Mission Management Team, and that would get fed into the launch team or the mission team, ops team, whichever the case may be.

DR. HALLOCK: Thank you.

ADM. GEHMAN:. Let me follow up on that. I want to get back to that exact relationship you just talked about, the Mission Management Team, the launch director, and yourself. You're all three sitting there and all three of you have some authority and some responsibility. Maybe you could go a little bit deeper into that. My understanding in layman's terms is that as long as the launch is going in accordance with the flight rules, if you can call them flight rules -- you probably call them launch rules -- then the launch director does his things and then if there's anything that's anomalous or different, then the Mission Management Team has to step in. Is that right?

THE WITNESS: You said it very well. The launch director can make decisions within the framework provided to him within the launch rules, launch commit criteria. Anything outside the authority given to him within that launch commit criteria must go to the Mission Management Team.

ADM. GEHMAN: But you're sitting there listening to all this. Can you overrule the Mission Management Team chief, or what is your role?

THE WITNESS: The chairman of the Mission Management Team reports to me. I have assigned that individual to chair the Mission Management Team; and so our relationship is that if there's something that I feel uncomfortable with, I can certainly stop the count at any time. And I'm paying close enough attention to it so that I have that relationship and knowledge that if for whatever reason I don't feel the technical discussion has been appropriate or the work, that's there's open work that I think needs to be closed, then I have the authority to step right in and stop the count.

ADM. GEHMAN: Thank you very much, Mr. Dittemore.

DR. HALLOCK: My other question was: Over the last couple of weeks I've had the opportunity to talk to a lot of the people here at Johnson, a lot of the people that are working for you -- by the way, they have been very, very helpful. I have seen that they are just as determined to get to the bottom of everything that's going on as I think we are right here at this point. I'm just curious what you perceive as sort of the morale, if you will, of everybody at this time.

THE WITNESS: Well, I think the morale is generally pretty good considering the conditions we're operating under. It's been six weeks since we had an event that changed all our lives, and every day that goes by gets better as far as the work force is concerned. As I mentioned to some folks earlier, the best therapy that we can do is to be extremely engaged in solving this particular problem; and everybody wants to be engaged in this effort, without exception. Senior management to the technician wants to be involved in this effort. Not all of them need to be, but their waking moments, their thrust, their reason for coming to work is to help you as an investigation Board solve the problems so that they can implement whatever needs to be done to get us back to flying.

I would say the morale is good in that sense. There is an even more increased determination and a greater commitment to look very closely at the system, and they are determined to identify if there's any weakness. And it's broader than just what may be determined as the root cause. They're going to look to see if there's something else in the system that may have existed for many years but now that -- they will come back and make a recommendation to me that says they'd like to make some improvements. Even though it may have nothing to do with the root cause, I suspect that they're going to be very interested in making some recommendations that would improve the overall configuration of the system. So they're engaged, definitely; and I hope you get the sense that there is an absolute 100 percent commitment on their part to support you in every activity.

DR. HALLOCK: I have been seeing that.

DR. WIDNALL: You know, following the *Challenger* accident, watching NASA as it moves forward in its organizational development, many observers have sort of commented that NASA is making a transition from an agency in which it's important to prove that it's safe to fly to one that needs to prove that it's not safe to fly. I don't know whether that's clear; but in other words, if you make a launch decision, rather than proving it's safe to do it, somebody has to come forward and prove that it's not safe in order to have it basically stopped.

A kind of corollary of that is the question of how do you take the bubbling and turmoil level of concern that clearly comes from your engineering workforce -- and we saw that -- and translate that into actionable programs, I guess, to resolve some of the uncertainty that's being expressed.

THE WITNESS: Well, my general observation is that we as a program are very zealous about making sure we are safe to fly, and I think our track record will defend that. Let me give you some examples.

We've had a number of different cases where we probably could have continued a launch and flown but decided not to because we didn't understand the events that transpired, we didn't feel comfortable with understanding the background, and we didn't think we had sufficient discussion to convince ourselves that we were safe to fly. And I think that is the right side of the equation to be on. We have scrubbed, we have taken timeouts, we have delayed 24, 48, whatever it takes for us to get comfortable; and I think we have a strong track record that will substantiate what I'm saying.

Last summer a particular technician identifies a very, very small, what is perceived to be inconsequential indicator in a flow line. That stopped the process. We didn't go into it and say, "Someone prove that we're unsafe." We went into it and said, "We have a crack in the flow liner. We're going to stop processing on all vehicles. We're going to inspect all vehicles and we're going to determine how big the problem is because unless we understand it, then how can we say we're safe to fly?" That's an example, and that delayed us on the order of six weeks and required a significant amount of effort across the program, both from the public side of the government, from private industry, from academia, to pull this together in a very concentrated effort.

Later on in the year we had other indications. As we were working on the *Discovery* OV 103 in its major modification period, we identified small indications on what we call the BSTRA or ball strut tie rod assembly. All by itself it looked very, very small and inconsequential; and if I think there was an attitude to prove it's unsafe, it would have just been in the system, worked its way through paper. Not in this case. It's immediately notified to upper management that they believe there's something here that we should look at.

Again we stopped processing and we go prove to ourselves that we're safe to fly. Again, significant amount of time and resources to prove to ourselves that we are able to continue with flight preparation.

So I don't know where the comments come from, but I think the track record is there to defend ourselves ably that we are a program, an institution, and a culture that today proves that we're safe to fly rather than any other method.

DR. WIDNALL: The second part of this really was this question of how do you take these bubbling turmoil and comments of concerns that you verbalized and really translate those into action?

THE WITNESS: I think you have to cultivate a culture that allows everyone the opportunity to raise their hand and say they have a concern, to have the work force feel that no matter what position they are at in their organization that they can bring to management's attention an issue that they feel is a significant one that management ought to address from a safety-of-flight point of view. The culture and the process have to be there, and I believe that is true today in our culture and our process.

However, I must also state that we also cultivate a culture of second-guessing, challenge, checks and balances, as I mentioned earlier, and healthy tension. We want the people in our system to challenge the assumptions. We want them to challenge the results of technical analysis or tests, and we do not feel threatened at all by that challenge. In fact, we believe it is healthy for us; and so when I hear about people in the system that are challenging and talking about particular analysis, that's what I want them to do. I want that to be part of our culture; but I also want them, if they believe that they have an issue, I want them to raise their hand and bring it forward to management. If they don't, given that I believe the culture is there and established for them to do so, then I must conclude that they do not believe strongly enough to bring it to management's attention, that it is something that they are in this challenging stages and they are doing a what-if type of discussion, which we also want them to do, to cover any event. So all I can say is that we cultivate that culture both on openness -- anybody can come and walk in my office and say they've got a problem. Anybody can walk in any of our management, senior management's offices and say they have a problem, and we will listen to them. I'd be very disappointed if I found it to be otherwise.

ADM. GEHMAN: Thank you very much.

MR. TETRAULT: One of the unfortunate results of this accident is that there will be future delays of launches and that, as I understand, will, in fact, result in some cutbacks within the program. My question to you is: Are you concerned about the loss of capability particularly in the technician ranks as some of those cutbacks occur?

THE WITNESS: Well, at this stage in our investigation in supporting you, we have not discussed any cutbacks in our work force. Not only have we not discussed it, we have not entertained it; and our work force is a part of our system. They're vibrant, they are creative, and we're going to need every one of the members of our work force to get us through this period of time. There are a number of activities that will be required to be completed, independent of the investigation, to get us back to a return-to-flight posture. I think the work force needs to understand that they are a part of this, they are a part of the return to flight, even though they might not be totally involved with the investigation and support of the Board.

So just to reinforce this message to the work force, we have not discussed or contemplated at this point any slowdown, any layoffs, or any reduction in our work force because we are aggressively trying to determine exactly what areas of emphasis, in addition to your investigation, we need to concentrate on to involve the work force in.

ADM. GEHMAN: Thank you very much.

GEN. BARRY: I'd like to afford you an opportunity to comment on this privatization report, if I may.

THE WITNESS: How much time do we have left?

GEN. BARRY: The point again, if I can restate it, is that since 1993 the Space Station Program's civil service workforce, it states in the report, was reduced nearly 50 percent, resulting in a lot of loss of skills and experience. In the same report it said in the previous five years, which would have been '96 to 2001, your program had approached contract consolidations; and there's a term in there, "oversight to insight," which it would be helpful to understand what that is, if you can spend some time on that one. During this transition which has gone on in this period of time, it states here that NASA's skills and experience will result in serious erosion of checks and balances critical to safety and mission success. The final statement I'll just read here, if I may, is: "Continued consolidation utilizing the existing approach results in a serious threat to safety and mission success. A different approach is required." So can you comment? I just want to afford you an opportunity to give us your insight on this report and what was meant by that.

THE WITNESS: I think you need to go back in time because the environment at the time that report was written is completely different than the environment that it is today. In the summer of 1999 -- and you've got to help me, General Barry, on my dates. The report was authored in September of --

GEN. BARRY: 2001.

THE WITNESS: Okay. So it's the summer of 2001. Thank you. There was an excitement in the air about talking about privatization, and what's interesting about that is there was no general agreement on what privatization really meant. So even by me putting out a report that says concept of privatization, it's debatable what that means. It was debatable in that summer what privatization meant. So you have to almost put yourself in that type of environment where there was a large excitement about thinking about where are we going in the future, were we going to ask the government to step farther away from operations.

They had made a commitment in '95, '96, when we went to a space flight operations contract and had transitioned government functions over to the private sector. The government had already made a commitment to step back in many areas. In the summer of 2001, there was again this excitement going on in the system at the highest levels in our agency concerning should we do more; and in that environment, I was asked to develop some concepts.

The other important thing I think we need to understand is, from my point of view as a manager of the Shuttle Program, I look over the entire assets of the program -- work force, facilities, skills, everything associated with the program -- and I, on a regular basis, along with my team, try to understand are we safe to operate today, are we going to be safe to operate a year from now, are we safe to operate five years from now. And they may have different answers depending on where you are with your work force and where you're going with your tactical and strategic activities.

In the summer of 2001, we had some basic program concerns; and the concern is that we had had a large decrease in our civil servant population supporting the program over the previous decade. I think we mentioned in the report somewhere between 40 and 50 percent civil servants supporting the program, reduced 40 to 50 percent. That's a significant decrease and a significant loss of experience and knowledge.

Now, some of that, I believe, is justified because we were coming off a heavy emphasis on development and we were turning the gain down on development and moving toward this operational aspect. So we didn't need as many people supporting the program. However, you have to consider and you have to project where you turn the faucet off so these people stop leaving the program. As we looked at it, we were concerned that the faucet had not closed, it was still open, and we were still getting a drain of civil servants over time. In fact, there continues to be and has continued to be a discussion about outsourcing and competitive sourcing and moving more functions from the government to the private sector. Those types of discussions, and knowing that your civil servant staffing to support the program continues to decrease, causes me as a program manager and my management team to have concerns.

As I mentioned earlier when we talked about SR&QA and the in-line primary path of safety and the secondary path of safety, the way you relate skills and experience in this program is to look at the checks and balances that are available to you and a healthy tension that needs to take place. It all goes back to that. If I lose the skills and experience in my program over a period of time, then I am slowly affecting in a negative way the checks and balances that I believe are critical and the healthy tension that must take place to maintain the safety; and if I don't maintain this value-added independent assessment, then I'm weakening my program's ability to fly safely in those three areas. I'm weakening independent assessment because I'm losing skills and knowledge that could be independent; I'm losing the ability to have knowledge to give me a healthy tension; and I'm losing the ability, with the loss of knowledge and experience, to have strong checks and balances.

So if I look into the future, project where I've been into the future, then I have a concern. If you looked at it, another way is I'm in a going-out-of-business mentality and I'm doing it slowly over time so that one year to the next it doesn't look like you're making much effect or having much influence but over three years or five years, if you don't project it carefully, you're going to get to that five-year point and you're going to ask yourself, "What have I done to myself?" And it won't be on my tenure as program manager, it will be the next program manager or the one after that that's going to look back and come find me and ask me what the heck I was doing that allowed this to happen. So I just give you that background because it's necessary to help you understand the concerns that I have about maintaining the right balance of work force to support this program on the government side and on the private sector side.

Along with that, to understand privatization, you have to understand that the role of the government has not changed. We are still accountable. I am still accountable for this program. Even though functions are being transitioned to the private sector, I am still accountable for everything that I had been accountable for five years ago; but I have less resources and perhaps less skill to do the same job. So if we want to still be accountable on the government side, then I have to help senior management understand the level of civil servant experience and skills necessary to operate the program.

That's really the foundation of that report. I wish, in hindsight, I had not called it privatization because it gets all mired up in somebody's definition of privatization, which someone can take a crack at that and we'll spend a year debating it. That was not our intent. Our intent really was to focus on the brain drain, the loss of skills and experience, to get everybody to focus on the checks and balances and the healthy tension and the organization that needs to exist to maintain flight safety. That's the basis of the report.

Now, I have to tell you also that as a team we worked for several months -- and I'm talking about my management team, my senior management in the program, along with some senior managers on the contractor side -- we got together and discussed these concerns that I just related to you. As a management team on the NASA side, our first option and our desirable option is to shore up where we believe the weaknesses are in the civil servant side. In other words, we believe we should be accountable, we accept that accountability, but I need the right resources in the right areas for the long term. I am safe to fly today. No question about that. I'm not talking about today. I'm not talking about next year. I'm talking about the trend and projection of where it's going to be unless we do the smart tactical actions today.

Our first option was to take the necessary action to either reverse direction in some areas that we thought were too weak and had gotten weaker over time and/or shore up areas for the future that we knew were going to be sensitive. We did not believe that to be a credible option because of the environment at the time. That's why I say you have to put yourself in the environment at the time. The environment at the time was not bigger government for the sake of the Shuttle Program, it was try to get lean and mean, try to get efficient, try to reduce and still be safe. So we didn't think that first option was very credible, and we wanted to be realists about this proposition.

So our only other choice as a management team that we believed could be credible was to somehow merge the work force, the best and brightest on the government, the best and brightest in the private sector, and somehow put them together in such a way that it preserved the safety of the program for the long term. We did not say how to do that necessarily. We did not say that had to be all turned over to the private sector. There are a lot of different options that people can talk about. We kind of left it just that way. There are several options if you wanted to address it, if people wanted to address it in the future. We just left it from a program point of view that these are our major issues, these are our concerns. Whatever options that people like to address, these basic factors need to be considered.

So when you talk about that report, it needs to be understood in the vein of the concerns the program had in 2001 and still has today; and it's my responsibility to make sure that my successor, the people that follow me as program manager, either the next one or the one after, I have to take the actions necessary today to let them be able to manage a viable program 2 years, 5 years, 10 years, 15 years in the future. I know most of you have been involved in these types of projections. It's very difficult to project into the future and be right or convince people that you're right. It's just subjective and it's judgment. That's what we were trying to do with that report.

GEN. BARRY: Thank you.

ADM. GEHMAN: I'm going to close out here, Mr. Dittemore, with one or two questions that maybe we can go over quickly. The first one is certificates of flight readiness waivers. For the people in the audience, what I understand were the waivers I'm talking about, they go all the way back to the original production of the Orbiter and every time there's a repair done that's not to spec but it's a certified repair, you can get a waiver for that. So it's very likely that on the Orbiter you may have several hundred waivers outstanding. Am I not correct?

THE WITNESS: Well, I have to be careful myself because there is a language that we need to understand. Because there are standard repairs, there are return-to-print repairs, there are repairs that require a buy-off by engineering, and a waiver is an approved condition where we may not fix a problem or we may accept the condition as is.

ADM. GEHMAN: So my question is: Without going into any particular waivers, who can approve a waiver and what is the mechanism by which that's done? Do they come to the program manager?

THE WITNESS: We have strict criteria on who can and cannot approve a waiver. In most instances a waiver goes to this new board that I talked to you about because most waivers involve the processing of the vehicle, for instance, and the manager for launch integration who chairs that noon board for me on a daily basis, if required, would disposition those waivers, with the noon board membership being all the people in the program involved, the flight through the mission ops, the ground processing, all the engineering disciplines and all the projects, listening to the conversation, deciding whether it has merit or no merit.

There are other waivers that may not be a single flight type of activity. Let me put it in context. You're processing a vehicle and you have a criteria to check out a helium regulator and it has a requirement that says it must be plus or minus 50 psi from a particular level and it comes in at plus 50.5, violates the criteria. People would take that forward and for that half a psi exceedance, is that acceptable or not acceptable? Is it acceptable for one flight, two flights, or five flights? So these are the types of things we would have a discussion on.

There would be others that may be more significant, in which case, as the program manager, I require them to come to my board because they're not a single flight issue or a processing issue. They're more of a long-term agreement, and I must weigh in on those.

ADM. GEHMAN: But, in general, it's either you or your designated representative.

THE WITNESS: That's right. There's clearly defined who can approve waivers, and that has been identified in our documentation.

ADM. GEHMAN: Good. In your experience, is there a process, then, to review the accumulation of waivers?

THE WITNESS: Yes, there is. In fact, I'm trying to think how often this is; but I believe it's quarterly. I go through systematically and look at the changes, the number of waivers or exceptions, as we also have a term, that have happened over a quarter. I look at the changes in our hazards, in our critical items list, and I'm trying to determine whether we have a system that's going out of balance. Do I have too many waivers all of a sudden? I'm trying to look for flags. If I see too many all of a sudden, I'm asking what's going on in the system. So I do that on a quarterly basis. I don't do it any more often than that because the date is so bouncy you can't do it.

ADM. GEHMAN: Thank you. Again referring particularly to the prime contractor, the USA contract, what entity at your level -- and if it's not at your level, you'll just advise us -- but how is the contractor either rewarded or penalized? Who decides if the contractor is rewarded or penalized for anything? I'm not talking about the loss of the Orbiter or crew here. I'm talking about anything -- delays, safety violations, whatever. How is the contractor rewarded or penalized?

THE WITNESS: It's generally done commensurate with the features of a contract. If the contract is an award fee contract or it has award fee features in the contract -- and USA does, that contract does -- then I would convene a performance evaluation board at the proper interval and we as a management team would get together and assess their performance over that time period. Once we have determined their particular performance, then I take that judgment to Mr. Kostelnik, who is the fee determining official. I present it to him. But yes, I do look at that.

ADM. GEHMAN: Thank you. My last question -- I thank you for bearing with us here. My last question again is kind of a process question. You were speaking about budgets and Mr. Kostelnik and Mr. Readdy. At NASA headquarters, is there any kind of a program evaluation office? I know there's a comptroller, I know there's a budget officer, but is there any kind of an office of program appraisal?

THE WITNESS: Well, that certainly doesn't ring a bell in my mind.

ADM. GEHMAN: Well, you would know it if there was one because you would be wrestling with them all the time.

THE WITNESS: Well, I'm wrestling; but that doesn't ring a bell.

ADM. GEHMAN: Thank you very much for your time, for bearing with us with the noon break and for being so forthcoming with your answers. It's been very helpful to the Board. There are matters which we will want to talk with you about some more, and we will ask you to return at some date in the future.

I would like to express on behalf of the Board, not only to you, Mr. Dittemore, but to the whole program, our thanks and our admiration for how hard and how diligently everybody's trying to get to the bottom of this. The energy, the zeal, the professionalism is quite evident to the Board. It's remarked upon frequently by the Board. It's very genuinely felt, and we know that you and your office want to find the cause of this tragedy just as much as we do. So we thank you very much for your cooperation. You're excused.

THE WITNESS: Thank you very much.

ADM. GEHMAN: Okay. We are going to continue on. If anybody needs to step away from the table at any time, please do so. We'll just continue on. I think I'm looking for Mr. Keith Chong.

Mr. Chong, good afternoon.

THE WITNESS: Good afternoon.

ADM. GEHMAN: We're very pleased to have you come join us, and I'm sure we're going to learn a lot. You're going to have to be patient with us. If you use any complicated words, we'll stop you. We are very thankful for you to come here.

I would ask us to start off by a couple of preliminaries. I'll read a statement that says you agree to tell us the truth. If you agree to that statement, just say, "I will." Then we would like for you to give us a short biographical sketch of where you're working right now and what your area of expertise is, if that's okay with you.

Let me just read the statement. Let me ask you to affirm that the information you will provide to this Board at this hearing will be accurate and complete to the best of your current knowledge and belief.

THE WITNESS: Yes, I will.

KEITH CHONG, having been first duly affirmed, testified as follows:

ADM. GEHMAN: Will you please state your full name and where you work and what your area of expertise is.

THE WITNESS: My name is Keith Chong. I'm a senior engineer scientist from Thermal Management Systems Group, Material Process Engineering Department, at Boeing Huntington Beach. Currently I work on Boeing Delta 4, EELV program, International Space Station.

ADM. GEHMAN: And the EELV program is?

THE WITNESS: Evolved expendable launch -- vehicle. Thank you very much.

THE WITNESS: -- and advanced development system program, which includes the space launch initiative, SLI; Orbiter express, and the X-37 vehicle.

Before this, I worked on the Space Shuttle Orbiter main propulsion system and reaction supply and distribution hardware, which includes the 17-inch disconnect, the small cryogenic feed lines that's used on the Orbiter, the feed lines that mate up to the 12-inch flanges of the main propulsion system, and the pre-valves closeout on the outer fuselage of the Orbiter. In addition to that, I served as a member of an External Tank thermal protection system working group sponsored by NASA Marshall Space Flight Center and worked with representatives from Lockheed Martin from Michoud and JSC, NASA Marshall Space Flight Center, and Kennedy Space Flight Center. I'm also as a focal point at Boeing in the Columbia accident foam debris analysis team.

I graduated from the University of Southern California with a Bachelor of Science degree in chemical engineering in 1984. I was asked to be here today to answer any questions you have regarding cryogenic foam insulation.

ADM. GEHMAN: Thank you very much. Just to be sure we understand, you are presently employed by Boeing?

THE WITNESS: I was originally employed by Rockwell International in 1988 and since then, you know, would be part of Boeing.

ADM. GEHMAN: The External Tank is made by Lockheed Martin?

THE WITNESS: That's correct, sir.

ADM. GEHMAN: Just to make sure. In the current vernacular, then, you're a foamologist, according to the press. Is that right?

THE WITNESS: That's the first time I've heard that.

ADM. GEHMAN: That's what the press tells me. On the Delta 4 rocket, the expendable launch vehicle, it also uses foam for insulation, does it not?

THE WITNESS: That is correct, sir.

ADM. GEHMAN: Could you describe what your role in that is and is that an external foam applied blanket similar to the External Tank?

THE WITNESS: Basically, yes, they are applied on the exterior surface of the POFI tank. The way we have done it is basically we have the tank seated horizontally and rotate with the help of a roller and the robot arm which applies the foam that goes along the length of the tank. The foam that we put on there is a urethane modified isocyanate foam. I occasionally use it as calling it a spray-on foam insulation. In short it stands for SOFI.

ADM. GEHMAN: Now, in addition to that, are there any fixtures, appurtenances that have to be covered or insulated by hand?

THE WITNESS: We are working actually, looking at how we can improve the current process we have on the Delta 4 common booster core where occasionally we do perform plug holes on the spray-on foam insulation to basically verify the integrity of the bond of the foam to the substrate. We also perform densities on those foams. Those are the steps that we perform to validate how good the foam is, how well it's made. In addition to that, we at Boeing have performed 100 percent laser shearography inspections. We check for debond on the entire surface of the common booster core.

ADM. GEHMAN: Thank you very much for that introduction.

DR. WIDNALL: I actually did the mission assurance on the Delta 4. I think one question that the Admiral asked you which might not have come through, I think he was asking you whether you have foam covering of some of the protuberances where the solid rockets join the main tank. Are those also covered with foam?

THE WITNESS: Yes, they are.

DR. WIDNALL: So there's a kind of special process?

THE WITNESS: Well, that particular common feed line that hooks up --

DR. WIDNALL: Well, feed lines and structure lines.

THE WITNESS: Those are usually, they are done by pour in place where you basically clamp the mold onto the exterior surface of the feed lines and inject foam in.

ADM. GEHMAN: Thank you.

DR. WIDNALL: And this laser inspection, is that basically like a non-destructive testing technique that would allow you to sense the bond between the foam and the metal surface underneath?

THE WITNESS: That is correct. It's a non-destructive testing.

DR. WIDNALL: Do you also use ultrasound?

THE WITNESS: Not to my knowledge.

DR. HALLOCK: I'm interested in hearing more about the concept of acceptance criteria. What kinds of things do you have to look at when you're dealing with foams like this in the sense of how well it's been put on, i.e., the density of the material that's there or any kind of testing that's done before you say, yes, I am done and that's done the way it's supposed to be done?

THE WITNESS: May I get clarification? When you say acceptance tests, are you referring to the raw material when we receive it or after we apply it on the External Tank?

DR. HALLOCK: After you apply it.

THE WITNESS: In a case like this, what we do is we have a real-time recording of the temperature of the tank, the temperature of the spray booth, the temperature of the component in the hose, and the pressure of the hose while it's being applied onto the CVC tanks. After we applied it, basically we would perform a plug hole test, basically about seven plug holes, one on the leading area where it was sprayed, another one in the middle, and a last one is beneath the robotic arm where the spray gun leads out. We would perform those plug holes on those areas. We also perform two plug holes on the dome of each side. From those we would determine how well the foam performs. That's part of the acceptance tests.

DR. HALLOCK: How about when you put this foam on a rocket like this? Is there an issue about aging? Is there a problem about how long it's still viable after you put it onto the craft?

THE WITNESS: Well, for Delta 4 it's rather a new program, so I don't have the answer about how long the foam would last.

GEN. BARRY: I would like to ask a question about ablative material. Is there any ablative material underneath the foam in the Delta 4?

THE WITNESS: The answer is no, sir. We apply the foam directly on the substrate. There is no ablative material underneath the foam.

GEN. BARRY: In your experience, can you give us any commentary on any value-added ablative material underneath the foam?

THE WITNESS: Unfortunately, no, I don't think I would have the opinion as far as --

GEN. BARRY: Are you familiar with cryopumping and some of the analysis that has been going on there?

THE WITNESS: The cryopumping? During our first flight we didn't have any experience with cryopumping on the entire facility surface of the CVC. However, we did some cryopumping on the BOFI, which stands for Bond On Foam Insulation.

GEN. BARRY: Are we talking about the Delta, or are we talking about the External Tank?

THE WITNESS: That's Delta. I only basically focus on Delta 4.

GEN. BARRY: But your position right now is you said you were on the working group for External Tank along with representatives from Lockheed Martin and Michoud, right? Okay. Are you involved with any of the analysis of the working group efforts as part of the mission response team?

THE WITNESS: No, sir. This ET working group was formed back in 1991 and basically ended in 94. That provided basically an avenue for us to discuss new developments and issues and problems that we run into with foam insulation. My main focus at the time was mainly on pour in place foam insulation.

MR. TETRAULT: Could you tell me whether the Delta program has experienced any loss of foam at launch and, if it hasn't, are you aware of any other programs that use foam which might have experienced that loss of foam?

THE WITNESS: No, I have not. I have not heard as far as what I got regarding foam loss from launch.

MR. TETRAULT: You haven't lost any on the Delta 4?

THE WITNESS: That I'm not sure. I wasn't aware there was any loss of foam.

ADM. GEHMAN: To follow up on that question, the insulating foam that you use, I mean the insulating foam, the design of it and the application of it, it is designed not to come off. You're not assuming that you're going to lose it.

THE WITNESS: That is correct, sir.

ADM. GEHMAN: The expectation is that foam should not shed off.

THE WITNESS: It does in some way I've seen from some of the hot gas tests at Huntington where foam does so-called blade off on the testing.

ADM. GEHMAN: the Delta 4 uses the same fuels as the External Tank?

THE WITNESS: Yes, sir.

ADM. GEHMAN: About the same temperatures?

THE WITNESS: I would say so because the Delta 4 rockets have liquid hydrogen and liquid oxygen. Liquid oxygen in this case is an oxidizer.

ADM. GEHMAN: Do you get them from the same vendor, do you know? Does the foam come from the same source?

THE WITNESS: Yes, sir. Correct.

GEN. DEAL: I'd like to get a little bit back to the laser shearography and a little bit of perhaps nondestructive inspection 101 for the Board and the audience. Can you explain the value and the purpose of laser shearography on the Delta and also why it may or may not or should or should not be applied to External Tank, as well?

THE WITNESS: Well, to answer your second part, I would defer that question to NASA and a Lockheed representative. As far as for our Delta 4, we find it real helpful in terms of performing that NDE method, nondestructive testing, because I was informed it takes about 10 seconds to perform a section of about a 2-foot by 2-foot area. So they can move along the tank quite readily.

ADM. GEHMAN: Would you mind moving your microphone a little closer. Thank you.

Did I understand you to say that for the Delta 4 you do this laser shearography for 100 percent of the tank?

THE WITNESS: That's correct, sir.

DR. WIDNALL: When you do that, what sort of voids, if any, do you find? What do you find out when you do that?

THE WITNESS: Well, the voids that have been found were mostly coming from the BOFI foam, which is the bond-on foam insulation, not the spray-on foam insulation.

DR. WIDNALL: And these are attachment points that we talked about? The attachment points, places are -- I mean, the pour-on foam is for the attachment points for the solid rockets?

ADM. GEHMAN: Or pipes and lines.

THE WITNESS: Okay. The pour in place?

DR. WIDNALL: Yes.

THE WITNESS: Okay. Can you repeat the question? Sorry.

DR. WIDNALL: Well, we don't want to get confused here. Why don't you tell me a little bit about the voids that you found.

THE WITNESS: Okay. The voids that we found are on the bond-on foam insulation. The way it's been done is they apply adhesive onto the panel of foam and they basically bond it in place to the metal substrate and they apply pressure to basically cinch the foam together and let it cure over a recommended time.

ADM. GEHMAN: So what you're saying is that what the laser shearography shows then is a problem in the foam or the bond?

THE WITNESS: The bond, sir.

ADM. GEHMAN: The bond. Thank you very much.

GEN. DEAL: As a former member of the External Tank working group, can you describe what your relationship was and what you dealt with as a member of that group?

THE WITNESS: I thought it was very well. We basically built up a core of folks from different what I would call sites. Basically it was very much an open book in terms of discussion or issues and problems because our main goal was to try to expedite issues and problems that may come up at Kennedy Space Center. You know, I thought it was really a good working relationship; and it was chaired at that time by Mr. Chris Raymond.

GEN. DEAL: What was your focus as a member of the ET working group?

THE WITNESS: My focus was mainly on at that time looking at qualifying an EPA compliant blowing agent for the foam. It was at that time a switch from the CFC11 to HCFC-141B.

GEN. BARRY: Just as a follow-up to that, are there any lessons learned from what you did on the External Tank that were applied to the Delta 4?

THE WITNESS: I would say no because, again, at that time I was mainly focused on the Shuttle Orbiter main propulsion system and the power reactive supply distribution hardware and those hardware are mainly using pour-in-place foam.

DR. HALLOCK: Can you talk a little bit about what happens when you've fueled the rocket -- that is, you put the liquid oxygen and liquid hydrogen? When that happens, I understand that the shell itself is going to contract because of the temperature change. What does that do to that bond or the foam itself? Is there a problem with things like moisture being absorbed at that time?

THE WITNESS: I can share with you a little experience I had during the certification of these pre-molded foam segments that we were looking at certifying to replace the old method -- I call it old method that's being replaced, of injecting foam into the mold. During that time in the tests, Kennedy Space Center team members were building these foam blocks that we brought over to Stennis, Mississippi, to perform this certification. We basically installed these foam sections together and held together with aluminum tape. Basically we watched. I was fortunate to watch a Shuttle rocket being fired; and after it's fired, we all as a team went up almost immediately to witness the foam sections. Yes, it does shrink quite a bit; and it was through several iterations that we finally got a foam segment that didn't crack all the way through.

ADM. GEHMAN: Mr. Chong, you'll have to forgive this very, very layman's question about insulating these fuel tanks which are, of course, extraordinarily cold. I believe one is maintained at something like minus 250 degrees and the other one is at minus 400 or something like that. So obviously they have to be insulated. Would you tell me, please, why you put the foam on the outside of the aerodynamic surface instead of inside and keep the outside of the aerodynamic surface smooth?

THE WITNESS: Actually when I first was brought in from Rockwell to Boeing, there was a team at that time with McDonald Douglas that were looking at insulating the interior surface of the tank. Learning as far as I go, I realized in talking with the folks who were from the inspection group that it would be a nightmare trying to inspect foam inside the tank and also the fact that the foam, wanting to shrink, might pull away from the substrate, the metal substrate.

ADM. GEHMAN: Thank you for that. It's not clear to me that it pulls away any more or less by putting it on the outside of the tank than the inside -- I don't mean on the inside of the tank but I mean on the inside of the vehicle. Why don't you insulate the tank instead of insulating the rocket? Why don't you insulate the vessel rather than insulating the outside of the aerodynamic vehicle, because the aerodynamic vehicle is going to be stressed by launch and aerodynamic forces and all that kind of stuff? I'm just having a hard time figuring that out. I'm sure there's a good reason for it.

THE WITNESS: Maybe I need to understand your question. Are you referring to putting foam between a sandwich core?

ADM. GEHMAN: Right, having a tank inside and then having an aerodynamically clean exterior skin. In the case of Delta 4, it's probably not such a big deal because if some of the foam comes off, there's nothing around it to do any damage; but in the case of the External Tank, if the foam comes off, there are a lot of things, a lot of moving parts and operating things that the foam could hit, not just the Orbiter wings. Orbiter control surfaces, Main Engines and Solid Rocket Booster motors. So I'm just wondering from an engineering point of view why would you imagine that they didn't insulate the fuel tanks and leave the outside aerodynamically smooth. There probably would be a good reason for it. I just didn't know if you knew what it was. I wouldn't want you to speculate.

I would like to go back to the question that General Deal asked about the External Tank working group that you were on, 1991 to 1994. Your role, as I understand it, was primarily to work on a group to make recommendations having to do with the changing of whatever that agent is --

THE WITNESS: The blowing agent.

ADM. GEHMAN: The blowing agent, right, because of environmental reasons essentially. The old one was what?

THE WITNESS: CFC11.

ADM. GEHMAN: Freon. Freon, which, of course, is environmentally hazardous. So you had to find another blowing agent. Was the consensus of your group that you went to the next best agent that you possibly could have, or do you think that you found a better agent?

THE WITNESS: I think at that time that was the best agent that's available in the industry for us to evaluate and use.

ADM. GEHMAN: But was it next best to freon or was it better than freon?

THE WITNESS: Okay. I heard that freon was better.

ADM. GEHMAN: This was the best that was available, not including freon?

THE WITNESS: Correct.

ADM. GEHMAN: Are you aware, of course, of what happened from the first time they used it on the ET, External Tank?

THE WITNESS: I was aware there was foam popping off, popcorning from the intertank.

ADM. GEHMAN: So NASA learned how to deal with that. Thank you very much.

GEN. BARRY: Could you give us a little bit of insight on the contractor oversight that we have with the Delta 4 program and, if you can, relate it to the way NASA operates? Do you have any insight on both sides? Or you can just share with us on how Delta 4 is doing. Government oversight of the program.

THE WITNESS: You know, I'm not sure I can answer that because I know -- I'm not being involved in that.

GEN. BARRY: Well, let me ask another question. Let's go back to the freon for a minute. What is the replacement spray? Is it GX6000? Does that ring a bell?

THE WITNESS: Can you repeat the question again?

GEN. BARRY: What is the replacement for freon? What did you call it, the spray-on foam? What was the type of spray-on that was?

ADM. GEHMAN: What is the name of it?

THE WITNESS: That's for the External Tank?

GEN. BARRY: Right.

THE WITNESS: As far as I know, it's North Carolina Foam Insulation 24-124.

MR. TETRAULT: I have one question. The working group that you were on, was that specifically look at replacing freon; or was it much more broad-based in terms of looking at all the problems there might be with regard to the External Tank?

THE WITNESS: It's more than just focusing on replacement of blowing agent. As an example I can cite to you is that I got requests from the folks from Kennedy Space Center as far as looking at another technique of applying the pour-in-place foam. Their recommendation was maybe put the foam, two-component foam into the melting bottle and shake it and then transfer it into the cavity they need to fill, instead of the previous method which was the foam was packaged in the chem kits that they mix. Their complaint, the challenge that they have was that you've got to be quick with those chem kit mixing because if you're not, the foam will basically literally squirt on you. So that was an improvement to the existing method, and from there we evaluated and basically certified it.

MR. TETRAULT: One final question. Was the periodic loss of foam which had been occurring considered by the working group to be a problem?

THE WITNESS: To be honest with you, that was not discussed.

DR. WIDNALL: I guess my question is somewhat similar. Your group ended in 1994. Are you aware of any other activities that have been going on in the External Tank to really improve the foam? I guess it's the second question that Roger asked. Was there any concern that one should continue to work this problem until one developed a foam and bonding system that had better adhesion properties?

THE WITNESS: Can you repeat the question? I'm sorry.

DR. WIDNALL: Well, I'm asking. The work that you described stopped in 1994 with this development. Are you aware of any concern that such work should continue to develop foams that don't fall off during launch and, if you are aware of such activities, what was sort of the level of intensity of such activities?

THE WITNESS: Unfortunately I was not aware, as far as how much work. I do know that they're working on the issue, but I don't get intimately involved in the spray-on foam insulation on the ET Orbiter.

ADM. GEHMAN: Can I follow up on Dr. Widnall's question? In your present position, did you research various options for fuel tank insulation of the Delta 4 rocket? What I mean is you probably looked at other options besides using the same foam that's used on the Shuttle ET.

THE WITNESS: Okay. There is another candidate of spray-on foam that was looked at, and it was made by a Japanese company, in Japan. From what I know is that it was dropped as a candidate, to the North Carolina foam that we currently use, because of the costs.

ADM. GEHMAN: Let me ask you a couple more questions. In your experience even with the Delta 4 rocket, can the foam absorb water? Can it absorb moisture?

THE WITNESS: The answer is, I would say, no, mainly because the foam in itself, it has 90 percent minimum closed cell content. However, that 10 percent included is because there are times when you do trim or sand the rind off, which exposes the closed cell of the foam.

ADM. GEHMAN: You probably are aware that the STS system, the whole system of rockets and External Tanks is rolled out to the launch pad almost always 30 days prior to launch, sometimes five weeks prior to launch. Would it be your experience that the foam, including foam which had been locally repaired and cover plates which had been put back on locally and things like that, would you expect that there would be some moisture content in that foam?

THE WITNESS: If there is, it will be mainly on the surface of the cell that's been exposed.

ADM. GEHMAN: Then, of course, if that moisture was subjected to minus 400 degrees, it would turn to ice?

THE WITNESS: Yes.

GEN. DEAL: I asked you a while ago about if there's any lessons learned from the External Tank that you applied to the Delta 4. I'd like to ask you the converse of that now. Are there any things, inspections or processes that you have on the Delta 4 that we should consider applying to the External Tank?

THE WITNESS: Yes. One recommendation I have would be looking at shearography. Obviously that works for us.

GEN. DEAL: Anything else?

THE WITNESS: No.

ADM. GEHMAN: If you all are complete...

Thank you very much. You have been very helpful. I apologize if we have asked questions that are so low and mundane, but we appreciate your patience.

THE WITNESS: You're welcome.

ADM. GEHMAN: We are now expecting to see Mr. Harry McDonald take the table there.

Good afternoon, Dr. McDonald. Welcome. We appreciate very much your traveling here from a great distance in some not very pleasant weather to help us with this problem. I'll ask you to tell us briefly about yourself and your experiences and your last job that you had; but first I would ask, if it would be all right with you, I would ask that you just agree to this affirmation which I will read to you that you will tell us the truth, which I don't think will be much of a problem. If that's all right with you, I would like you to affirm to the Board that the information you will provide to the Board in this hearing will be accurate and complete to the best of your knowledge and belief.

THE WITNESS: I will.

HARRY McDONALD, having been first duly affirmed, testified as follows:

ADM. GEHMAN: Thank you very much. Dr. McDonald, if you would, please tell us a little about yourself before we start the questions.

THE WITNESS: I am a professor at the University of Tennessee, Chattanooga, and I hold the chair of excellence in computational engineering. Prior to that, I was the Center Director at the NASA Ames Research Center in Moffett Field, California. Prior to that, I was a professor at Penn State in the computational field also. Obviously I'm from Scotland originally, and I came to this country and never regretted it.

ADM. GEHMAN: Thank you very much. Maybe we could ask you to move your microphone a little bit closer. Thank you very much.

As the director of the Ames Research Center, you were the author or the chairman of a recent study of the Shuttle Program. Could you tell us the nature of that study, when it was, and why and how it got started?

THE WITNESS: Certainly. I've actually written a statement.

ADM. GEHMAN: Thank you. Go right ahead.

THE WITNESS: It covers that. If I may, I'll read it.

ADM. GEHMAN: Please go right ahead.

THE WITNESS: On July 25th of 1999, during the flight of the Space Shuttle *Columbia*, commanded by Eileen Collins, two separate malfunctions occurred which set in motion a significant series of events. At takeoff, a pin broke loose and ruptured cooling tubes in the Space Shuttle Main Engine, causing a slight reduction in the eventual attitude which the Shuttle achieved.

Separately, during that same launch, two of the Shuttle's engine controllers unexpectedly shut down. By design, backups seamlessly activated and assumed the lost controller functions and the vehicle made it safely into orbit and completed its mission and returned home.

Following that, a pattern of minor failure clearly had emerged that suggested to the NASA engineers a nascent wiring problem existed across the entire Shuttle fleet. After being informed of the engineers' concerns, NASA officials immediately ordered wiring inspections of all four Shuttles, grounded the vehicle; and while repairs were effected, NASA administrators also ordered a complete review of the Space Station Program with regards to safety and empowered an independent panel of experts to that end.

The group, which I chaired, was known as the Shuttle Independent Assessment Team or SAIT. Our mandate from NASA was to evaluate procedures, maintenance procedures in particular and processes, and to make recommendations for improvements, without regard to cost.

The administrator at that time, Dan Golden, took me aside and urged me to leave no stone unturned. Our work stretched from October of '99 to March of 2000. Among our more than 90 findings, SAIT determined that processes, procedures, and training which had evolved over the years and that had, in fact, made the Shuttle safer had, in fact, been eroded. The major reason for this erosion was the reduction in resources and appropriate staffing.

I believe the report is quite detailed on these issues and stands on its own merits. NASA agreed with our observations on the staffing issues and immediately moved to stop further Shuttle staffing reductions from the civil service side. They added safety inspections and sought additional resources for the program. Wiring inspections and repairs were extensively performed on all of the vehicles and monitored. Indeed, before we had submitted our formal report, NASA had added 100 new inspectors to the work force at Kennedy; and on the same day as we released our report, Joseph Rothenberg, the Associate Administrator for human space flight, at that time announced that 800 additional civil servants would be brought in to Kennedy Space Center. So clearly the agency took our report very, very seriously.

Following an extensive internal review of our findings of over 120 recommendations that we made, some were acted on without delay, as I have indicated. Some it was felt, would not be effective. They were submitted to the Space Station Program for their review, and their review came back that they felt some would not be effective and/or required significant resources or longer periods of time before they could be implemented. Some were implemented. Some were deferred.

I was personally disappointed that more of our recommendations were not or could not be implemented. Documentation of the disposition by the agency of our recommendations exists and was made available to me for this meeting and I believe will be posted on the web for people who are interested in it.

In the SAIT report it was recommended that the implementation process be examined, the implementation of our recommendations be examined by another independent review team later. It was also recognized by SAIT that our particular team did not have the technical expertise to perform an in-depth review of other components of the space transportation system -- for example, the External Tank, the Space Shuttle Main Engine, and the solid rocket motor.

In the light of what was learned on the Orbiter, however, our team felt that a number of the issues were systemic in nature and such that an investigation of the other system components was, indeed, called for. Accordingly, it was one of our recommendations that an independent panel of appropriately qualified experts be formed to perform reviews of the Space Shuttle engines, Solid Rocket Motor and the External Tank.

The members of the SAIT were also asked for their views on the safety of the vehicle, the Orbiter, one of three, for a return-to-flight status. Much discussion took place by the team and it was concluded after extensive consideration that the SAIT response should be carefully restricted to a statement that in light of the extensive inspections of the vehicle which had been undertaken, and upon completion of some additional wiring inspections that we had recommended, it was likely that the vehicle would possess less risk than other Orbiters which had recently flown. SAIT did not express a view on the absolute level of flight safety or flight risk but expressed a view of the flight risk relative to other Orbiters that had been flown.

I'd like to conclude this particular part by recalling two statements from our report -- one being, "The Shuttle Program is one of the most complex engineering activities undertaken anywhere in the world at the present time," and the other being, "SAIT was continually impressed with the skill, dedication, commitment, and concern for astronaut safety by the entire Shuttle work force." I see no reason to qualify either of these remarks today.

Thank you, sir.

ADM. GEHMAN: Thank you very much.

GEN. HESS: Doctor, in reading through the report, one of the points that you make in here is that there seems to be a tendency for accepting risk, based on past success. I wonder if you would give us a few comments on how you came to that conclusion and what you think might be affecting that particular mentality inside of an organization as complex as NASA.

THE WITNESS: Well, indeed, we did come to that conclusion after extensive review and discussion with the people involved. I think there was a basic flaw in the reasoning of many well-intentioned people; and that is the concept that if you have a 1-in-100 chance of risk or of an event occurring, the event can occur in the first or the last and it's equal probability when the event would occur. There seemed to be the perception within agency that if I have flown 20 times, the risk is less than if I have just flown once. And we were continually attempting to inform them unless they've changed the risk positively, you still have the same issue even after 50 flights or 60 flights.

Now, how do you address that issue? One of our big concerns is that clearly everybody in the agency has this desire and sense of the importance, critical importance of safety. There's no issue about that. The question is how do you translate that into a safe and effective program. That is very, very difficult, given the complexity of the issue.

One of the several of our suggestions really aimed at what I might call communication that we understand the level of risk that people are adopting. For instance, in tracking the pin ejection event, we discovered that the PRACA, Problem Reporting And Corrective Action data base did not have an appropriate recording of the ejection of the pin. Indeed, the real probability of a pin ejection was 1 in 10; and I don't think anybody realized that that was the probability of an event.

Now, the second part of that was that the Shuttle Main Engine was, in fact, designed to have cooling tubes fractured; and I believe the number is it can stand four tubes in the Eileen Collins flight. Only two were ruptured. So effectively it didn't reach a high visibility. But the real reason for pinning the oxygen ejectors is that a broken injector, which is what you were repairing, you pin it to stop the flow going through it. The real reason for pinning it is if the ejector tube is broken, there's a risk of fire in the power head of the Main Engine, which is a whole different ball game. So on the one hand you have an assumption of risk by well-meaning, well-intentioned people that is not appropriate in this system context.

So part of our thrust was to try and improve communications, improve the data bases so that you could have an immediate reaction to what is the probability of a pin failure and what is the effect, the true effect of a pin failure. So working on that type of resolution of the issue to try and translate these very well-meaning, well-intentioned safety-is-first into a safe and effective plan is what we were trying to bring attention to the fact that many of our process were, in fact, deficient, had been eroded. A long answer, I'm sorry, but it's a key question, I believe.

GEN. DEAL: Sir, as you have stated, you were disappointed that all the SAIT recommendations were not implemented. It's clear that you have confidence in those recommendations. Could you give us a flavor for maybe the top two or three recommendations that were not adopted that you may still harbor concerns over?

THE WITNESS: If I may, I picked up the wrong file here. If I may, just so that I can be precise. It's in my briefcase.

ADM. GEHMAN: Help yourself. Being precise is a good idea.

THE WITNESS: I was fortunate to get this file from NASA Ames yesterday. Having left the agency, I had to file a FOIA request for my own memos, which is fine. You probably have all read the report or been exposed to it. It was given, as I mentioned, to the Shuttle Program for their review. They presented a very detailed critique of every single recommendation. Some they accepted; some they did not. We responded to all of their critiques, and I wanted to give you the sense of that before I went into some specifics.

This is our response to their critique. (Reading) The process described by the Space Shuttle Program to address the SAIT recommendations is one that SAIT viewed very favorably, i.e, that existing processes would be reviewed in detail to further examine the weaknesses suggested by the evidence obtained, observed by the SAIT. In several areas the SSP, Space Station Program, appears to have successfully fulfilled this approach. For instance -- then we give a series of cases where we believe they successfully implemented what it was we were trying to recommend.

Then we go on: In a number of cases, however, the SSP appeared not to have followed this process to the required degree. Existing procedures are occasionally quoted as a response, without seeming to provide an assessment of their adequacy or to address the SAIT's concern. Examples of these include responses to recommendations. Then we give a series of issues that we felt were not addressed.

In other responses, evidence provided by the SAIT is ignored. For instance, Issue 5 was written to address the actual breakdown in the process of performing green runs of repaired SSMEs. However, green run testing of the SSME and its failure was not discussed in the SSP response. Another example -- and so on and so on.

Category two, No. 13, in which the incident of spilled hyperbolic fluid caused by inadequate operator experience, as reported to the SIAT, was not addressed -- that was on *Columbia*, incidentally, in 1999. Other responses are based on assertions that dispute evidence observed by the SIAT, for example -- and we give a list.

In several case of disputed evidence, the SIAT interpretation of the evidence is corroborated by findings of the USA Orbiter Subsystem and Maintenance Process Review. And it goes on to give examples.

And lastly, finally some responses from the SSP do not provide enough information to assess their adequacy relative to the findings and recommendations.

Now, we can go into specific examples; but I was trying to give you the flavor of our response. We concluded that particular exchange of memos by the general observation. The overall feeling left with the reader following a review of the SSP response is that the program views its highest risk as that being associated with human error. This leaves the program to address many problems with increased awareness, process management, and while these are clearly worthwhile activities, the SIAT felt that a higher priority should be given to creating solutions where the opportunity for making mistakes was reduced. This led the SIAT team to emphasize in-depth incident analysis, in particular human factors analysis of near-misses and diving catches and other incidents which could have had much more severe consequences than what actually occurred. Based on this analysis, actions could have and should have been taken to remove or reduce the probability of a repetition.

So that was our feeling on how the agency addressed the -- how the SSP addressed our concerns. I think there was one -- the closeout memo from myself to the Associate Administrator of Spaceflight closes with this observation. "Therefore I must reiterate the SIAT's recommendation to set up a follow-on independent review committee with a charter to provide additional continued inquiry into Shuttle processing and maintenance. This review committee should as a first action bring a detailed approach, implementation and results of SSP's response to the SIAT recommendations."

In other words, we had felt we had reached essentially an impasse, that we had said one thing, the program had said another and let's let some time pass, let's bring in another independent review team and make an assessment of what had been done and what had not been done, what was right and what was probably erroneous. I'm sure there were certain of our recommendations that were based on our poor understanding of their process. I'm sure that's the case, but not all of it was based on that.

ADM. GEHMAN: Thank you.

DR. HALLOCK: In your introduction, you used a phrase that there were a number of recommendations that could not be implemented. Could you expound upon that, please?

THE WITNESS: Well, I think the particular events I had in mind were events that were rather longer term in nature. I think I referred to them earlier, to straighten out the data bases that exists on problems and issues that occur, in order to make them accessible to certain Google-type searches so you could pull up all the instances and not only just on the particular local data base but throughout the entire data base that had been collected over the years on the engine or the rest of it. That's a long-term project requiring a considerable development, a considerable application of resources, of people, et cetera. And that was, in our view, quite appropriately put into the Shuttle upgrades program, which the Administrator at that time, Mr. Golden, had gone over and gotten $1.7 billion for a Shuttle upgrade program. I believe that was the figure in the budget. We expected and hoped that programs, the longer-term programs that we had advocated would be funded; and it was indicated that they would be funded as part of that practice. However, that program, as we all know, was significantly reduced and a number of these activities were either curtailed or not performed. Again, it gets down to risk perception and what the value of these issues were perceived to be to the program.

GEN. BARRY: Dr. McDonald, let me compliment you on your report. I think most of the Board has commented, I heard at least more than once, that it is one of the more thorough documents that certainly has helped us get a focus on some specific areas. With that in mind, I was intrigued with one of the comments that's reported here on problem reporting and tracking process.

Now, you know the E-mail discussions that have been going on in the paper; and, of course, we're in the process of reviewing that too and have done quite a bit of work there. One of the statements that you had is it does not provide high confidence that all potentially significant problems or trends are captured, processed, and visible to decision makers. Based on what you have read and also based on your report, can you comment on the NASA culture that might be indicated by what you capture in your report? I know you're going to want to comment on this, but it might help us with some insights.

THE WITNESS: Well, the PRACA data base was clearly built in an earlier era before modern information technologies became available, before browsers, before data base management tools. It was essentially a tracking procedure to ensure that a given problem would be properly signed off on, so just a data compilation that ensured that an operator could find out that a particular incident had been -- a repair had been performed.

ADM. GEHMAN: Dr. McDonald, you're referring to -- we use an abbreviation here, P-R-A-C-A, PRACA?

THE WITNESS: Yes. Problem Resolution, And Corrective Action data base. And there are several data bases. An in-flight anomaly data base date. And there's a problem resolution data base. You know, there are multiple data bases. We wanted to consolidate and make them accessible to modern search techniques so you that could pull off information like that.

I think it's not an issue that presented -- I have no concern at all that people like Ron Dittemore, presented with the facts, will make the right decision. No concern at all on that issue. The concern is presenting him with the facts, and many of them are very deep, frequency of certain events occurring -- for example, the pin ejection that we observed and, in your case, Flight 87, STS-87. What was the resolution of the foam issue on STS-87? What was the flight clearance process for STS-88? When the problem recurred on 88, how was it resolved for 90 and then 91? I mean, when someone like Dittemore goes and tries to make an assessment of what the risk is for the FRR, flight readiness review, the instant access to all of that past history would have become valuable, incredibly valuable, I think; but we had not given it, in my view, sufficiently high priority.

ADM. GEHMAN: Thank you, sir. Let me ask a question, too. I've read your report and I agree with the other Board members that it's eerily prescient. The question that I want to get to is: Are you satisfied that in your report -- or did you cover this in your report -- are you satisfied that the NASA systems are sufficiently broad and stand-back far enough, that they could detect very subtle changes in risk factors just because, for example, the system is getting old or, for example, the original assumption back in 1975 and '76 when the RFP went out was that each Shuttle would fly 100 missions, that everything has to be built to last 100 missions? That's 30 years ago. That was a 30-year-ago assumption. It could be that there are trends out there that would suggest to us that that assumption is not going to be a reality.

Did you find, based on your report, that these macro trends, even though each indicator is just a tiny little pin dropping out, just like in your case where you lost a little pin and someone goes and fixes it and now it's fixed -- but, of course, it isn't fixed -- it's part of a bigger trend. To what degree are you content that these kinds of trends can be detected by the fault resolution and tracking system that we have?

THE WITNESS: I think it's best done by saying what the action was. There was considerable concern over precisely this point following our report; and with the complete support of the Associate Administrators, the administrator, Mr. Golden, we instituted two new programs designed for safety. One of its components -- and this is a research program, a clearly significant research program. It subsequently matured into something we all Engineering for Complex Systems. It was to try and provide the latest in terms of risk assessment techniques to the Shuttle Program office to help them, because it's a very difficult task that they faced, as well as some more advanced techniques that were focused on this issue of detecting very subtle trends and how important they might be. So a major program, research program was initiated by the agency to address precisely those issues. And other existing techniques like quality safety assessment techniques, QSRA, and other techniques that should have been routine were examined for their appropriateness in terms of the program. So it did galvanize the agency into a very significant effort in that regard. But insofar as implementing these procedures, well, no.

GEN. HESS: Doctor, one of the main things that has run through all of our examinations of the agency as part of this unfortunate disaster is the overall impact of reduction in the work force to maintain costs and schedule and the pressures that brings and actually the unintended consequence of sending perhaps a message to the work force that there is an imbalance between actually being safe and performing. Your report talks to that issue in several areas; but one that's particularly interesting to me is the part where you suggest that adopting industry standards for use in a program for the Shuttle, which is not really an operational vehicle, is sending a mixed message to the work force. I was wondering if this was kind of backed up by your interviews or what was the basis of that particular part.

THE WITNESS: I think all of this was based on interviews with the work force and interpretation of what they were saying. I would point out that, yes, they had heard and believed at one level that safety was critical, extremely important. Many of these people were really, really responsible technicians and engineers and, in fact, several of our team members which came from the Air Force and the Navy, commented on where do you get these people from, the quality of the individuals. So they were clearly deeply concerned both with the turmoil in the agency, the cuts that had been made, what was their particular future, and if they had to go work for the contractor, then it would be a different basis of employment, what did that all mean.

There most certainly was this mixed message of safety is very, very important, it's No. 1, and yet we were cutting back mandatory inspection points, government mandatory inspection points. Clearly some of that was very appropriate to cut back in a number of inspections. As Mr. Dittemore said this morning, on the basis of experience we've learned that some inspections were not required; but from their perspective I don't think we did a very good job of convincing them that these inspections had been reduced because they were unnecessary.

So they did get this mixed message, and it was of deep concern to us. While it came across in the one-on-one interviews, it was very difficult to get them to say this to management and to other people. It was only the confidence that they began to get in our particular Board and its independence -- and clearly we were independent. I think the report shows clearly that we were under no constraints as to what was said, and the agency is to be complimented for the freedom which they gave us. So the concerns were there, the mixed messages were there, and we had done a very good job of convincing them of the real issues.

DR. WIDNALL: Okay. I, too, have a two-part question. In your report, you have some return-to-flight recommendations. I'm doing a quick read here, but my sense is that you were recommending that this issue of waivers and exceptions be re-examined and the processes that lead to accepting hardware and design that perhaps didn't meet original spec. Do you have any comments on that? Were those recommendations accepted, or do you still have concerns in those areas?

THE WITNESS: Well, I think they were accepted in part and our concerns reside in the qualitative nature of some of the assessments. In family, out of family -- we couldn't really get a really good definition. It varied from person to person. So there was no consistent definition. Fair wear and tear was a subjective judgment. So there were issues like that that permeated it. While it was clear that it was received, the implementation of a more rigorous process was difficult.

Again, we come back to this information flow. The flight readiness review would be looking at perhaps 200 waivers, some of them minor. It really bothered us that clearly they would not understand or would not be able to go into the history of each one of those waivers. They were relying on someone assessing whether or not a waiver was justified. And we had this concern exhibited with the pin, that a relatively incorrect or poor understanding of the risk might lead to something being granted a waiver that was inappropriate and the ability to interrogate each of the waivers in terms of history, complete engineering backup, all of these factors was something that we would have like to have seen implemented. So that went beyond what the program office could do at this point in time. So, you know, that was another problem.

DR. WIDNALL: I guess the second part of my question really has to do with risk. I sort of see two risk curves in this process. One is a descending curve, and the other is a rising curve. The descending curve kind of goes along with the R&D nature of the Shuttle in some sense. It is like a research project. Every time you have a successful flight, there is a sense that your region of uncertainty is being narrowed and maybe you are free to take "risks" that you wouldn't have taken on the earlier flight. So that's sort of a descending curve. On the other hand, you have the ascending curve, which is the aging of the vehicle. So I'm really struck by the assumption that one can expand the family, whatever that means, on the basis of previous successes. I might ask you to comment on that.

THE WITNESS: It was just simply that the safe way is to adopt the philosophy that you haven't really done a whole lot to retire the risk. I mean, you're still flying the same vehicle. You haven't changed -- well, that is not quite correct. There were, of course, changes to the vehicle. The vehicle was becoming safer; but fundamentally unless you identify the risk that you are retiring, you have to stick with your original 1 in 100 or whatever it was. So the risk identification and the elimination is a critical point in allowing you to increase the safety of the vehicle. You have to understand the risk assumptions.

So I quite agree that, yes, in an experimental vehicle when you've flown once, you've made a big achievement. When you have flown twice --

DR. WIDNALL: Even better.

THE WITNESS: But fundamentally you started off with a 1-in-50 or 1-in-100 probability of failure; and you're still in that ballpark. Yes, a great deal was learned in each flight, I believe, and improvements were made; but there was the unidentified risks or poorly understood risks that continue to remain that brought the overall probability to fairly low levels.

GEN. BARRY: Dr. McDonald, one of the issues that you brought up -- and I know you had a rather large, extensive human factors team as part of your effort, which was very insightful-- but it says here that one of the things that you do here on your Issue 6 is that you say the Shuttle Program should systematically evaluate and eliminate all potential human single-point failures. Would you comment on that, on how much that was followed through on by NASA and maybe some others that you might look into?

THE WITNESS: Really, no, I couldn't address that particular issue. Yes, we were concerned about it. How the agency followed up was operation-specific, item-by-item, operation-by-operation. We could have cited a couple of cases where we saw single-point human factors issues, but I think the concept was to try and implement a more general program of eliminating single-point human failure. That required the program to look rather specifically at the various maintenance operations to determine if there were and what these were and how they should be eliminated. We had several that we could have identified, but we were interested in a much more broader assessment by the office.

ADM. GEHMAN: Dr. McDonald, judging from your report and your comments here today, I would gather that -- I'll make a statement, and let me see if you agree with me or not. Hiring more inspectors is really not the issue here; it's a process issue, a process problem. It's more complex. This is a very, very complex system; and when there are system failures, they're usually complex failures. So just hiring more inspectors is not the issue. Am I correct?

THE WITNESS: You are. I think hiring inspectors for the particular problem that we were addressing of wiring, it is the only current -- it was then the only acceptable method of determining the wiring issues. It's not a very good method. In general, what we're talking about is much more of a process issue. I would agree with that statement.

ADM. GEHMAN: Thank you very much for traveling down here to Houston to talk to us. As you may have been able to tell from this Board, your report, because it's not only the most recent study but also because we think it's very, very well done -- and we regret that it seems to be very applicable -- has obviously resonated with this Board. We've all read it and gotten good ideas from it. So we thank you for your service, your continued service. I think you should feel good that your report was not put on the shelf someplace and filed away but seems to be a live document that's still influencing things. So congratulations and thank you very much.

This Board is finished. Thank you very much for today.

(Hearing concluded at 2:30 p.m.)

March 17, 2003
Houston, Texas

Columbia Accident Investigation Board Public Hearing
Monday, March 17, 2003

1:00 p.m.
Hilton Houston - Clear Lake
3000 NASA Road One
Houston, Texas

Board Members Present:
Admiral Hal Gehman
Rear Admiral Stephen Turcotte
Brigadier General Duane Deal
Mr. Roger E. Tetrault
Dr. Sheila Widnall
Mr. G. Scott Hubbard
Mr. Steven Wallace

Witnesses Testifying:
Dr. William Ailor
Mr. Paul Hill
Mr. Robert "Doug" White

ADM. GEHMAN: Good afternoon, ladies and gentlemen. Welcome to our second public hearing. The subject of this afternoon's hearing is going to be a discussion of the reentry of the Shuttle *Columbia*, and we'll hear from several witnesses this afternoon. The first one is Dr. William Ailor. Dr. Ailor is the director of the Center for Orbital and Reentry Debris Studies from the Aerospace Corporation.

We are very thankful, Dr. Ailor, for you for taking time to come down here and help us walk through this. What the Board is interested is, first of all, a non-NASA view of how things reenter the atmosphere, which will help us form our questions for later this afternoon when we get the detailed analysis of how the *Columbia* entered the atmosphere, and your presentation will help us understand to a much greater degree what we'll hear later.

Dr. Ailor, I would offer to ask you to give us a short bio or your background, if you please; and then if you're prepared to start, we are prepared to listen.

WILLIAM AILOR testified as follows:

DR. AILOR: Okay. Thank you very much. Just by way of background, I joined Aerospace in 1974 and have been basically working reentries ever since that time. I'll go over in my presentation a little bit more detail on some of the ones we've worked on before, but Aerospace established the Center for Orbital Reentry Debris Studies back in 1997 in recognition of the kinds of issues that we expected to see from both space debris and the hazards posed by reentry and in recognition that there needed to be a fair amount of work done to understand the reentry breakup process. I'll go over some of that in my presentation.

So a little bit more background, I did work on the External Tank reentry a number of years ago, one of the issues where it was associated with what altitude did that break up. We worked very closely with NASA in resolving those issues. Then I've also been in various capacities on the Interagency Nuclear Safety Review Panel, which reports to the White House on space missions which carry radioactive materials – so Cassini, Mars Pathfinder, Mars Exploration Rover. We've worked on all of those.

So if I could have the first chart. Okay. Go back one. No, that's good, I'm sorry.

What I'm going to talk about is what we can learn from reentry debris. This is really based on the experience that we've had over the last 25 years in this area, actually longer than that. Aerospace has been working in this area for a long time, and our desire has been really to understand the breakup process. Again, these things coming down through the atmosphere can present a hazard to people and property on the ground. One of our objectives has been to understand what that hazard is and to be able to model it and perhaps minimize it as time goes on.

So what I've got here is an overview of the reentry breakup process. This is just for a standard reentry; and as I'll show you in a minute, we see a number of these a year. For a typical satellite reentering, it slowly comes down through the atmosphere, slowly works its way down out of orbit in an orbit decay fashion or, in fact, you can actually drive something into the atmosphere – and I'll talk about that in a bit, as well.

Basically for unprotected space hardware, the heating and loads will gradually tear it apart. I'll talk more about that in a minute. The kinds of things that we've seen that survive reentry are things that you would probably guess might, things like steel sometimes – I'll talk about that – glass, titanium, and then parts that are sheltered by other parts.

One of the things about the reentry breakup process is that the heating is like, in a sense, cooking an onion. You basically start from the outside; and then as you heat the pieces up to a point where the materials will fail, that will expose some new materials. They'll go through the same process and the object can be broken apart. We do have objects that are melted and shedded away, things like aluminum, solar panels. Things like that come off pretty early. Mylar sheets. Some satellites are wrapped in Mylar sheets.

Once this debris comes off from the parent body, it follows its own trajectory at that point. So it will go on about its business, basically, based on its own properties. If it's a very dense, heavy piece, for example, it may go further. If it's a very lightweight piece like a solar panel or something like that, it will fall early in the trajectory.

Then the debris pieces impact on a footprint on the ground. I've got an illustration there that just shows that typically what we see is initial breakup or shedding of some things like solar panels that come pretty quickly. And we have catastrophic breakup. I'll talk more about that but typically it can be quite a substantial event. There can be secondary breakups that happen when those pieces come apart. Then you see a footprint where you get low-mass debris that comes in early; and typically longer, heavier pieces go late. We'll talk more about that, as well.

Next chart. Okay. So just some characteristics of reentry breakup. It's characterized by intense heating and major fragmentation; and as I mentioned, fragments are shed as the structure heats and fails. Typically we see instantaneous high loads. For example, when an object comes off of a parent body it now experiences the air stream that exists there; and it will respond based on its own characteristics. For example, if you've got a very lightweight piece that comes off of a heavier object that's coming through the atmosphere, it's like throwing a piece of paper out of a car. That will decelerate very quickly, and the same things happens even at Mach 20. So when you do that, you see very high loads; and you can also see very high heating. That can be important if you're trying to understand what actually happened in the process, because now you've got an object that's been separated from a parent body that, just because of its own interaction with the atmosphere, will have seen a fairly severe environment.

You can have some events with moderate velocity increments. What I mean by that is if you've got a fuel tank or something like that that explodes, it's like a balloon. Some of those fragments will pick up some velocity increment from that. We've measured as high as a thousand feet per second. And the initial breakup can be energetic. Basically a typical way for things to break up when they reenter is that they'll come down through the atmosphere for a certain amount of time, they look absolutely fine, we've seen videos of these things where they just like spacecraft coming down, and all of sudden they come apart. When they come apart, they just disintegrate. That altitude typically is around 42 nautical miles, plus or minus a few nautical miles; but that's a pretty good guess. So just as a rule of thumb, it seems like a critical point for space vehicle reentry and breakup is around 42 miles. We have never had any measurements internal to a spacecraft during this breakup process and that's something that we would like to see. It would really help us understand the process better.

Next chart. Survivability depends on a numbers of factors. The material. For example, the melting point of the material, the heat capacity. Just by example, it's very rare to find aluminum on the ground from a standard spacecraft reentry; and finding aluminum on the ground would basically mean that that aluminum was somehow protected as it came down. Steel can survive. It doesn't have to, though. We have cases – for example, there was a Russian satellite that came down in Canada, had steel, a reactor case. That reactor case basically disintegrated during the reentry, but also I'll show you some pictures of steel that did.

Size, shape, and weight. An empty fuel tank, in a sense it's a lightweight object relative to its size. That will affect its survivability, and that can be very important. For example, fuel tanks survive. Things as dense as a battery? We've never found a battery on the ground.

Release conditions. If an object comes out late in the reentry, after being shielded for a portion of the reentry, that means a lot of the energy has been taken out of that trajectory prior to that object's release; and that object is more likely to survive. And shielding. Again, objects that have been shielded for partial reentry can survive; and that's one reason, by the way, that, for example, you can find circuit boards on the ground from satellite reentries. What that means is typically when a satellite is being constructed, circuit boards are built internal to other boxes which are internal to other structures and so forth. Again, if you think about this heating process where you're removing the outer layers as you come in, every time you do that, you're removing energy and then finally these things will be released.

Next chart. When these things come down, there's a typically generated debris footprint. Now, this is a notional footprint here. I've got several breakup conditions separated by about 30 seconds in trajectory time. This shows things like the types of dispersions that we typically see. This has got dispersions in winds. So winds will affect things as they fall, even a big, heavy object, as I'll show you in a minute. Ballistic coefficient is a measure of, in a sense, how dense an object is; and that will affect where things go. Typically on these footprints at least, for example in the red swatch you see up there, things that have gone longest downrange, farthest to your right, would be heavier objects. The lighter objects would hit towards the up-range portion.

Then atmospheric density. We don't quite know what density is in most trajectories. So in that case we have to build a factor in to allow for that. Then also, as I mentioned, it's possible to get some velocity increment as things come down. So we put in a delta feed for that.

So basically what you can see here is these ellipsoids were generated at each of these time intervals, and you can see how they overlay each other. If you look carefully at Breakup 4 down there, that's the one where the trajectory is now healed over a bit and you can see that even though the same types of debris are there, the footprint is inside of the one just prior to that. This indicates that trying to figure out where debris came from on a reentering spacecraft and where it happened is a very difficult process, indeed. These are four specific time steps. What you have to recognize is this is basically happening continuously as the spacecraft reenters. So the footprint is not even as nice as what you see here.

Next chart, please. Noteworthy reentries. Just to give you a little background, it was mentioned earlier that someone said this is not a data-rich area; and I have to agree with that. What you see here are some of the primary data sources for doing this type of work. Cosmos 954 came down in 1978. That was a reactor-powered satellite and there was radioactive debris that came down in Canada. Since it was radioactive, you could find it pretty easily and a lot of that debris was recovered and was examined and documented. That's probably exceptional on these kinds of things. Typically the effort is simply not put forward to find debris on the ground, and so we simply don't have as much.

Skylab occurred in 1979. Some of the debris fell in Australia. There was some debris found, but again there was really no detailed analysis of the footprint itself, as far as I'm aware.

I'll show you some pictures of some Delta 2nd stages in a minute. We do have large debris pieces surviving from that. We did reconstruct the trajectories and try to understand the breakup of those.

And there were two targeted reentries. The ones above that were all basically, in a sense, brought down just by the atmosphere itself. In other words, the atmosphere drags things out of orbit slowly. The last two were actually targeted into ocean areas because of potential hazards they posed. The Compton Gamma Ray Observatory, that was targeted to an ocean area. There was no debris found from that one. And then the Mir Space Station was also targeted to an ocean area. The only debris I'm aware that was found was reported by a guy who was beachcombing down in Fiji, a job I'd like to have. He did have one piece. It's not been substantiated that it actually came from Mir, but likely Mir had debris surviving and it may float up on a beach somewhere.

Next chart. The type of work you can do with a reentry as far as reconstructing what actually happened to it, there are a number of things you need to do. There's maybe tracking data – for example, radar data. Video data, for example, the type of thing that people would take. If people have seen it from aircraft, any of that data can be very useful in rebuilding what's happened in a reentry break. Public sightings and witnesses. On most of the reentries we've got here, the public actually has seen some of these things coming down. That information has been very useful in rebuilding what happened during the reentry.

Debris on the ground. Knowing where things are, what they look like, how much they weigh – all that information can be critical to rebuilding what happened. That's one of the reasons why the work that's going on now, both from the public and other agencies looking for debris, is really critical to this investigation.

Data on the original vehicle. It's one thing to have debris on the ground, but you need to know what the original configuration was like. Sometimes we simply don't have good information on that. If it's a foreign satellite or something like that, we may not know exactly what was coming through the atmosphere. So we don't have a good feel for taking the debris back up.

The next thing you try to do is fuse all that information and basically rebuild the reentry trajectory, try to match the impact locations to possible release points and take any existing weather data, any of that sort of thing in, and then finally conduct metallurgical analyses on the debris to estimate temperatures, really look at what went on, those kinds of things.

Next chart. This is an example of a reentry. This one came down over Canada. This was in 1997. You can see that on that chart we show a breakup altitude at the magic 42 nautical mile number. And there are some fragments. We'll talk more about those, but this is one. This again, the public was out. This was about 3:00 o'clock in the morning. There were reports to news stations and so forth, and we actually used that information.

Next chart. This is some pictures of the debris recovered from that one; and this is one of the larger debris fields, I guess, that we've actually had a chance to see. As I say, typically unless it lands next to a farmer's house as you see in that chart there, people don't find these things unless they happen to be out and about. So what you see in the upper-left corner, this is the original configuration as it was being loaded onto the launch vehicle up there. There actually was a satellite on top of that. This stage was released in orbit and was in orbit for about nine months and then gradually the atmosphere dragged it down.

The big brown tank you see over there is about a 570-pound stainless steel tank. It landed about 50 yards from a farmer's house here in Texas. He was not pleased. The woman you see on the top right actually was brushed on the shoulder by a piece of the debris. Again, she was very lucky; but it's a very lightweight piece.

The sphere you see down here was one of four on that vehicle. That was the only one found, although we believe they all survived. So they're still on the ground somewhere.

The bottom one just shows that these things can survive in pretty good condition. Those are screws that you actually could unscrew. They held an aluminum plate onto the Tank itself. The aluminum is gone, but the screws were still there and just fine.

Next chart. This again gives you a little detail on that one. Again 550-pound tank. 67-pound titanium sphere. 100-pound thrust chamber. Footprint length was about 400 nautical miles long on this one.

Next chart. This is a detail of the trajectory reconstruction. The trajectory comes in from the top and each of those little black dots is about two seconds apart. So you can see just by the spread of those dots that it's moving at a pretty good clip originally. That's up and around 18 nautical miles up. When you get down to around 10 nautical miles, it looks like it does a little dogleg there and that is due to wind. So basically where an object of this type comes into the atmosphere, typically all the orbital and all that motion is gone, the atmosphere has basically taken that energy out, and it will fall from, say, 50,000 feet straight down. That's one reason why when you see debris on the ground, even on the pictures of the farmer's house with the debris there, you'll notice there's really no crater. Most people don't realize these things just fall straight down and they just land. That's just a characteristic of this. That little dogleg is again caused by winds. It hit the jet stream, and it blew it over. This, again, was a 570-pound tank. So you can see that even that can be moved.

Next chart. One of the things that we did was we were able to get a portion of this fragment that brushed Lottie Williams on the shoulder and we actually wanted to find out if, in fact, it did come from the launch vehicle or from that vehicle. We analyzed that and found that – if you take the next page please – that it did. The trajectory time was consistent. She was out walking at around 3:30 in the morning and actually saw the reentry and then this thing came down and brushed her on the shoulder and she recovered that. We did get a piece. We brought it into our labs and did an energy dispersive X-ray analysis of it. There are actually two on this little red chart you see here. There are actually two lines there. One is the original material, and the second is what was recovered. So we are very confident that this material actually came from that vehicle.

Next chart. The second thing we did is take samples from the large tank itself, put it through a metallurgical analysis. We found, for example, that in portions the aluminum actually combined with the stainless steel and that we were able to use that to pin down the maximum temperature on the Tank between 1200 and 1280 degrees centigrade. The other interesting thing, and I'll show you another example of this, is that it appears that this aluminum splashing back – again, aluminum is there on other parts of the structure – that the aluminum splashing back on the Tank can actually oxidize or burn and the heat released by that can melt holes. We believe that's why the hole was actually melted in this tank.

Next chart. Just to show you, this is not all that unusual an event. This is some pictures of basically the same debris objects. These came down near Capetown, South Africa, in April of 2000. So basically the same objects.

Next chart. This is another one we have. This is a solid rocket motor stage that came down in Saudi Arabia. This one is made out of titanium, which makes it a little unusual. The ones you saw before were typically out of steel. This is titanium. It would be expected to survive very nicely. We have evidence again that the hole you see here was actually burned, in a sense, in the casing as the aluminum oxidized on it.

Next chart. So just learning a little bit from the debris and limitations there, we typically model reentry breakup at the macro level. We simply don't have a good understanding of what happens at the micro level with these kinds of things simply because we don't have a lot of data to base our models on at that level. We do have a few reentries where significant debris is found; but, just by way of information, of the stages that came down in Texas and South Africa – we have about ten of those that come down a year – those are the only two we've found, only two where debris was found. So most of these land in the water or in places where they are not discovered. We also see about a hundred reentries of major objects a year. So finding debris on the ground is very unusual, although we do get hits on our website. People email us with things they have found and ask us if that potentially is of that type.

Just by rule of thumb, we would estimate that about anywhere from 10 to 40 percent of an object will actually survive reentry and that can depend on what it's made of. If it's got some big, heavy, empty fuel tanks, that can really be a factor there. There has been relatively little work on reconstructing reentry breakup events. The ones I've mentioned are about all there are. Again, one of the most important features is there's really been no systematic retrieval effort except in a couple of cases. I guess the Cosmos 954 would be an exception and, again, the objective there was to recover the radioactive material.

Next chart. Some observations. As I mentioned, the heating to an object can really be exacerbated by burning of other material. For example, this phenomenon of aluminum melting and splashing back and the heat of oxidation actually increasing the temperature and burning holes, we believe that's a real situation. There are large aerodynamic deceleration loads, and also you've got an object that's already been fairly well heated as the reentry progresses. So that can lead to structural failure and actually can mask other information about what happened during the breakup.

Combining data from multiple sources can be critical for reconstructing a reentry event. Finally, the distribution of debris on the footprint may actually be very useful in providing clues on the breakup sequence itself. So things like if you find objects early in a trajectory, that can be really very critical to seeing how that reentry progressed.

Next chart. So, in summary, reentry breakup is not well characterized at the micro level. That breakup and subsequent disintegration can and does destroy clues of critical events. The debris field may be very useful in helping to track down what ultimately happened. Data fusing is really a critical part of this. You really must take everything that you can learn, all the data you can get, and try to reconstruct what the event was. Then a final piece of that is laboratory analysis of the debris pieces themselves to look for things that can be shown to have occurred earlier or have been protected by other objects as the event progressed.

I think that's my briefing Sir.

ADM. GEHMAN: Thank you very much, Dr. Ailor. All right panel. I know we've got some questions.

MR. HUBBARD: Dr. Ailor, thanks for being here with us. We appreciate someone of your expertise speaking to us. I have two questions that are follow-ups on some statements that you made. One is about the percentage of material that's been recovered in your previous data base. Where we are today with the *Columbia* is something on the order, by weight, of 15 to 20 percent. So I would like your assessment, based on what you know, of whether you think this is a low or a high or what we might expect in the future.

DR. AILOR: Well, as I mentioned for typical reentries we see between, say, 10 and 40 percent. It really can depend on what materials the object is made of. There may be significant debris pieces that have yet to be discovered, I don't know, but I would say that's certainly in the range of the experience in the past. The other part of this is that we've never had the detailed look or the energetic search for debris that we're seeing now. So it's possible that you may get a higher percentage as time goes on.

MR. HUBBARD: Thank you. The other question was related to your statement about aluminum rarely being found on the ground. We're finding some aluminum, although mixed with other debris or attached to other debris. Can you give us a brief explanation of why that might be the case?

DR. AILOR: Yes. Our experience has been that unprotected aluminum will not survive a reentry event. The heating is just too high. It typically comes off very early in the trajectory. We do find aluminum, say, bits of aluminum that has been flowed back on to tanks and been protected, say, by a titanium sphere or something like that. It will flow onto the lee side and be protected back there. But we typically don't find that. For debris that you're finding now, most likely aluminum on the ground was protected for a significant part of the reentry and probably was released late, when there wasn't sufficient heating to cause it to melt.

ADM. GEHMAN: Thank you.

MR. TETRAULT: Dr. Ailor, one of your charts talked to the five satellites that had broken up in the atmosphere. To put this in perspective, could you tell me how many total pieces in history have we had compared to the 30,000 pieces that we will now be working with from the Shuttle.

DR. AILOR: Well, in history, we actually have examined probably five or six, just to give you an example, the several big tanks and so forth. There was a number of debris pieces that were picked up from the Cosmos 954. I would say in history we're probably talking about in the order of maybe 250 or so that have actually been noticed by humans on the ground and reported.

MR. TETRAULT: Thank you. One follow-up question. You talked about the ballistic coefficient. For everybody's edification, could you kind of distinguish the difference in the ballistic coefficient between something like a tile, a tank, and maybe a landing gear strut.

DR. AILOR: Absolutely. Ballistic coefficient is a measure of how significantly the atmosphere is going to affect the flight of an object. The way to think about it is a very low ballistic coefficient object would be like a feather. Extremely low ballistic coefficient. A Shuttle tile, for example, released by itself, very light object, would have a very low ballistic coefficient, as well. Something with a medium ballistic coefficient would be something like a tank, an empty fuel tank. That big tank I just showed you here has a ballistic coefficient on the order of 15 to 20. Then something like you were mentioning, a landing gear strut, I probably would imagine that would be up to 40 or 50, something on that order. A ball bearing would be something that would have a high ballistic coefficient. So it would be something where the aerodynamic properties really would make it less susceptible to the atmosphere and also its mass properties would give it a lot of inertia.

MR. TETRAULT: Thank you.

ADM. TURCOTTE: In the examples that you gave of the different reentries that you had, they were obviously at different inclinations and they were at different reentry profiles. Would you kind of explain the difference in what you know of the Shuttle's reentry profile at that inclination and some of the data that you've had in the past from the other satellite reentries.

DR. AILOR: Yes. The other satellites that I spoke of either were deorbited or basically were orbit decayed down, had very shallow path angles typically. They flew what we call ballistic trajectories, which mean there really wasn't much lift involved with them. Of course, the Orbiter is a lifting object and lift did play a big role in its trajectory – for a good portion of it, anyway. That trajectory will affect the heating rates and so forth and will affect how the object responds to the atmosphere.

MR. WALLACE: This is the first time we've had a breakup of a vehicle designed for reentry. Is that a fair statement?

DR. AILOR: Of this type, yes.

MR. WALLACE: This ballpark, your 42-mile estimate, was pretty close, given the situation of the *Columbia*. Does the fact that this was a vehicle designed for a safe reentry change some of your estimates about percentage we're likely to find and any other sort of effect on the breakup sequence?

DR. AILOR: Well, it certainly could. As a matter of fact, the fact that there is a heat shielding on at least a portion of all the body for a portion of the time and then some of the body parts after that will affect what survived. That's certainly true. I should mention that the Shuttle External Tank also is a reentering vehicle after it's released from the Orbiter during launch. That typically breaks up at a slightly lower altitude, maybe around 40 nautical miles plus or minus a little bit. What happens there is there is some amount of heat shielding and it does protect it for a little bit. So there are objects where there is a shielding existing and I think the fact that the breakup sequence that can be shown that there was a material loss at a very high altitude for the Orbiter may indicate that the heat shield may have had a problem.

DR. WIDNALL: You mentioned earlier that aluminum rarely survives, certainly in its bare state. Could you sort of go over all of the possible things that you could think of happening to aluminum in reentry both for, say, an individual panel that suddenly found itself all by itself in the atmosphere and also maybe a panel, say, on the leading edge, like leading edge spar of the Shuttle wing, that was attached to the Shuttle but was bare? What are the different range of things that could happen to such aluminum?

DR. AILOR: I'll give you an example. Some of the work we've done has been looking at a large spacecraft that reentered with solar panels and we believe and have data to indicate that the solar panel came off early in that reentry. Some of data we have makes us believe that that solar panel, even with an aluminum structure, actually survived. So that's a case where again you have a big –

DR. WIDNALL: Now, that's ballistic coefficient.

DR. AILOR: That's exactly right. it's a big, flat, plate. It spreads out, stops very quickly, and then essentially just falls to the ground. So something like that could survive. So in that case aluminum could be expected to survive. If aluminum is being carried along by a heavy object – for example, you saw the Tanks we have here – these were big, solid pieces of material. The aluminum on it is a piece of structure. As it's being carried by that heavier object, it's really governed by the aerodynamic and heating and so forth that's characteristic of that object. That could be much higher than the aluminum itself can stand; and when that aluminum gets weak, it will come apart.

DR. WIDNALL: I'd like to go beyond that. So you're saying melting?

DR. AILOR: Melting. Absolutely.

DR. WIDNALL: Vaporization?

DR. AILOR: Melting, yes. Turn into droplets.

DR. WIDNALL: Well, droplets? How about individual atoms, vaporization?

DR. AILOR: I would assume. You'd have to ask somebody more qualified in that area than I am.

DR. WIDNALL: Oxidation?

DR. AILOR: Oxidation for sure. We've seen evidence of that.

DR. WIDNALL: Of course, another word for oxidation is burning.

DR. AILOR: Exactly.

DR. WIDNALL: The example you gave was aluminum deposited on another tank which essentially burned and created – but I suppose it could also burn all by itself.

DR. AILOR: It could, although aluminum released by itself probably would stay in a droplet form and decelerate pretty quickly. So aluminum that would be carried along by something I think would really be more likely to see that.

MR. TETRAULT: In the hole that was created that you talked about, was that created by the aluminum burning or the alloying effect?

DR. AILOR: It was, we believe, by the oxidation of the aluminum itself; and that raised the temperature up where you could actually see the alloying occur.

ADM. GEHMAN: I was very interested in your comment about the ball of paper being thrown out the window of the car – not just because that's my level of understanding. What you suggested was that in an entry scenario like we're investigating here, there is a heating and an aerodynamic force, one of which is extraordinarily fast, and then when the object then becomes free and floats down to earth, it's still got heat but it's no longer of this extraordinarily short-period high intensity. My question is: When we go looking through debris, should we be able to detect those two phenomena – that is, a piece of metal which has been flash heated versus a piece of metal that's been subjected to prolonged – by prolonged I mean tens of seconds or maybe even more? Can you see the difference, in your experience?

DR. AILOR: For aluminum to actually see, as you say, the flash heating, the way that will work is that when an object is actually kicked off, if it's has got material attached to it – for example, it's tile material with some substructure attached to it – if it comes out in a way where the tile material is forward and actually protects the material behind it, then that might be likely to survive. The problem is going to be with, No. 1, the breakup process is going to continue on about anything, about any object that's put out into the stream that's going to continue to see heating for a short period of time. If there is much material there and it's a very low ballistic coefficient item like a big, flat plate with some material behind it, structural material, that will heat up very quickly, as you say. The aerodynamic loads will also be quite high as soon as it hits the air stream. That can have a tendency to fracture it further. So this breakup process is going to continue as it comes down. Secondly the dynamics may actually get into the process. So this object is tumbling. Then the different sides will see the air stream. So it will be a difficult process, I think, to try to see a piece on the ground and make sense out of it from that perspective.

ADM. GEHMAN: I take it in one of your viewgraphs, for example, of a sphere that came from one of the Deltas or something like that in which all of the burn marks all around the sphere look approximately the same, would it be, in your experience, safe to conclude that that sphere had been tumbling and all of the sides had been subjected to the same amount of heat, whereas the one that had the hole burned in it it's safe to analyze that that was another event of some kind? That's kind of what I was getting at.

DR. AILOR: That certainly can be. You're right about that. As a matter of fact, on one of the Delta tanks, one of the spheres, about a 2-foot diameter sphere, one side actually does have droplets of aluminum that are clearly visible on it. The other side is absolutely clean. So you can say that during the heating phase that one side was facing the oncoming air stream and saw more heating than the other side did. Exactly.

ADM. GEHMAN: Another question. Certainly in the case of those spheres – and by the way, in the case of *Columbia*, I'd ask, Mr. Tetrault, we have found essentially 20 out of 25?

MR. TETRAULT: We found at least 25, not counting fragments, out of approximately 30. I don't know what the exact count is (talking over – inaudible).

ADM. GEHMAN: (To Mr. Ailor) As you predicted, the spheres all survived. But in the debris field, not discounting the spheres, your suggestion is that in the terminal velocity, in the terminal vectors, even when you start off going 10,000 miles an hour, by the time you reach the thick part of the atmosphere, you're essentially dropping vertically.

DR. AILOR: Correct.

ADM. GEHMAN: Therefore, how would you characterize whether or not we should find buried debris or not? Would you expect most of the debris to be on or near the surface?

DR. AILOR: I would expect most of the debris would be on or near the surface. Buried debris would not be typical for a spacecraft reentry. That would require a very dense material and would also require it to have some aerodynamic properties which you're not going to find on a reentry object.

GEN. DEAL: Dr. Ailor, I've got two questions for you. You've probably heard that from the second to the fourth day on orbit there was a piece of debris that was separated from the Shuttle and that went on to reenter, we have some extensive analysis going on through testing at Wright Patterson Air Force Base right now, trying to determine the radar characteristics of it. Are there any type of predictive methods that you know of that might tell us, by the characteristics of its reentry, what type of material it was?

DR. AILOR: Certainly if we had information on the reentry itself, yes. On the rate of decay, the rate of decay from orbit would be indicative of the overall aerodynamic properties of the object and its weight. So that would be some useful information to have. If there's tracking data, for example, on the reentry itself, that can be used.

GEN. DEAL: Then a second question. I looked at your slide that said from a Saudi Arabia reentry back in 2001, analysis is still ongoing, which doesn't bode well for us to get back to our day jobs anytime soon – two years later. Can you tell us what we can expect to find through laboratory analysis of the debris in the short term?

DR. AILOR: In the short term, the critical thing, I think, is going to be to try to center the analysis on certain debris pieces that there's some reason to believe have high value. What I mean by that is if there's debris that can be determined by analysis to have come from a particular part of the vehicle itself, that's of interest. Then you should really focus on that. I think the initiating event is probably what is of interest here. So a lot of the final debris that is in the debris field will have happened well after the initiating event. So the search that's going on for early debris is really very intelligent and the right thing to do.

The other thing would be to look for the debris itself and see again if there's characteristics of the field that would indicate that debris in this area, for example, came from a portion of the Orbiter of interest. So I would really try to focus on that. Laboratory analysis? There's too much debris here to be doing that extensively. So it's going to have to be focused.

DR. WIDNALL: Why do things tumble in the atmosphere, and is there a possible diagnostic use of measurements that appear to show something tumbling?

DR. AILOR: Well, even in orbit, things can tumble. For example, as you come down from orbit, you know, there's still a little bit of atmosphere up there and so as you get into the portion where there's enough to actually affect the dynamics of an object and have that become a more principled player, it will gradually overpower the gravity gradient forces which are there and try to stabilize the spacecraft. That interaction then will cause an object to tumble.

As you come down through the atmosphere, the mass properties and aerodynamic properties of an object will also cause it to tumble. We certainly see that. As to whether or not things like tumble rate could be a factor? It certainly could be, but you'd have to know a fair amount about the aerodynamic properties, about the geometry and other properties of the object to be able to determine that, I think.

MR. HUBBARD: I'd like to pursue a little bit more the question of how we might be able to determine the initiating event and distinguish that from the processes that may have happened post breakup. In your experience, would you say that from directionality of, let's say, a deposition of molten materials or the way the surface had been worn away by heat, we could begin to separate the two? Would that be a fair characterization?

DR. AILOR: Certainly could be. For example, the Orbiter was controlled for a good period of time and if evidence is found that could have occurred during that period and it indicates that a particular flow pattern or something like that, I think that could be very useful. Absolutely. I think the early debris would be really critical to an analysis like that.

MR. HUBBARD: Even from debris on the ground, following the discussion of ballistic entry of a steel strut, if it's worn away sort of equivalently versus something that shows that's there's more deposition or thermal damage on one side or another, it might be a distinguishing characteristic?

DR. AILOR: It certainly could be.

ADM. GEHMAN: Sir, based on your analysis of previous satellite reentries – I don't want to put words in your mouth, but let me make sure I understand it – your suggestion there on kind of your first viewgraph was that the typical reentry, the process starts rather slowly and little things come off but then it reaches some catastrophic point where everything flies apart. I have got that right?

DR. AILOR: That's basically correct.

ADM. GEHMAN: And that is not an unusual scenario, doesn't indicate a design flaw or anything like, it's just that aerodynamics and heating of the things reach a point where it can't tolerate it?

DR. AILOR: Exactly. And basically when the disintegration process starts, it is typified by not a violent event exactly but you can call it a catastrophic event where the spacecraft really comes apart into a number of portions and then from that point on, an observer on the ground would essentially see a number of objects proceeding through the sky.

MR. TETRAULT: We've struggled, like everyone, with how do you separate out reentry heating from the event itself; and our plan is to really look hard at the differences between the right wing and the left wing. I would assume that you would agree that that's probably a good approach in trying to look at the differences between the two?

DR. AILOR: Yes, indeed, I would.

MR. WALLACE: In the civil aviation field where I usually work, we often have the challenge of differentiating damage that may have precipitated a failure event in the sky or damage that was sort of part of the failure sequence versus what was impact damage on the ground, often very critical distinctions to be made; and, of course, here we add in the thermal effects. What are your sort of thoughts on the basic methods you can use to sort those things out?

DR. AILOR: Well, as you say, the challenge here is going to be that the heating itself is going to have the potential of masking the heating and loads during the breakup process; and as an object comes down and continues to break up as it enters the atmosphere, it's going to have this tendency to mask the initiating event. That's going to be really the challenge here. That's why I think that the effort really needs to be focusing on the early debris and on, as you say, the differences. If there are scenarios that would cause differences in some of the debris, that would be very useful to know about. Thirdly, to focus on surviving objects which can be traced back to areas of interest by one fashion or another.

MR. WALLACE: Has there been anything generally in your observation of the *Columbia* debris distribution and recovery process that has sort of surprised you?

DR. AILOR: Well, I've been pleasantly surprised by the efforts that's been made to really recover the debris pieces and get specific information on those things – the weights, the latitude and longitudes of those. The amount of effort that's being put into it, I think, is not really characteristic of these kinds of events and may be very useful. So I would say I've been very pleasantly surprised by that.

ADM. GEHMAN: Dr. Ailor, the two most western pieces of debris that we've found both have been tiles, either a fragment of a tile or an individual tile, not connected to any metal or any structure. My understanding is you are suggesting, then, that a tile would have a relatively low ballistic coefficient –

DR. AILOR: Right.

ADM. GEHMAN: – and therefore the flight path is nearly vertical?

DR. AILOR: Well, certainly ultimately will be vertical, yes.

ADM. GEHMAN: What I mean is compared to something with a high ballistic coefficient.

DR. AILOR: Yes.

ADM. GEHMAN: Backtracking into space, then, it would be safe to assume that these things, these tiles came off relatively close to where they were found on the ground, compared to a dense object?

DR. AILOR: Yes. That's exactly right.

ADM. GEHMAN: The fact that in almost all the dense objects that we've found we've found a couple of hundred miles down range, what I'm trying to do is rationalize in my find the dichotomy between something with a low ballistic coefficient that comes off late versus something with a high ballistic coefficient that comes off early, because you could have them found in reverse places on the ground.

DR. AILOR: Well, a lot of that will depend on the timing of the release, too. If you've got something that's released at a very high altitude early in the reentry and it has a very low ballistic coefficient, as you said, that object will, in essence, stop very quickly and flutter to the ground. It's complicated by the fact that if it was simply a tile that came off, that's one thing; but if it was actually bringing something else with it, then there may be more going on there. That other piece of material would have probably increased the ballistic coefficient a little bit, which would make it blow a little further down.

As you saw from the footprint chart that I gave where it had the multiple footprints there, the altitude and what the trajectory looks like as it begins to heal over there will really affect how things fly; but there can be low ballistic coefficient pieces that are released all through that process. So some will be carried further because they're attached to heavier debris. Some will be released and then flutter to the ground. So as you move forward in time, the footprint becomes much more complicated.

ADM. GEHMAN: Another question. You mentioned the inability of aluminum to survive reentry for one reason or another. It either burns up, melts, oxidizes, vaporizes. What is your experience with rubber? We have found five of the six tires, and maybe a fraction of the sixth. We have found five of the six tires, two or three of which actually look like tires.

DR. AILOR: Well, in the first place, I've never seen a spacecraft come down with rubber on it before.

ADM. GEHMAN: You've probably never seen one with wheels either?

DR. AILOR: No, never.

ADM. GEHMAN: You've never seen rubber in the debris?

DR. AILOR: I haven't. I'm sure someone could take a look and basically say if rubber experienced heating of this type, how would it be expected to respond. Some materials can be protected by the fact that they actually shed away external layers, for example, ablative materials that are used on the spacecraft reentries typically. So it may have properties that would enable it to survive of that type.

ADM. GEHMAN: Very good. This debris field that we have here I think you're familiar with. We're talking about just west of Dallas to just over the Louisiana border, which is about 375 miles or something like that. Are you surprised it's that small or that big, considering that, I guess, the first shedding event that we know about was at about 225,000 feet – actually we're going to find that out here in another 20 minutes or so. Right. You had a viewgraph up there that indicated in one of these reentry things it was spread over 400 miles. What do you conclude from this one?

DR. AILOR: That footprint I was talking about was from the little piece that actually brushed the lady on the shoulder. Very low ballistic coefficient piece, probably less than 1 – so it was something that, in fact, did flutter down – to the fairly large objects which were ballistic coefficients up to around 50, 60, something like that. So those are a reasonable range of ballistic coefficients.

The size of the footprint here is about what you would expect to see, I think.

MR. WALLACE: You were very complimentary of the amount of shoe leather that's gone into this recovery. Do you expect that any further major breakthroughs or strokes of luck are more a matter of shoe leather, or are there calculation methods you think might be further explored, backtracking pieces you have found?

DR. AILOR: Well, there's a couple of things. First, I think the work that's going on relative to finding the debris is really an important part; and that has to be emphasized. That's going to be key to solving this puzzle, I believe. The second part would be to look at the debris field itself, but you have to have collected debris in that field. So this idea of going out and finding these things, I imagine that pieces will continue to be found over a period of time and they need to be cataloged and brought in and examined just as they are being now. But to really look for anything that's related to, as I've mentioned before, possible scenarios – for example, the right-wing-versus-left-wing scenario and those kinds of things. So I think that's the way it should go.

MR. HUBBARD: One last question for me at least. Looking at your observations and summary, you bring up the concept of data fusion here. I wonder if you could elaborate on that a little bit. What do you really mean there?

DR. AILOR: Well, basically the data fusion means that, for example, where we have videos that have been taken by private citizens, taking those videos, processing those things, we know the Orbiter's trajectory very well during portions of reentry, in a sense, fusing that data so you take the video data, you marry it with the trajectory data so you know exactly what you're looking at. You can use that information to help derive information about, what objects are shed, where are these objects, what the timing is, what are the characteristics of those objects, things like that. We talked about ballistic coefficient; but you can estimate, based on how fast something separates from the Orbiter in a video, what the characteristics of that object are. So that's what I mean by fusion, just taking all of the existing data and bringing it all together so that you actually have a complete picture, as good as you can do with the data you've got, of what actually happened.

MR. HUBBARD: Would you include thermodynamic analysis, you know, reentry heating in addition to these actual empirical observations?

DR. AILOR: Yes, I think that's true; but the fusing I'm talking about really is more of a trajectory level, if you see what I mean. There's certainly other data. The data on the ground, for example, needs to be brought into this, as well, and should be. So it's really a question of fusing the various data. I come out of the trajectory side of the house. So looking at data from where things happened in the trajectory, tracking them down, trying to derive information on the ground, and then really developing a best estimate of what actually happened is what I'm speaking of.

ADM. GEHMAN: That leads to my last question – that is, if you would, make a value judgment for us on the accuracy and efficacy of this reverse trajectory analysis. In other words, if you find something on the ground, how much effort and what value should be placed on trying to predict the point in the sky that this thing became an independent object? If you would, take a shot at that.

DR. AILOR: That is going to be a real tough problem, quite frankly.

ADM. GEHMAN: You mean because it's just not an accurate process?

DR. AILOR: It's not an accurate process. As I mentioned in my opening remarks, we don't have internal information from a spacecraft that's breaking up as to what exactly is happening with it. So modeling it down and doing computer models of the reentry and breakup of a spacecraft, we recognize that there's uncertainty in there. The problem with taking debris on the ground and transferring it back up is you don't really know how it got here. There will be debris on the ground that will be surprising, very lightweight things, things that in a sense could burn very easily but may have actually survived and impacted the ground. Those objects we know were shielded, because they wouldn't have gotten there otherwise; but where it was originally in the vehicle and then the scenario that it followed for shedding the various layers of material and the changes in the aerodynamic and mass properties of that host object as it came through the atmosphere is going to be a very tough thing to derive. That's why I think that really a key here is to look at the early debris as closely as you can to really try to determine what really happened prior to a lot of that breakup process going on.

ADM. GEHMAN: Of course, it's probably a variable – once again, I don't want to put words in your mouth. For example, if you were to tell me the ballistic coefficient of a sphere, a fuel sphere, I bet you could pin that ballistic coefficient pretty well; but if it was a piece of debris or a jagged-edged thing that was part tile, part metal, part strut, part bar, the ballistic coefficient might be a pretty big estimate?

DR. AILOR: Yes. In fact, again, the ballistic coefficient of what you actually find on the ground was different at say, 75,000 feet or 100,000 feet or 120,000 feet. So the higher up you get, the bigger the changes, if you're talking about going backwards in time. So what you find on the ground is one thing, but trying to translate that back up and say, okay, well, we know it fractured off of something, what was that? We don't quite know what that was.

DR. WIDNALL: From a forensic point of view, what are some of the most interesting observations that you can imagine making on the debris? The second part of that is does Aerospace Corporation have any metallurgic capabilities to help us analyze some of the observations we make on this debris?

DR. AILOR: We do have, and we have analyzed some of the debris in the past. So we have some experience in doing this work. The kinds of things that, again, will be important to look for here are opportunities, if you want to call them that, for preserving some of the original events. That could be where material is found, either heat shield material or something like that is found from areas where it likely came off and protected some evidence of the original events, that would be really critical. So I think it's going to be a question of looking for objects on the ground where it's likely that some of the original evidence from the original burning or fragmentation would be preserved, perhaps behind the wing leading edge or behind tiles, those kinds of things.

ADM. GEHMAN: Thank you very much. Would you like to have the last word? Any advice for us on how to solve this riddle?

DR. AILOR: No. It's certainly a tough problem, but I think the advantage here is that there's been so much interest by the public in actually helping to gather debris pieces. I think that's really to be complimented. We typically don't see that kind of interest, and those debris pieces can really be essential in helping solve this puzzle. So I think that's really been valuable.

ADM. GEHMAN: Thank you. On behalf of the Board, we thank you for your appearance here today and for summarizing what I know is a deeper and more exhaustive study of the reentry physics and aerodynamics. We appreciate your effort and want you to know that we've learned from you and we'll see if we can't solve this riddle with your help. Thank you very much.

The Board will take about a five-minute break.

(Recess taken)

ADM. GEHMAN: All right. Board, we're privileged to have two people who have been studying this tragedy since the first day and know more about it than most other people. Mr. Paul Hill and Mr. Doug White.

Gentlemen, before we start, we don't swear witnesses in but we do ask them to affirm that they're going to tell the truth and the whole truth. So I will read a statement of affirmation to you and ask you, if you agree with it, just say you will. So before we begin, let me first ask you to affirm that the information you provide to this Board today will be accurate and complete to the best of your current knowledge and belief.

MR. HILL: I will.

MR. WHITE: Yes, I will.

ADM. GEHMAN: Gentlemen, we know you, but for the record we would like you to introduce yourself and say a few words about where you work and what your background is and then we would be delighted to listen to as much of an opening statement as you would like.

PAUL HILL and DOUG WHITE testified as follows:

MR. HILL: My name is Paul Hill, and I work in Missions Operations Directorate here on the Space Shuttle. I'm a Space Station Flight Director. I've been a flight director for about seven years.

ADM. GEHMAN: And you are currently – what are you doing for the MRT?

MR. HILL: For the MRT I run a team that's called the early sightings assessment team. After Doug talks about the time line, I'll go into great detail about what we do and how we do it. The short answer is we're trying to make some sense out of the public imagery and any external sensor data that we can get our hands on to tell us what was happening to us as early in reentry as possible and maybe shed as much engineering information as possible on what was going on with the vehicle before we knew what was happening on the ground.

MR. WHITE: My name is Doug White. I'm a director of operations requirements for United Space Alliance. In my job I'm responsible for turnaround test requirements at the Cape. I'm also responsible for anomaly resolution. I'm also responsible for the engineering support during missions. I do have the time line to talk about today. As far as what I'm doing on the mission response team, I am on the team which we call the technical integration team. Basically our job is, from a management perspective, to try to pull together all the different efforts of the different teams, the aero, the thermal, the scenario teams, and try to make sense out of all the data from all the teams and then try to bring a coherent story together.

ADM. GEHMAN: Thank you very much. Which one of you is going to go first?

MR. WHITE: I think I'll go first. I plan to walk everyone through the time line. If you go to page 3 of my briefing, please.

On page 3, this is a graphic showing the sensors that we're most interested in in the left wing. This particular chart shows the sensors in the left wing. There are a number of sensors in the wheel well that we are interested in that we got data from that behaved in an off-nominal way. There are also temperature sensors out in the wing, some of which went off line, which was off-nominal, and some of which did stay on line, which also tells us things that were not affected.

The different colored wires that you see represent the wiring runs for those particular sensors. The pink one is for sensors that were aft in the wing and runs forward past the wheel well and then ultimately into the mid body where some sidewall temperature sensors, one of which has a yellow line coming from it, that indicates the wire run for that particular sensor which was inside the mid body. There's also a green and a gray wire run you see in the back there that goes through a connector box and into the aft. The green wire run is for sensor data from those particular sensors indicated by green dots. Then the gray wire run is for a power cable. It's a little bit different than the sensor wires. This provided power to the actuators and came from a box there which is labeled ASSA4. That stands for air surface servo amplifier. That basically provides electrical power and commanding to the actuators for the elevons on the back of the wing.

ADM. GEHMAN: Doug, before we leave that, pardon me for interrupting. To what degree is that a cartoon and to what degree is that a fairly accurate representation of where the cables actually run?

MR. WHITE: It's kind of in between a cartoon and fairly accurate. For example, the pink wire does run exactly alongside the wheel well and does turn and go in front of the wheel well and does run to a connector right forward of the wheel well, as is indicated there. So those are approximate locations of where those wire runs. Now, in the back there we see the green and gray and pink all together. Those wires may actually be separated in space by 1 or 2 or 3 feet. This is looking down on the wing, and so you can't see the actual vertical separation between these wire runs. Just because they happen to show up on top of each other in the picture doesn't necessarily mean that they're bundled together within the vehicle.

ADM. GEHMAN: What's the little insert box?

MR. WHITE: I'm sorry, I forgot to mention that. That little insert is for some sensors that were forward on the Orbiter. These are temperature sensors on a supply water dump nozzle, which is a nozzle used to dump excess water overboard. Right below that is a temperature sensors for the waste water dump nozzle, again used to dump waster water overboard. Then there's another one forward which is called the vacuum vent dump nozzle. Those sensors also gave us some off-nominal readings. Since they were too far forward to show in this scale, we just put them in as a little inset.

MR. WALLACE: Just to follow on Admiral Gehman's first question, are the Orbiters different? Are there variances in the actual location of the wires in the Orbiters?

MR. WHITE: There maybe slight differences between 102 since it was the first one built. 102 had a lot of wiring which was called development flight instrumentation, a lot of wiring for that. During its most recent major modification period, we removed a lot of that wiring. Some of it we just left in place. So the wiring on 102 was substantially different in the DFI aspect. But for the sensor wiring, it was pretty much the same –

ADM. GEHMAN: DFI? Developmental flight instrumentation?

MR. WHITE: Yes. DFI, developmental flight instrumentation.

ADM. GEHMAN: I'm the acronym police here.

MR. TETRAULT: Let me continue with the wire questioning. We do know that there were actually four cable runs running back aft that went around the wheel well compartment, one on top of the other. Are all of those sensors that you show going off in one those runs or in all of those runs or some portion in each of those runs?

MR. WHITE: All of the ones in the pink are all within one particular cable. We don't have the specifics about whether or not, for a particular part of run, any one of the wires was like at the back of that bundle or on the top of that bundle. There are also more –

MR. TETRAULT: The question is: As I look inside the Shuttle wheel well door and look up, there were four wire bundles that run aft?

MR. WHITE: Right. All of the ones in the pink wire are in a single bundle.

MR. TETRAULT: Okay. Are the red ones in that same bundle, the ones that went off in the aft end?

MR. WHITE: Yes, all of the ones that went off in the aft.

MR. TETRAULT: So everything that went off are in one single bundle?

MR. WHITE: Yes. There are also many other wires, though, in that bundle for which we do not have data.

MR. TETRAULT: Understood. Do we know if that's the top bundle or the middle bundle or the lower bundle?

MR. WHITE: If I remember the picture right, it's the upper one.

ADM. GEHMAN: But we'll find that out.

MR. WHITE: Yeah. And I can give you the more exact answer. I'm just trying to remember it off the top of my head now.

ADM. GEHMAN: We'll go back to the blueprints. Okay. Please continue. Thank you.

MR. WHITE: All right. Next slide, please. This particular time is about 7 1/2 minutes before loss of signal, at a GMT of 13:52, and all of our sensors appeared nominal.

Next slide, please. Now, this slide we didn't show any sensors going off line but we put this in the time line. This particular time 13:52:05 is the first indication that we had some off nominal from an aerodynamic standpoint. We were able to derive aerodynamic coefficients in yaw and roll which showed us that we were flying differently than we expected to. You're going to hear a lot more about that tomorrow, but basically the way we have done that is to look at the way we should have been flying, look at the way we actually were flying, and take the difference between the two and come out with some moments on the vehicle both in the yaw and the roll. This particular off-nominal event, it started first in the yaw component. We are seeing a different yaw here at this point in time than we expected to see.

Next slide, please. This is our first sensor that we saw with a small rise, and I want to stress that this was a very small –

ADM. GEHMAN: Excuse me for interrupting again. If it's okay with you, we'll talk about these things while you have them up.

MR. WHITE: All right. That's fine.

ADM. GEHMAN: This off-nominal measurement we will talk about tomorrow when we talk about aerodynamics and thermodynamics. I want to get to the level of detail that and your team have been going through. You didn't realize this until about Rev 12 or Rev 10. Can you tell me when this became apparent?

DR. AILOR: Well, fairly early on, the aerodynamic guys knew that we had differences in the flight control from what we would have normally seen. They looked at the aileron, and the aileron was behaving differently and continued to behave differently throughout the entry. It took a while before we could back out that particular moment in time that we just went through there was the very first indication that this derived yaw delta was first affecting us at that point in time, but fairly early on we were able to see some of the larger flight control responses that were off nominal to us.

ADM. GEHMAN: I could look it up here, but you may be able to tell me. We are approximately what altitude and what speed here?

MR. WHITE: I don't have those numbers. There are versions of this that do have all those numbers on there. I guess I could look it up, too. I have some notes here.

ADM. GEHMAN: But we're approximately 235,000 feet.

MR. WHITE: That's about right.

ADM. GEHMAN: Okay. Please go forward.

MR. WHITE: All right. This is the first sensor that went off line. This is a left main gear brake line, Temperature D. It began a very slow rise. We call it a bit flip, which is essentially one bit in the data stream showed that it was rising. And we believe this is the first indication that there was an off-nominal event and something was going on with the Orbiter inside that was causing that measurement to rise.

Going on to the next page, these are the supply water dump nozzles A and B that I talked about. There are three nozzles to the forward there – the supply water dump; the vacuum vent dump, which is the very forward one; and the waste water dump, which is actually below the supply water dump. These nozzle temperatures A and B both began an off-nominal rise rate. If you look at the graphs, you'll see a very small knee in the graph where the two sensors are rising at a particular rate and then there's a bend where they start rising at a faster rate. This continues for about 15 seconds or so and then it bends back over and starts rising at the same rate that it had been before, at the nominal rate.

MR. WALLACE: This picture doesn't tell you where that is, does it?

MR. WHITE: Well, again, that's why it was an inset. They're very far forward on the Orbiter, just right at the beginning of the wing. That little diagonal you see there is the very beginning of the wing chine, and they're just aft of the crew module portion of the vehicle. They're on the side wall. We're just showing them on the top for visibility. They're actually both on the side wall, just above the wing.

MR. HUBBARD: Now, this anomaly is in a completely different place – as you say, well forward. Is there anything that would lead you to believe that this is, in fact, a sensor malfunction, you know, something wrong with the box, the electronics box?

MR. WHITE: It does not appear to be. We don't know of failure scenario that would explain this as a sensor malfunction. We think it is real data showing us there was a change. Now, whether or not the change that caused these temperatures to rise is related to what ultimately caused our tragedy, we don't know. They may be connected, they may not. So we're including this in our data, and we'll continue to look at it until we can explain it.

MR. HUBBARD: So you're including that, this is real data, from everything that you know?

MR. WHITE: Yes.

DR. WIDNALL: How anomalous was this anomaly? Have you looked at early Shuttle flights to see if you had similar events?

MR. WHITE: For this particular measurement, we did look at every single mission; and every single mission, these vent nozzle temperatures rise at a very straight, steady rate. So this is an anomaly in that the rate changed; but it was a very short duration, about 15 seconds or so. They were rising at a higher rate; and after that, they went back to their same nominal rate. So whatever caused them to rise at this higher rate was a transient, at least locally transient event.

ADM. GEHMAN: I'm just stating the obvious here. Obviously this is pre-video here. We're out over the ocean?

MR. WHITE: Right. This is out over the ocean. If you notice in the lower left, there's a ground track trying to show approximately where we were with regards to the ground tracking. We're still well off the coast.

ADM. GEHMAN: So if something was going on, we have no video of it.

MR. WHITE: Right.

MR. HILL: We are within a few minutes of having our first video when we see this.

MR. WHITE: All right. If you go on to the next slide. This is the vacuum vent, just a few seconds later. It began its rise as well.

Next slide. Now we're back into the wheel well. This is the left main gear brake line temperature A. This is down on the strut for the landing gear, and it began a very slow rise. Again, all of temperatures in the wheel well first exhibit a very slow rise rate. It wasn't until about two minutes from now in the time line that they began a much more rapid rise rate.

ADM. GEHMAN: We're both trying to do the same thing here. We're trying to characterize the heat in the wheel well.

MR. WHITE: Yes.

ADM. GEHMAN: Can you describe to me exactly where the sensor is? Is it inside a block that's measuring the hydraulic fluid temperature, or is it up against the block where the sensor is out?

MR. WHITE: This particular one is on the hydraulic line that's on the strut. So it does have some exposure, fairly good exposure to the atmosphere in the wheel well.

ADM. GEHMAN: So it's not buried inside a great big block or something?

MR. WHITE: That particular one is not; but, you know, there is a heat sink of the actual strut itself. That provides some heat sink. Some of the temp sensors down in the wheel, you have the heat sink of the wheel itself. Many of the temp sensors that you see lined up four in a row that are on the side wall, some of those are actually under epoxy covers and so would not have a good exposure to radiation or convected heating.

ADM. GEHMAN: But this particular one?

MR. WHITE: This particular one would have a fairly good exposure.

ADM. GEHMAN: Thank you.

MR. WHITE: Next slide, please. This is back on the side wall. Again, this is the left main gear brake line temp C. Again, beginning a very slow rise.

Next slide, please. All right. Now we start to see things going on in the wing and we believe this is directly related to some sort of burning or disintegration of that pink wire run that's affecting these sensors. The reason we believe that is because some of the other sensors nearby them don't show any effects and these sensor do start to show effects. So we think it's happening away from where those sensors are.

It's showing not completely colored in. It's off line. These sensors, we've done some testing that when you burn through the wire, you end up with a variable shorting, a variable resistance in the wire and you start to see the sensor kind of trail-off in time. It doesn't immediately just go off to its off-scale low reading. So this particular sensor at this time began to trend down.

Next slide, please. Then a few seconds later that sensor was completely off line.

Next slide, please. All right. Here's another indication that we put in the time line of another off-nominal aero event. This is the first clear indication. We mentioned before that we had the derived yaw moment showing us we're off nominal. At this point we began to have an off-nominal roll component to the aerodynamics.

Next slide, please. Again, this is another sensor in the wing which began to trend down. This is the hydraulic System 1 left inboard elevon actuator return line temperature, and it began its movement downwards.

Next slide, please. Hydraulic System 3 on the left outboard elevon –

MR. HUBBARD: Just clarification as we go here. The ones that you feel fairly certain are showing the actual wire damage, have you been able to back up and reconstruct in the wire bundle what was where?

MR. WHITE: No, that's one of the things that we don't know. The drawings are not specific enough to allow you to reconstruct which wire might have been on the outside of the bundle, if you will, and which wire might have been farther back in the bundle, which wire might have been right in the center. We don't have that level of detail to know what the placement of each single wire was within its larger bundle.

MR. HUBBARD: Is there a hope of reconstructing that from closeout photos or as-built drawings or anything or is that pretty much –

MR. WHITE: No, we will not be able to reconstruct that.

ADM. GEHMAN: Are the wire bundles themselves encapsulated or covered other than the individual wires being covered?

MR. WHITE: Individual wires, sometimes you have like twisted shielded pairs and you have shielding around those; but then once you make a larger wire bundle, no, the wires themselves are not covered with any kind of insulation. We do, for a lot of our wire runs, put convoluted tubing around, that black crenelated tubing that provides some impact resistance for people working around the wire. That's made out of a Teflon-like material and provides some impact resistance, but it wasn't designed to provide any kind of a thermal barrier or anything like that.

ADM. TURCOTTE: As you're talking about all the wire here, all of this wire that you are talking about is all Kapton wire. Is that correct?

MR. WHITE: Yes. This is all Kapton-covered wire. Yes.

All right. We'll go to the next slide. This is the hydraulic System 3 left outboard elevon actuator and return line temp that actually finally went off line. As I said, it had begun its little – it takes a few seconds for these things to go down. Some of the ones that I'll show you a little bit later actually took quite a while to go off line, which indicates to us that maybe they were shorting or that part of the wire was burning through more slowly at that point.

Next slide, please. This is back to the system 1 on the inboard. That one has now gone off line.

Next one. This is hydraulic System 1 on the left outboard. That particular sensor is now gone off line. Again, as I said before, the reason we believe that the damage is occurring away from the actual location of the sensor is because you see that green dot right next to it and that particular sensor was not reading anything off nominal at that particular time. So whatever was causing the damage was happening somewhere else.

Next slide, please. This is back to Hydraulic System 2 left inboard elevon actuator. Return line temperature again started its slow change to going off line.

Next slide, please. Now we'll go back forward, and you notice that our supply water dump nozzles have now come back to their nominal rise rates. So whatever effect was going up front is now not there anymore and the supply water dump temperatures are back to their – they're still increasing. That's nominal, the way they've been for every other flight.

Next slide, please. Then also the vacuum vent nozzle also at the same time went back to nominal. You can see at this point we're just now crossing the California coast and just about to pick up video, which Paul will talk to you about in a moment.

ADM. GEHMAN: Doug, the sensors back by the elevons, all of them back there – I've got the same thing in front of me that you have. For the people in the audience, there's a time line, this little sliding scale across the top of the viewgraph.

MR. WHITE: Right.

ADM. GEHMAN: The first sensor. I'm talking about just the sensors that dropped off scale low. The first one is 52:56, and then the one just before this you've said was 53:35. So essentially that scenario that you just went through with these five sensors, that happened in 40 seconds. By my arithmetic it took about 40 seconds, that little scenario you just went through. If we assume that you're right that the insulation of the wires were melted and they shorted to each other or shorted to ground or opened – and by the way you should be able to tell us that, right?

MR. WHITE: Well, again, we have done testing so far to where we took – we're planning on doing more tests to get a more representative case, but we took a wire bundle, we attached sensors to the end of it, we put a torch on it, and we looked at the characteristics of the sensors going off line, and they do look similar to what we saw in the vehicle. We do see them begin to do a slow decline, and then they eventually go off scale low.

ADM. GEHMAN: So just for my mental picture, then this little scenario of whatever happened in that wire bundle took about 40 seconds, according to my arithmetic.

MR. WHITE: Yes.

ADM. TURCOTTE: Before we continue, could you explain the physical – I guess the void that is the wing, is it possible, for example, for air to flow freely in there? Is it a sealed compartment? Could you explain that as you're looking at the sensors – in particular, the relationship?

MR. WHITE: Let me see if I can explain a little bit. If you see the panels all along the edge there of the wing, those are the reinforced carbon-carbon panels or RCC panels. Behind them is an aluminum spar that runs all the way down the length of the wing. You see the vertical lines. Those are solid aluminum spars with some cutouts through them that would allow a vent passage, if you will. There's one main vent passage pretty much where the pink wire runs, which is how you get through those spars. The horizontal lines are representative of rows of boron aluminum rib struts which are basically tubes that are there for reinforcing the structure of the wing. So that area from up and down on the slide here would be all open; but in each one of the spars, which are those vertical lines, you're closed out except for some small openings.

ADM. GEHMAN: And the wheel well?

MR. WHITE: The wheel well is completely enclosed from the rest of the wing. There is a hole in the very front of the wheel well that's about 5 inches in diameter which would allow some flow into there. There are some other drain holes and some small openings around some of the hinge covers which would allow a very small amount of flow out. The square area of the hole into the wheel well in the front is about 19 square inches. The remaining holes altogether total less than 1 square inch.

ADM. GEHMAN: So the forward bulkhead of the wheel well, there's a hole with a screen –

MR. WHITE: Yes, it does have a screen on it.

ADM. GEHMAN: – which allows kind of free communication into this what we call the glove area.

MR. WHITE: That is correct. Yes.

ADM. TURCOTTE: So it's safe to say that an air molecule, once inside the wing, is pretty much free to flow around the inside of the wing?

MR. WHITE: Through the vent passages. Right. Also there's another hole between the wing glove area and the mid body that's forward, about where that yellow arrow is. There's another hole in the mid body there which is rather large. That particular hole is about 146 square inches.

DR. WIDNALL: What is the material that the wheel well structure is made out of?

DR. AILOR: It's made out of aluminum honeycomb.

DR. WIDNALL: How thick is it?

DR. AILOR: I do not know that thickness. We can get that for you.

DR. WIDNALL: Okay. But it's basically a thin piece of the honeycomb and another piece?

MR. WHITE: Right. A thin face sheet, some honeycomb material, and another face sheet.

Next slide, please. All right. We've annotated the debris events. We are over California now and we'll see in the videos from the public that we were starting to see debris being shed from the Orbiter. This is the first one that we've seen in any of the videos that have been provided to us. So we call it Debris No. 1. The timing on that is plus or minus 2 seconds, which is about the best we can resolve from the video.

Next slide, please. Debris No. 2.

Next slide. Debris No. 3. Coming off relatively rapidly.

Next slide, please. You notice with the little time hack up at the top there, we're starting to put triangles below the line for the debris events. The diamonds along the line there are for the off-nominal sensor readings, and then the two triangles on the top of the line are for the aerodynamic readings. That's how you read that little graph up at the top.

Next slide. This is the fifth debris.

Next slide. Okay. Now, we start to see another temperature rise in the wheel well. This is again also on the strut. Also should have some fairly good communication with the flow of whatever is happening in there. This is left main gear brake line Temperature B.

MR. TETRAULT: Can I ask a question about that? This one is probably the most confusing sensor to me personally. Line Temperature A went off – and I notice that you appear to have changed the timing on this a little bit – went off at about a minute sooner than this. Line Temperature A and B are about – the sensors are about 2 inches apart.

MR. WHITE: That's correct.

MR. TETRAULT: At the same time, you have D and C which have gone, which have significantly gone off already early, significantly separated both in the X, Y, and Z dimensions, which would tend to suggest that the entire wheel well compartment is warm. Why do you see this big, huge time lapse between A and B, which are separated by 2 inches? Is there any explanation that you all have come up with, or at least theory on why there is this big separation in time?

MR. WHITE: Right now we do not know of a good theory that holds together that says why one would show the rise and not the other. At about this time now, the rises are starting to become significant. So we don't have a good theory. It may be the amount of heat sink, the way it was attached to the strut itself that provided some more resistance to temperature rise. We really don't have a good theory right now for why one 2 inches away would rise earlier than another one.

MR. TETRAULT: It's significant in terms of the time. A minute in this entire time frame is a virtual eternity.

MR. WHITE: Yes. One possible explanation that we've been kicking around is the fact that whatever the event is that is causing heating in the wheel well might not be constant in the sense that it's continuing to direct flow into the wheel well. Perhaps we were directing flow in at one point in time and through the dynamics of the vehicle through the evolving change in the damage to the vehicle that the flow was redirected to some other part of the wing for a time and then came back.

MR. TETRAULT: You're talking about the equivalent of a run-away fire hose kind of thing.

MR. WHITE: Something like that. I wouldn't describe it quite that way; but, yeah, something like that where if you had some sort of a plume heating into the wing that maybe it was pointing one direction first and then another and then back again.

DR. WIDNALL: Given the extensive damage that has already occurred to the vehicle at this early time, I guess I'd question the use of the word "early debris." I guess from my point of view I would call them mid debris. I mean it's clear to me from the time line that things must have fallen off in the ocean well before California. And we don't know obviously.

MR. WHITE: Right. We don't have any evidence of that. These are the first debris events that we see. So we just started at 1.

DR. WIDNALL: But at this point you've already got some kind of hole in the vehicle, you've got a wire bundle that's either completely burned through or burning through, you've started to pick up what I interpret as flow inside the wing. So clearly some structural damage has already taken place, by whatever mechanism.

MR. WHITE: Right. We do believe that we had structural damage somehow at this point in time that was allowing flow into the wing. Whether or not we had shed any debris out over the ocean earlier, we can't say one way or the other. It would be speculation.

MR. HILL: We call them early debris to distinguish them from the actual spacecraft breakup over Texas.

DR. WIDNALL: I understand that.

ADM. GEHMAN: Doug, in your machine here, you don't have the sister viewgraph?

MR. WHITE: I do, but they told me they could only project one at once. If you want to see the other one – you're talking about for the vertical elevations between these?

ADM. GEHMAN: Right. If you could do one of them. I don't know if you could do the companion to this one or not.

MR. WHITE: Well, if they want to go ahead and bring it up, it's called Part 2.

ADM. GEHMAN: Well, okay. Let's not do that.

MR. WHITE: Okay. We could do that. I think they only have the capability to show one at once, though.

All right. Let's go on to the next slide. All right. You asked about how early we were able to see things. The start of the slow aileron trim change – again, I put the triangle up on top of the line there – this was one of the early aerodynamic things that we noticed. The two events that we talked about earlier took some time for us to back out and reconstruct.

From examining the data shortly after the accident, this was one of the things that we noted pretty early in the data. So this is another aerodynamic event that's off nominal. We started to see a slow trim change in the aileron.

In the Orbiter there is no real physical aileron like you might have in an airplane. The aileron is a theoretical difference between the elevon position on one side of the vehicle and the elevon position on the other side of the vehicle. So by adjusting the relative different positions between those, you can create the aileron effect. So that aileron effect was keeping the vehicle flying the way we wanted it to. So as the forces began to change on the vehicle, the trim changed; and we saw that in the data.

MR. HUBBARD: Doug, I just want to check and see that we're working from the same time line here. What I've got is what's called Rev 15.

MR. WHITE: Yes. This should be Rev 15.

MR. HUBBARD: Now, you skipped past what are labeled "Unexpected Com Dropouts." Is that because they are not part of the temperature sensor story?

MR. WHITE: When I was coming here today and preparing for this, it was a question to myself whether I should brief from the time line that has every single event in it or I should brief from this more graphical presentation which did leave some of the events out. This particular graphical presentation does not have every single event like some of the com dropouts. To this point we've already had numerous com dropouts that we consider anomalous. We just did not model those in this particular graphical presentation.

MR. HUBBARD: So I guess the follow-up question to that is: Where are the avionics boxes or the antennas or whatever associated with those and can you make any connection between this set of anomalies and the com dropouts?

MR. WHITE: Well, we are trying to do that. We are trying to create an entire picture where we can explain events that would affect everything that we see. So com dropouts would be one of the things that we would try to explain. As for the location of the actual avionics boxes, they're in the avionics bays which are forward in the crew module; and the antennas are in the crew module region, on the top and the bottom of the vehicle both.

MR. HUBBARD: So this is work in process.

MR. WHITE: So they're well forward of this area where we're seeing the heating, but that's not to say whether or not some disturbance in the hot gas flow around the vehicle may or may not create a situation that would cause the com to drop out. We were at fairly good look angles between us and the satellite. So we should have had good communication in this region. We have looked at past flights. So we did have good communication in these regions. So again, that's why we consider some of these com dropouts as anomalous events.

MR. TETRAULT: Have you seen any relationship to the com dropout and the debris event?

MR. WHITE: I'd have to look at the timing that says how close one was to the other, but I don't believe we have been able to link any of those very closely.

MR. HILL: There are debris events that are within seconds of some of the com dropouts. That doesn't necessarily tell you they're related, but there are debris shedding events in this same time frame.

MR. HUBBARD: Okay. So the set of charts here, Rev 15, just looking quickly through those since you're not going to cover these, I see up through Com Event 14. How many of those are there?

MR. WHITE: Well, let's see here. Let me get my other version of the time line. We had at 13:52:09 through 13:52 – well, let's back up. 13:50:00 through 13:50:43, we had five periods of com dropout from one to six seconds each. 13:52:09 through 13:52:55, there were four periods – again from one to six seconds each. That would cover Events 6 through 9. Then again, 13:53:32 through 13:54:22, which would be right in this period here, there were two more periods. One was two seconds. One was 8 seconds. Those would be Com Events 10 and 11. There are some more events, 12 and 13, that are down in the 55, 56 time frame; and Com Event 14 was down at 13:56:55.

MR. HUBBARD: Okay. So can we expect to see some point in the near future a composite plot or a plot like this that shows the antenna wire, the antenna, where the avionics is and so forth and kind of be able to put it together?

MR. WHITE: Well, the scale – we could probably do on a separate page just because of the scale. Yes, we could go ahead and do some kind of a graphical representation of that. Again, we don't see anything anomalous in the behavior of the com system other than com wasn't getting through to the ground. So there may not have been anything physical going on within the Orbiter itself at that location on the vehicle itself.

MR. HUBBARD: It could have been some interference between the Orbiter and receiving stations?

MR. WHITE: Yes, it could have been, again, as I said, some kind of disturbance in the hot gas around the vehicle at that time possibly.

MR. HUBBARD: Okay. Thank you. We'll, I'm sure, be pursuing this further.

GEN. DEAL: I'd like to bring up a question about Dr. Widnall's statement about perhaps earlier debris that was not witnessed. Can you kind of put it in context, when we saw heat onset and also the beginning of peak heating?

MR. WHITE: Let's see here. Let me look at my really detailed time line and the event times for that. The beginning of entry interface, which is about 400,000 feet, is 13:44:09. The start of peak heating is at –

DR. WIDNALL: 50.

MR. WHITE: 50. Okay. Thank you.

GEN. DEAL: The reason I ask that is to underscore her statement. There could have been things that weren't witnessed because you are starting to experience heat before we started seeing –

MR. WHITE: Right. There could have been.

DR. WIDNALL: About the com. I'm very interested in the com. Is that anomalous for the whole range of Shuttle missions, this loss of com?

MR. WHITE: Yes. For this particular period, we have called these losses of com "anomalous events." We've compared them to other flights of *Columbia* on similar trajectories and we believe we should – again, because of the look angles and where we were, we believe we should have had good com in this period.

DR. WIDNALL: So it wasn't just a simple matter of shielding by the vehicle of some antenna? You've already dismissed that possibility?

MR. WHITE: Yes. We've looked at that, and we truly believe there is something anomalous going on here. Now, what it was and how to describe the effect, we're not sure how to do that yet. We're still working on it; but, yes, we do believe that the com dropouts in this period were anomalous.

ADM. TURCOTTE: This is one of the first aerodynamic events that you've indicated here and I'm guessing you're interpolating here roughly we're in the 220s, probably lower Mach 20s. What kind of aerodynamic pressure is the air foil experiencing at this point?

MR. WHITE: Again, I don't have those numbers in front of me. There are versions of this that have –

DR. WIDNALL: Fifty.

MR. WHITE: Thank you. I was going to go look that up in my notes.

ADM. TURCOTTE: If you were to put that in layman's terms, we're looking at, say, around 120 knots or something like that –

ADM. GEHMAN: Less. The QBAR was 29 PSF.

MR. WHITE: Okay. That's pounds per square foot.

ADM. TURCOTTE: Probably roughly 80 knots, something like that.

ADM. GEHMAN: And the Mach is 22.7. So you used PSF?

MR. WHITE: Yeah. QBAR is in pounds per square foot.

ADM. GEHMAN: Yeah, I know that. When you're doing conversion to knots, you use PSF? So something like 75 or 80 knots air speed, something like that.

MR. WHITE: Okay.

ADM. GEHMAN: And we are in a stagnation temperature now of 2850.

MR. WHITE: Yes.

ADM. GEHMAN: So we're peak heating.

MR. WHITE: Yes. Very high heating at this time.

ADM. GEHMAN: I think the point is that there is not 10,000 knots of air flowing past this vehicle.

MR. WHITE: Right. We were at a very low dynamic pressure at this region. Right. Lots of heat but very low dynamic pressure.

ADM. GEHMAN: But things are falling off.

MR. WHITE: That is correct.

Next one, then. This is another temperature. This is on a left main gear strut actuator temperature.

Next slide, please. This is a side wall temperature. This is the left aft fuselage side wall temperature. Now, this particular temperature is about where it's indicated there on the left aft side wall, almost at the end of the wing. This is another indication that something going on externally in the flow above the wing is causing this heating up on the side wall that far aft.

ADM. GEHMAN: Now, would you attribute this more to external heating rather than internal heating?

MR. WHITE: Yes, I would. We have done some calculations, though, that say you could theoretically get enough flow or heating internally to cause this to rise. We have shown, though, that externally, if you were just missing the blankets, you wouldn't have enough heat to cause the temperature to rise. But theoretically it would be possible. We've done some numbers that said you could have had heating from internal. That's also possible.

ADM. GEHMAN: Is this sensor right underneath the blanket –

MR. WHITE: Yes. This is on the skin right under.

ADM. GEHMAN: On the skin right –

MR. WHITE: Underneath the blanket. Yes, sir.

ADM. GEHMAN: Thank you.

MR. WHITE: Next slide. Now, we're back to the left main gear strut actuator temperature. This particular temperature is on a strut when the gear goes down that supports and braces the gear, and again this one saw a rise. Again, you also notice, as you mentioned earlier, there are other sensors in the neighborhood that are still showing nominal at this point.

Next slide. Flash 1. The triangles below line there, this is another debris event. We saw a brightening of the Orbiter image on the video, which occurred where the Orbiter was; and then as the Orbiter moved away, the splash tended to persist in the trail that was showing behind the Orbiter.

Debris No. 6. Next slide, please. Debris No. 6 is the sixth piece of debris that we've been able to observe in the video. This one I used a larger triangle, to indicate that this was a relatively significant piece of debris compared to the other ones. Debris No. 6 and Debris No. 14, from the video that we have, appear to be the largest and brightest debris.

DR. WIDNALL: Could you back up one?

MR. WHITE: Yes.

DR. WIDNALL: Do you have an explanation for Flash No. 1?

MR. HILL: We think Flash No. 1 is attributed to Debris 6 actually separating from the vehicle. We just don't see Debris 6 as a separate object until a few seconds later, but we really do think this is the initial event as that object came off the vehicle, crossed through the plasma wake and shock wake.

ADM. GEHMAN: But we're going to get a chance to talk about that.

DR. WIDNALL: Yes. Tomorrow.

MR. WHITE: Debris No. 6 was right after that. And next slide, please.

Now we start to see some temperatures on the wheels themselves. These temperature measurements are down on the body of the wheel. This is the first one of these. So we're starting to see a little bit of a rise. Again, we noted there was two bits. There was a very small increase in the temperature of the wheel.

Next slide. Debris No. 7. Again, we are over Nevada.

Next slide. All right. Another temperature measurement on the side wall of the wheel well. This is System 3 left-hand forward brake switching valve return line temperature.

Next slide. Debris No. 8. Approaching the Utah border.

Next slide. Debris No. 9.

Next slide. Debris No. 10. These all come off relatively close to each other.

Next slide. Debris No. 11.

ADM. GEHMAN: And you're going Mach 22 at this time with a QBAR of about 35 PSF.

MR. WHITE: Thank you. Next slide, please. This is another temperature on the side wall. This particular one is on the sill, which is actually the top of the wall. It would be underneath the payload bay as the payload bay door comes up and over. This particular temperature would be sitting about right here, just under the door, on the top of the side wall. So we're getting some more heating up there. Again, this leads us to believe that we had something going on with the external flow that was causing higher-than-normal heating above the wing in this region.

ADM. GEHMAN: At this point, the Orbiter is flying with its right wing down, left wing up.

MR. WHITE: Yes.

ADM. GEHMAN: Yes, it is. Hasn't done its roll.

MR. WHITE: Hasn't done the roll reversal, yes.

ADM. GEHMAN: So these are left fuselage measurements here.

MR. WHITE: Yes, they are on the left side.

ADM. GEHMAN: Left side of the body. Is there a hotter side or a cooler side? I know the bottom heating is uniform, but is there any reason aerodynamically or thermally to account for the left side being warmer? In other words, should I read anything into it? Would you expect the left side to be cooler, this particular side, since it's up and away?

MR. WHITE: Well, I think you really need to ask the thermal guys tomorrow.

ADM. GEHMAN: You're right.

MR. WHITE: Generally, from what they've told us, it should be about the same and we believe these rises here were from some off-nominal event causing more heating on the left-hand side. As compared on a normal entry, one roll reversal compared to another roll reversal, I really can't comment on the relative slight differences you might see in temperature.

ADM. GEHMAN: We'll pursue that tomorrow.

MR. WHITE: Next slide, please. This is Debris No. 12; and we're just crossing the Arizona border.

Next slide. Debris No. 13.

Debris No. 14. Next slide. This again is a very large debris relative to the other debris events. So we show the triangles a little bit larger at this time.

ADM. GEHMAN: So it's Debris No. 6 and 14 we want to pay attention to.

MR. WHITE: Right. Paul's going to talk to you about that, about our efforts to track Debris No. 6 and 14 and see if we can figure out a footprint and perhaps recover those debris.

All right. Next slide, please. Now, we lost these five wing temperature measurements early on; and now we are starting to lose some more. This particular one is the left lower wing skin temperature. This measurement is on the lower wing skin itself, right on the bottom side of the vehicle. This one is starting to – this decline. And as you'll notice, these took quite a bit more time to go off line than the previous five that did go off line.

ADM. GEHMAN: Now, these five that went off earlier, I can't tell from the color code whether or not they are in the same –

MR. WHITE: Yes, they are in the same wire bundle as the five that went off.

ADM. GEHMAN: They're in the same wire bundle, but they're not on the same circuit. It kind of shows that they are pink.

MR. WHITE: Well, yes. Each one of these sensors would have its own wire within the wire bundle, yes.

ADM. GEHMAN: So we should not read anything into the fact that there's a difference between these five going and these two here. I mean they're just different wires.

MR. WHITE: Different wires within the same bundle, yes, sir. And, you know, I was talking about twisted shielded pairs earlier. These wires for each one of these sensors is actually, if I remember right, a triplet of wires which is then encased in Kapton and then that particular wire that's formed from the triplet is one wire of many in the larger bundle.

Next slide, please. This is Debris No. 15.

Next slide, please. Now, we have another wheel well temperature. This is a left main gear uplock actuator temperature. This is the actuator that holds the gear in the lock for the gear, locked in the up position; and we're seeing an off-nominal temperature rise there. Also notice that there's another sensor on the side wall. We've colored it orange, which means its temperature rise now has exceeded 15 degrees from what we would consider nominal. So the temperature on the side wall continues to increase.

Next slide, please. Now, there's another skin temperature. This one happens to be the upper wing skin temperature. It's approximately above the one in the lower but on the upper surface of the wing, and this one is starting to go off line. You also notice that the lower one hasn't quite failed all the way completely yet by this point in time.

Next slide, please.

ADM. GEHMAN: Excuse me. Now, what should we read into the fact now that on your cartoon here every sensor on this line here has now failed? Are there other wires in that bundle?

MR. WHITE: There are many other wires in the bundle.

ADM. GEHMAN: In the same bundle?

MR. WHITE: In the same bundle. Yes, sir. These are the only – on that particular bundle, that pink that we indicated in pink there, those are the only ones that we have data for. The other wires in the bundle are either not used anymore because they were development flight instrumentation which we are no longer using or they're a series of instruments that are recorded on what we call our Orbiter experiment recorder, which records measurements and then we dump the tape when we get it to the ground and look at the values for that; but they're not available to us in realtime. One of the things we've been hoping to find in the debris is that recorder to see whether or not any of the tape survived that may give us some of the data to tell us how other measurements in this area were faring at this time and so we can learn more about the event.

ADM. GEHMAN: Would you estimate how many of those sensors there are in there?

MR. WHITE: I went and got the number once for somebody. I do not remember the exact number off the top of my head.

ADM. GEHMAN: Dozens more?

MR. WHITE: It's on the order of a dozen or so.

ADM. GEHMAN: Thank you very much.

MR. WHITE: Next slide, please. Okay. This is Debris No. 16. This is a debris event that was picked up in the Kirtland video, which I'm sure everybody's heard about a video shot by some of the folks at Kirtland Air Force Base; and we were able to see a debris event from that particular video.

Next slide. All right. This is the main landing gear. Back on the tires again and on the wheel. The main landing gear left-hand outboard tire pressure No. 2. It's starting to show a little bit of an increase, only one bit.

Next slide.

ADM. GEHMAN: Could we back up just a second here? I think for the time line we need to determine when the roll reversal was. I think it happens right about 56:55. About 30 seconds ago we did the roll reversal.

MR. WHITE: That's correct.

MR. HILL: We start at 56:30 and finish at –

MR. WHITE: Right. 56:55.

ADM. GEHMAN: So the roll reversal is now complete.

MR. WHITE: Yes. That's the complete of the first roll reversal.

ADM. GEHMAN: Now the left wing is down.

MR. WHITE: Right.

ADM. GEHMAN: People keep telling me that that doesn't make any difference in coordinated flight, but I think it helps to understand.

MR. WHITE: All right. Next slide, please. All right. This is the lower wing skin temperature finally completes its descent down to off-scale low. It did take a little longer than the first five. Again, to us that just indicates that the rate of burning or the rate of shorting of that particular wire was different than the first five – again, possibly indicative that whatever was causing the burning was changing direction or heat rates or something like that.

Next slide, please. And then the upper wing skin temperature follows that shortly.

Next slide, please. Now, we start to see finally the last of the hydraulic measurements in the wheel well start to go up. You can notice some of the other measurements have now turned orange – again, indicating that they are continuing to rise and have gone more than 15 degrees above what we could consider nominal for this particular point in the flight.

Next slide, please. This is what we're calling Flare 1. This is another event that we observed out of the video taken at Kirtland Air Force Base. We see an asymmetrical brightening of the shape. In the video you can see one side of the Orbiter image get brighter than the other side.

DR. WIDNALL: Which side?

MR. WHITE: It appears to us to be the left side.

Next slide, please. Then Flare 2. Again you see another little bit of a flare, again apparently from the left side.

Next slide, please. This is another aerodynamic event that we put in here graphically. This is the start of the sharp aileron trim increase. Remember we've been doing a slow aileron trim increase, trying to keep vehicle flying the way we want it to fly, trying to make it respond. At this point there is some event that happens that causes the aerodynamic forces to require a much greater trim on the aileron and so the trim begins increasing very rapidly here. Again, you'll have some charts tomorrow, when the aerodynamics guys talk, to show you how rapidly that aerodynamic set of forces was increasing.

Next slide, please. We're also seeing an increase now in the derived rolling and yawing moments, those moments I told you that we were able to back out way up early that showed something off nominal. Again, the slopes of these moments are starting to change substantially at this point.

Next slide, please. This is on the tire itself. This is main landing gear left-hand tire pressure No. 1. Again, it's starting to show this damage trend as it's going down. Again, as you mentioned earlier, one of things that's a mystery to us is why the measurements on the tire seem to hang in there for so long whereas other measurements farther back in the wheel well seem to be significantly off nominal by this point in time. Again, it may have something to do with how well those measurements are protected by the tires themselves and the heat sink and the mass of the wheels themselves.

Next slide, please. This is on the other tire. This is main landing gear left-hand inboard tire pressure No. 1. It's showing some damage trends.

Something else I might say at this point too is you watch all these temperature measurements and pressure measurements for the wheels go off line. We saw these in a staggered kind of a fashion, which indicates to us that the tires themselves did not rupture or blow up, at least not at this point in time. That may have happened after our loss of signal, but at this point in time these measurements are going off in a staggered fashion. That says that the tires were still intact at this time.

Next slide, please. Back to the left outboard wire damage trend showing on one of the sensors there. Wheel temp.

Next slide, please. Back to the inboard one. Damage trend there.

Next slide, please. We finally get the landing gear left-hand outboard tire pressure No. 1 to go completely off line.

Next slide, please. Now the left outboard wheel temp goes off line.

Next slide. Now, the landing gear left-hand outboard tire pressure No. 2 starts to go off line.

ADM. GEHMAN: Doug, once again, the people in the audience can't see the companion viewgraph that goes with this that shows the actual temperature sensors.

MR. WHITE: Right.

ADM. GEHMAN: But I'll describe. I'll hold it up, for example. Which one are we on? The left-hand outboard tire pressure. The temperature is normal. There's no rise in temperature, and then the thing drops off.

MR. WHITE: The thing just goes off. Right. The temperature is constant, and then it just drops off. Right.

ADM. GEHMAN: And that's true of all of them.

MR. WHITE: Right. That indicates to us that the tire was intact, that we weren't seeing some sort of a pressure increase in the tire that it was about to rupture and that there was damage to the wire for that measurement that caused it to drop off line.

ADM. GEHMAN: And whatever heat was causing all these temperature sensors to rise, that heat was not present up here and –

MR. WHITE: Well, it was present to some different degree. It was having different effects. Again, since it's difficult to model the propagation of how the heat was getting in there – and we're working on that and it's a difficult thing – but it was obviously having different effects there than it was farther back in the wheel well.

ADM. GEHMAN: Let me rephrase the question. These temperature sensors here are all rising.

MR. WHITE: Yes.

ADM. GEHMAN: These temperature sensors here, there's no temperature rise in any of those sensors. They just drop off.

MR. WHITE: They just drop off, right, which says the wires were getting damaged.

ADM. GEHMAN: I understand neither you nor I can figure out why that happened, but these temperatures are rising and some of them have now gone orange, indicating that the rate of the rise is now alarming, whereas these don't show any rises whatsoever.

MR. WHITE: That's correct.

DR. WIDNALL: Where is the cable located for those wires, the blue ones?

MR. WHITE: The ones on the wheels themselves, the lines run on the back of the gear, on the back of the strut and they run up the strut.

DR. WIDNALL: Can you show it?

MR. WHITE: They run along the strut here. They come up to the back of the wheel well. They come to actually a kind of a junction box here and they run across the ceiling to the front of the wheel well and then they run out through a connector into the mid body about there.

DR. WIDNALL: So they're inside the wheel well structure?

MR. WHITE: Yes, they are inside the wheel well structure.

DR. WIDNALL: And at least over part of the area, they're mounted on the front bulkhead.

MR. WHITE: Yes.

ADM. GEHMAN: But I think Sheila's point is very pertinent because even though these sensors did not show any temperature rises, the wire that feeds these temperature goes all the way back into this region?

MR. WHITE: Yes.

ADM. GEHMAN: And then comes back out of that region again because of the way the landing gear was folded back over on itself.

MR. WHITE: Yes. And if you want to surmise that maybe we're just today burning through wires here, you would want to think that it was down closer to the sensors themselves on the strut because there are other temperature measurements again that are coming in this bundle across the top of the wheel well and then out through that connector that are still reading and acting just fine. So some kind of burning was going on there. It was most likely down on the strut next to the wheels themselves rather than up on the ceiling of the wheel well.

ADM. GEHMAN: Thank you.

MR. WHITE: Next slide. This is main landing gear left-hand inboard tire pressure No. 1 has gone off line.

Next slide. This is main landing gear left-hand inboard tire pressure. Again it's showing a very slight increase in tire pressure. A 3 1/2 pressure rise in two seconds. That didn't last very long because that sensor went off line shortly thereafter.

Next slide. You see right there in the next slide it started to go off line and that measurement started to trend down.

Next slide, please. Another main wheel well temperature that went off line.

Next slide. Then the next-to-the-last one went off line.

Next slide. Then finally the last one. So all of our sensors, both temperature and pressure on the wheels, have gone off; but again since it was a staggered fashion, we don't believe that one or the other of the wheels let loose, which would have lost all of them simultaneously.

Next slide, please. This particular measurement, the change here, this is called the left main gear downlocked. This is a sensor which tells us that the gear would be down and locked. This particular sensor changed to a 1 state, which is an off-nominal reading for this state. We did do some wire testing to see how this particular sensor would fail if its wire was burned through. It would fail to a 1 state. So this could be either real, that said that maybe the gear did come down at this point and we got a 1 because we were suppose to, or it could be just that the wire had burned through. The other sensors in the wheel well, you can see the other three red squares there, they were still all reading their nominal values, which told us that the door was up and locked. We have three other sensors. We have the door up, a gear up, and a no weight on wheels; and all of those were reading their nominal values. However, from testing that we did from wire burning to see how those would fail, those could fail in their nominal state if their wires were burned through. So it is possible that those wires were already failed but we didn't know it. It's also possible they were reading exactly the way they should have because the door was still up and locked at this time.

ADM. TURCOTTE: Is this the same location of the previous tire pressure wire bundle that you described before and that is located along the center line of the gear?

MR. WHITE: Right. This particular one is along the strut. Now, the one that you see very forward there, that particular wire bundle runs all by itself across the front of the wheel well and up to that connector. It's not in the same bundle until very late with this particular one that's failed here. So that's a separate bundle, but the three on the gear there are all in the same bundle.

ADM. TURCOTTE: So that's the one that's located on the trunion assembly by the dust cover where it goes through into the wing?

MR. WHITE: This particular one is on the strut itself, but the wires then run as you described back into the mid body there across the top.

Next slide, please. Right. This is sensors starting to go off line, one of the ones that had been reading temperatures, system 2 left-hand aft brake switching valve return temperature, starting to go off line.

Next slide, please. Now, this other wire that goes to the ASSA that was the gray wire that actually looks kind of purplish here, this is starting to show that it was burning through somewhere and shorting. We have evidence that our air surface servo amplifier was shorting out and was not providing power the way it should have to Channel No. 4 for the elevon actuators, but the inboard and the outboard we begin to see off-nominal events and in the detailed time line there are quite a few off-nominal events. This is right before LOS or one second before we lost signal here, but this does indicate to us a sequence of events that I just labeled with this one event here, that we were burning through this power wire, causing shorting to go on in that air surface servo amplifier. What we also see from the data here at this point is that the other three channels were taking over and the redundancy management that's built into the system was working the way it was supposed to be working. The other three channels took over and were in control even though this system was failing.

Next slide, please. This is just prior to loss of signal. You can see all the things off line.

MR. HUBBARD: Doug, before you get to that loss of signal. If you were to come up with some kind of a metric of event as a function of time and you plot that from the beginning to this point, do you imagine that that's linear or is there some knee in the curve? Is there some point in this nine minutes or so here where things pick up?

MR. WHITE: Yes. I would call the knee in the curve the place where we showed the start of the sharp aileron trim increase, which is back up with one of those triangles there on the top. The vehicle was in control and was responding to commands up to that point, and after that point something changed apparently and it still continued to be in control and still continued to respond to commands but the rates and the amount of muscle it needed to continue flying the vehicle the way it should be flown was continuing to increase. Something definitely happened at that point. Again, we don't know what; but something definitely happened at that point to cause the flight control system to need more muscle and start to have to fight harder to control the vehicle.

MR. HUBBARD: And that was at about?

DR. WIDNALL: I think that's about 57.

MR. WHITE: Yeah. That would be about right.

DR. WIDNALL: I guess the comment I would make – because I have looked at that particular instance of time – that really coincides with a rather sharp increase in the rate of rise of dynamic pressure.

MR. WHITE: Yes, it does.

ADM. GEHMAN: Okay. Thank you.

MR. WHITE: Right. That's as far as I planned to brief in these charts. As you know, there is some data that we recovered from the satellites post-LOS. If you want to talk about that, I can answer questions about that; but I don't have any more charts.

ADM. GEHMAN: Okay. Let's let Paul have the floor for a few minutes and then questions.

MR. HILL: Okay. Now, as I mentioned before, what my team has been doing is evaluating various public imagery, various external sensors and trying to make some sense out of the data and see if we can get smarter about what's coming off the vehicle earlier on as far west as we can, as well as get some engineering data to tell us specifically what those objects are and where they're going.

I don't really have prepared presentation charts. I'm going to wander through some discussion on this map. I have a few other pictures I'm going to show you, and I did bring a composite video that shows examples of continuous video from the California coast through about mid New Mexico. Since this video was put together, we have added one that takes us about 50 miles offshore California and we have some video from Kirtland Air Force Base that takes us through just about the New Mexico, Texas border. Those aren't going to be on this tape that we're going to see here in a few minutes.

Let me start with the process, then we'll play the tape. To give you an idea, when we first starting getting these videos, our first job really was to put them in chronological order. That's still photographs, video, et cetera. We very quickly focused on just the video and saved a lot of the still photography analysis for later.

Our first goal is to establish some absolute reference for time in each one of the videos. Once we have that, we can put them in chronological order. As we were going through that process, probably three or four days after the accident, we first saw in these videos individual debris shedding events; and that was our first indication that something, in fact, was coming off the vehicle early on, that we didn't just start having structural damage, say, over west or east Texas. You'll see, as we play the tape, some of the things that we use for cues in establishing time and establishing relative geometry. There are a couple of celestial references in a couple of the tapes. You'll see a star. You'll see Venus crossing, which will be very clear. At least half the photographers snapped their GPS location so we know exactly where they were standing. In the case of the Venus crossing, because we know where that photographer was standing and we see the Orbiter actually flying in front of Venus, we can calculate when in time that had to have happened. So now we can put that tape exactly where it was in time and we know exactly where the Orbiter was in space and then we can sync the videos that preceded that one and the ones that followed to that tape. We had a few other cues like that in other tapes, and I'll try to describe those as we go when we play the tape.

As we started seeing these debris shedding events – and you'll see these in the tape, although some of them you do have to look closely because they only last in the order of a second or second and a half in cases, we then set about calculating the exact times that the debris was coming off the vehicle. As we established those exact times, we went to work, trying to do relative motion and ballistic analysis. I'll come back and talk about that here in a few minutes.

Interestingly, not only was NASA not aware that debris was coming off that early before we looked at this video but most of these photographers did not see any debris shedding in their own photography until they heard about the accident on the radio or on TV and went back and played back their video. Then they could see them. Like I said, in most cases debris flash or the speck that you see in the video lasted for a second and a half or so, in most cases less than a second.

The types of things to look for in the video. In some cases there's flashes, like Doug talked about. In other cases you can see a bright dot which is Orbiter and plasma wake behind the Orbiter, and then you'll see another dot come from a dot. And you'll see when we play the video we are not seeing images of an Orbiter against a dark sky where we can clearly make out the planform and shape of the spacecraft where we can clearly resolve down and see where some object is coming off the vehicle. We see a dot, we see another dot appear from that dot, and one of the dot goes away. And we will talk about that some more as the video plays.

The other thing to think about as we watch the video is we are making some speculations about what we are seeing. We think that the brighter objects are more massive, are more significant, potentially higher ballistics numbers. Certainly the things that the individual light for the individual pieces of debris persists longer, we expect that those objects are more massive, higher ballistic number because we think that the reason they persist longer is they are moving faster. So they stay lit. They have their own plasma wake, longer than, say, some lighter thing, say, an individual tile comes off versus maybe some other heavier object. But I'll also say we cannot just look at these videos and just determine what is it that's coming off the vehicle. Are we losing a tile here? Are we losing some section of the thermal blanket that's on part of the external surface of the vehicle? We can't tell that, and to this day with the good data that we have on the ballistic motion and the ballistic analysis and the footprints, we still cannot say exactly what it is we see coming off. We are making some judgments on which of them are more significant or more massive than the others. And we talked about Debris 6 and Debris 14. When we play the video, you'll see why we're focusing on those.

So why don't we go ahead and play the video and then we'll come back and I'll talk some more about what we've done on trajectory analysis.

ADM. GEHMAN: You can feel free to stand up and narrate or point. However, you feel comfortable showing us what happened.

MR. HILL: This is just after the California coast. As I mentioned, you see a dot. That's the Orbiter. And the view looks more or less like this as we change the vantage point. We'll start picking up the con trail.

Now, if you blinked you missed that, that was Debris 1 and that was Debris 2. Those little dots that came off, that was debris. As I mentioned, you can't make out the planform, you really can't see the Orbiter, and you have no idea what's coming off. Also, as I mentioned, on some of these or most of these, the debris itself doesn't last very long at all.

ADM. GEHMAN: Now, this is a significant event.

MR. HILL: Yes. Now this bright dot you see here, this is Venus. When our flight dynamics folks saw this, they were very excited because this allowed us to put this video within plus or minus a second of where it actually happened.

Now, you can see the flash persist in the wake and then you see Debris 6 come off. Even though they're separated by a few seconds there, our speculation is the flash was some burning event associated with Debris 6 and then that object coming off the Orbiter.

ADM. GEHMAN: If I understand it, Debris No. 6 is the one you tracked to the vicinity of Caliente, Nevada, and we are valiantly trying to find.

MR. HILL: We do think that is Debris 6, and I'm going to show the footprints for that and explain that a little bit more.

There you saw Debris 7 come off. Now, again, also just for a reference, all of these are taken with camcorders. These are commercial camcorders. This is somebody in the public, standing outside with a camcorder, generally zoomed way in, trying to track the Orbiter flying overhead at 12,000 miles per hour by hand.

ADM. GEHMAN: You recommend people pay attention to Debris No. 14. That's the other one.

MR. HILL: Now, as we come up on Debris 14, the thing to think how is bright that flash was before Debris 6. Compare that to what Debris 14 looks like. Also, for comparison, Debris 6 was lit from between 6 and 12 seconds.

Now, there you saw how bright that was and also you saw that you have this cloud where around the Orbiter, the video itself or the pixels became saturated. That is the most bright – the brightest object that we saw in any of the video. And I'm going to come back and talk about its relative motion and Debris 6's relative motion here in a few minutes.

You can see here we're getting further east. We're getting out over New Mexico. The sky is lightning up, which makes it more and more difficult in the videos that we have out there to track the Orbiter and specifically to pick out individual debris shedding events.

ADM. GEHMAN: But in your experience and the experience of the experts, that hot gas envelope right there looks just like any other entry that you know about?

MR. HILL: That's right. Except for any of the flaring or flashes or anything else, the bright spot you see there looks like just all the other videos that we have. As a matter of fact, one of the photographers that sent us this video sent us six previous entry videos that he took, most of which with the same camera, and looked just like this except absolutely no flares, no dots coming off.

ADM. GEHMAN: The number down in the right-hand corner is what's on the camcorder, but that's not calibrated time. Your times are in the bottom left-hand corner.

MR. HILL: That's right. Now, we have done a fair amount of work. Again, about half of these photographers were amateur astronomers and they had synced their clocks themselves to atomic clocks. Some of them went back and taped the atomic clock so that we could do our own calibration, and some of them did some of that afterwards.

Now, the things you're seeing here are just prior to or including the main breakup.

ADM. GEHMAN: But this is post loss of signal.

MR. HILL: Correct. We left this in here for completeness. We're going to talk a little bit about post-breakup and pre-breakup trajectory analysis. I thought we would go ahead and run the tape through this to give us a place to start from. These videos were all taken from Texas, of course.

This was taken from an Apache helicopter, looking through its forward-looking IR targeting sensor. Now, the thing to think about here – we'll come back and talk about this in a while – is the significant number of secondary and tertiary breakups that you see in these videos. That will be important when we talk trajectory analysis.

DR. WIDNALL: Can I ask a question? Are there any gaps in time missing, where you don't have video? Is there a continuous time line between the first sighting and these later pictures? Are you missing anything?

MR. HILL: There is a small gap in the East Texas or the East New Mexico, West Texas area. It is not as big as represented on this tape.

DR. WIDNALL: How long is it? A minute?

MR. HILL: I would say it's on the order of a minute or two minutes. Everything else west of Albuquerque, we have near-continuous video for. Now, it shifts around from vantage point to vantage point and there are dropouts in individual video. As a matter of fact, if you segue into the map here for a few minutes, the blue dots that you can see on the map, those represent where the individual photographers were standing. If you take this one, for example, here, this is in Flagstaff. This blue line extending out this way, there's another that extends out this way on the map, that wedge represents the full part of the trajectory that that photographer filmed in his camcorder. It doesn't necessarily mean that that photographer has continuous coverage of the Orbiter for that full swath because many of them dropped track, lost the Orbiter. They'd look away from the view finder. The camera came down, and they had to go find it again. But for the most part, with all of the overlapping video we have from California all the way through New Mexico, we've been able to piece together essentially continuous views of the Orbiter.

Now, the other important thing is on some of these objects when we see them coming off the Orbiter in one view, we may not see that same object coming off for another second or so in another view. In some cases we don't see it from a different vantage point of the same incident. Some of that is because one observer, say, may be looking from the north side of the trajectory and the folks down here are videoing from the south and one of them may have the Orbiter itself maybe obscuring the view of, say, the flash or the individual debris coming off. Since that debris only persisted for maybe a second in most videos, it wouldn't take much obscuration at all for one video not to see it. The short answer is we have near-continuous video until right about here, and that's east of Albuquerque, New Mexico, and there's this gap and we pick up with that Texas video of the main breakup. South of Dallas.

DR. WIDNALL: You have a gap between Albuquerque and –

MR. HILL: Albuquerque and about the Dallas area, which I guess you would expect because of the relative population. Most of the video we have, even out here in Arizona and New Mexico, which is relatively thinly populated, most of that we have from Albuquerque, from Flagstaff and from Las Vegas. And the one from Flagstaff in particular, they tracked for a significant period of time, from horizon to horizon. So that's our explanation for the gap there.

Now, going back to the video a little bit, you see the type of relative motion or the type of relative distances you see in the objects that come off the Orbiter. We're able to zoom in on those objects. We're able to zoom in on the Orbiter. The imagery folks here at JSC are able to take all that jitter out so that there's no motion except for the relative motion between the object and the Orbiter. We can then measure how that object moves away from the Orbiter; and since we know exactly where the Orbiter is in space relative to the photographer and we know exactly what the timing is, we can calculate the ballistic number of that object, based on how it moves relative to the Orbiter, because we know the Orbiter's ballistic number, of course. We then take that ballistic number for the object and we propagate that down and build a vector so that we can propagate the object forward all the way down to the ground. Then we generate a series of footprints at 80,000 and 35,000 feet and ground impact.

If we can put up page 2 of my charts. We've done a couple of things. What you see here is a very generic footprint. We started with this. Before you could calculate relative motion and ballistics off the video, we made some simple assumptions like we were shedding a tile every two seconds from California all the way to Texas. Based on the known ballistic properties of the tile, that gives us a debris swath that looks like this, which is still enormous; and it's about 30 miles above, 30 miles below the ground track for that full distance. That's what we knew very quickly, within a day or so of the accident.

If we move on to the next page, a similar footprint based on the main body breakup, also based on various simplified assumptions on ballistic numbers, both the light and heavy objects. This footprint is for the debris field in East Texas; and it, in fact, is centered right over the debris in East Texas. On the far right side down in the lower corner, that's near Fort Polk, Louisiana, which, in fact, is where main engine components have been found. Now, again, these are both very generic and they're based on relatively wide simplified assumptions.

If we go to the next page, this is based on Debris 6. This is that object that we see coming off somewhere near the Nevada, California border. In fact, this footprint, this blue line here, that's the Nevada, Utah state line. This small box you see here, if we exactly nailed the debris shedding time, if we exactly nailed our ballistic analysis, that's where you would expect that object to be laying, if it also didn't generate any lift.

We've done a bunch of other detailed analysis. If you go to the next page, just for comparison sake, depending on the errors that we had, it is just as likely that the object, instead of landing in that no-lifting box here in the middle, could have drifted off track to the north, off track to the south, just by generating lift. If we had some error in the time that we calculate in that object coming off or in our ballistic analysis, then it could also fall short up here in this part of the footprint or along down here.

Could we back up a page, please. Now, this is Debris 6. This is the first one we had analysis on. We were able to get analysis completed on this one earlier because we had that Venus crossing and we really knew the relative motion of this one much better than we knew everything else.

After we built the footprint, then the process would be go through the FAA radar data which we have saved off and recorded; and we're working with the NTSB for them to search that radar data to find patterns that would not normally be noticed by air traffic controllers. In that process we have found a thread up here in this area which is just inside Nevada before crossing into Utah and another one down here just south and then another one over here in Utah near Mount Zion National Park. These are the first three radar threads that we found; and, in fact, these are the three areas that we have been trying to search here for about a month now.

The one in Utah is very mountainous terrain and is most likely only going to be searched by air. It has been searched already by air. We're talking about doing some more air search. This one up here in Nevada which is near a place called Caliente, Nevada, we have had folks on the ground there, searching. It's also snowed out there about five times, to the tune of 4 to 5 inches of snow each, since February 1st, which certainly our problem of searching and finding things.

We also say again we don't know what this object was. We know that, based on its relative motion, it has a ballistic number on the order of 3.75 to 4.75, which compared to the Orbiter ballistic number, which is on the order of 100 to 110, makes it something that's relatively small and light.

Like you said, Admiral, we expect this object to be Debris 6. I mean, the objects that we're finding the radar threads for, we expect it to be Debris 6 because it lies right in this Debris 6 footprint and so close to the no-lift in the box. We don't know for a fact that it is because, as Dr. Ailor said, as these things come off the vehicle, they could continue to fail, break into smaller pieces, which then could completely changes their ballistic properties. Our general process is the same, though. We calculate relative motion, calculate ballistics, propagate out this footprint, and then we search the footprint for radar threads.

If we go to the next page again, this is Debris 14. That is that second object that was so bright compared to Debris 6. Let me correct something that I told you on Thursday.

Debris 6, you can see, persists, depending on the video you look at, for between 6 and 12 seconds. Debris 14, we see, persists for 4 1/2 to 7 1/2 seconds, depending on the video you look at; but Debris 14 is also much, much brighter than any other object, including Debris 6.

How do you interpret that? We're not sure. We do think that relative brightness is an indicator of something that's larger and more massive. We think that the amount of time that individual flares or the light around that debris persists is also indicative of the larger ballistic numbers, which tells you you're dealing with something that's probably larger and heavier. That's as much as we know. We know how these things behave ballistically way up high when there's not a lot of air.

In addition to just searching these footprints for FAA radar, we've also moved all the way out west to the west coast of California and we are searching all air traffic control radar anywhere it intersects our ground track or that wide generic swath around the ground track to again see if we see any patterns of *Columbia* debris falling through that radar that would have been ignored by air traffic control. To date, we still have not found any threads out there; and as you know, we have not found any of the *Columbia* debris laying on the ground out west, based on these threads.

Now, searching the radar data bases is relatively labor intensive. Clearly, putting people on the ground out there to search even 5 square miles is labor intensive. We have since started testing various Shuttle components up at Wright Patterson Air Force Base at the Air Force research lab.

Our initial focus was on that Flight Day 2 object and to try to determine what we could do to identify what may have fallen off the Orbiter or fallen out of the Orbiter – if, in fact, that's what that object is attributable to. So for those radars, we specified a list of thermal protection system, predominantly a couple of different of types of tiles, a couple of different types of blanket type insulation that's on the outside of the Orbiter. We're also going to send up an RCC panel, a carrier plate, and the horse collar, that thermal seal that goes around the carrier plate. Those are all in work right now. And we sent up some different types of thermal insulation that go in the payload bay.

Once we had that in work, it occurred to us we could do similar type radar testing also at Wright Pat that is tuned towards the radars, these air traffic control radars, that we are looking for our debris falling down through. And that also is in work. For many of those materials, that testing, too, has already been completed and we are expecting detailed results sometime this week.

By the same token, we are looking to identify a set of SRB components and ET components and we'll have the full set tested for the C band radars we track their ascent, UHF radars we track while they're in orbit, and then the L band air traffic control radars that would drop debris down through the air. All of that is supposed tell us is it reasonable to expect that we could track the materials that are most likely to come off the Orbiter or, to look at it another way, how big would those materials have to be. So would we have to have a tile the size of a car to be able to track out here, or is it reasonable to think we could track a single tile or piece of tile? I expect that we'll have information on that here within the week.

That gives you an idea how we think we're going to find any of this debris. Also, as Dr. Ailor said, the key to finding or looking for this debris is we know what happened more or less in East Texas, at least at the gross level. It will be difficult for us to do trajectory work with the debris we find in East Texas and back it up to the vehicle and try to determine what was happening over Texas. This debris could tell us where the breach started; and if we can locate some of this and use it to isolate where the breach on the outside of the vehicle started, that's going to make us immensely smarter on exactly how the failure started in the first place.

Now, at the same time there are some folks out at Ames Research Center in California that are capable of analyzing the spectral data, the luminosity in the video and the still photography, and it's possible they'll be able to get us some engineering data on exactly what's burning, exactly what they see coming off in the plasma wake. Probably the easier of the two analyses will be looking at the relative luminosity, and it is possible that by looking at and measuring the luminosity of the debris in video, comparing that to the Orbiter's luminosity where the Orbiter is not saturating the video, we know what the Orbiter's instantaneous drag is, we can use a ratio of that drag and the luminosity, compare that to the debris, and it's possible we'll be able to estimate the actual drag on the debris, which then makes us smarter about what's coming off.

Our initial hope was to also get good enough spectral data to resolve down the actual material. Unfortunately, we expect that the three colors we can get from commercial camcorders will not be good enough. In combination with the distances they were shot through, the fact that a lot of this light was having to go through both the Orbiter plasma wake as well as some plasma wake around the debris, our hopes are much lower that we'll get good spectral data, but we're setting up feasibility tests for both of those out at Ames Research Center and we expect to have those tests set up and in the works sometime during the very near future.

The last thing I'll tell you about is the miscellaneous sensor analysis we have in the works. Again, the first one is something that we were originally very hopeful and we are much less hopeful now. And that will be the infrasonic analysis or infrasonic data. There are various type of microphones that are set up across the continental United States and out in Hawaii. They did measure sound data on the Orbiter during this entry. They have similar data on previous entries. We thought that it would be possible potentially to bring some of that sensor data back to the Orbiter ground track and essentially give us a calibration or a signature of these debris shedding events as they occurred across the ground track. We have since found that the various variables associated with bringing that data back to our ground track and back to our place and time in the sky are probably going to be large enough that we're not going to be able to do that. So we expect that not to pan out.

We have various other DOD sensor data like radar, and then there are other types of data like that that we also have evaluated and we have put on the time line. You have seen some of those. Most of that data also, regardless of the type of sensor, is not good enough to specify, say, engineering properties or specify any kind of properties on any individual tracked object, unfortunately. We had originally hoped that we would be able to track individual pieces of debris coming off the Orbiter, specify the vectors on those things, and use those to be smarter to get them all the way to the ground. And across the board, the types of sensor data, the external sensor data that we have is not going to be good enough to do that and, interestingly, the public video we have is probably the best data we have to try to find some of this debris out west.

The last thing I guess I could tell you. On the ground track here, without going into a lot of detail, I mentioned these blue dots are the photographers. The white dots you see on the ground track, each one of those is an individual debris shedding event. If you stand back and just kind of look at the view from 10,000 feet, you can see that from California pretty much all the way to Texas you see a relatively steady stream of objects coming off the Orbiter. Now, there's a few places where you don't see as much. That doesn't really necessarily mean we don't have small pieces of debris continuing to come off the vehicle. It could just be the perspective and point of view during that phase of flight and the photographers just couldn't see it. Likewise, you don't see any of these white dots out here where we don't have video because we don't have any way of seeing it, but I think it would be valid assumption that we are continuing to drop debris all the way through. And it is likely that if we had video during this time frame, because we had a lit sky, we wouldn't have seen individual objects coming off unless they were relatively large and we saw some bright flare.

MR. WALLACE: Paul, why don't you show with your pointer there where this west piece of debris was found.

MR. HILL: Let's see. The westernmost piece of debris was found just south of Lubbock, which I would say is right around in here. Let me also say for that westernmost piece of debris, that Littlefield tile, which is generally how we refer to it, we have done some top-level trajectory analysis on that. We expect that piece of tile came off somewhere in this time frame here, potentially while we had video from Kirtland Air Force Base, but that also is based on the mass properties and size of that tile in its state on the ground. Of course, it was part of a larger piece higher up in the air and it probably also came off much earlier than that.

ADM. GEHMAN: That trajectory analysis you just spoke of, that does include true winds of the day.

MR. HILL: Yes, sir, it does. Now, what doesn't include true winds of the day is that generic swath you saw from California all the way to Texas, although we are in process of putting real winds of the day in that.

Let me go back up a page in my slides, please. Now, I don't show the radar threads here but, again, I mentioned here around this band there is a radar thread, probably the radar thread we were most interested in that we followed, where radar thread is just the long string of radar hits that we followed in this pattern on air traffic control radar that we think is a attributable to Debris 6 or some piece of Debris 6. Now, that radar thread started right about here. Again, right on the ground track, right where you would expect a non-lifting object to be, and then it tracked to the north and east, which also was with the prevailing winds of the day. So our interpretation of that is, as that object dropped down into the heavier air where you would acquire it on air traffic control radar, which is about 80,000 feet, then it fell ballistically above that, got down into heavier the air, started becoming more lifty, started wafting with the winds, and again then started tracking here in the north and east as it came down lower. If you look at the topographical map of that radar site and where that object lost track, our speculation there is that we tracked that object to within about a thousand feet of the ground, which is why we think we have about a 5-square-mile search area for that object out west.

That was everything I was going to tell you in a big picture and how we're doing what we're doing. In general, we're continuing with the relative motion analysis on all these objects. I expect here in the next couple of weeks we'll have ballistic footprints rolling in at a relatively regular rate, starting with Debris 1 and 2 out west and then working its way east. We also expect that we're going to see those footprints start to stack up and overlap significantly with Debris 6 and Debris 14, and then we're working on figuring out from those overlaps how to come up with concentrated search areas based on where we think it's most likely we'll find any and all of this debris out west.

MR. WALLACE: So this piece that you tracked to a thousand feet above the ground, there's no question that that arrived at the ground; but is there a question about a lot of this other debris is likely to have just been burned up?

MR. HILL: Let's see. The debris we have radar threads for, any one we have radar threads for, if you assume that those are our debris – which is still somewhat of an assumption – then we are relatively confident that those are on the ground somewhere near where we lose track of those objects. Now, the other things we see in video very likely could have either burned or completely disintegrated from G loads or aerodynamic forces before they got to the ground. We don't know.

MR. WALLACE: When we say 1 through 14, can you say how many of those came up on radar.

MR. HILL: Well, I can answer a different way. We have about four key radar threads that we are searching out west. There's these three here that are in the Debris 6 footprint, and there's another one in Albuquerque that did not come from this analysis. It was started based on some folks in Albuquerque who thought they heard something fall through the sky and impact the ground, and NTSB found those radar threads. Now, if you assume those are ours, we are reasonably confident that those things are on the ground somewhere. All the rest of these that we don't have any radar threads for yet may or may not have made it to the ground. We have just now started searching the Debris 14 footprint for radar threads. So we could go another one to two weeks before we finish searching all of that radar to determine whether or not we see these.

MR. WALLACE: In how much of the area that you're searching are you dealing with snow-covered ground?

MR. HILL: All of these areas out west, certainly in the Nevada and Utah area, have been snow covered off and on at least four or five times since February 1st. As a matter of fact, the primary search box out there in Caliente, Nevada, was on hold and we had about 15 percent of that area to finish searching and it's been like that for two weeks, maybe going on three weeks now, all because it was snow covered. If you're looking for something small like a piece of a tile, it's reasonable to assume they're not going to find that on snow-covered ground.

ADM. GEHMAN: What can you say about still photography? Has that been of any value?

MR. HILL: We're doing some work with still photography. There is photography that was taken from California, in particular, time-lapse photography that may yield us the best spectral data. It did give us a few more cues when we were trying to narrow down maybe one or two seconds on debris shedding timing. I don't expect we're going to get a whole lot smarter from the still photography than that, however; but we are buying still cameras from many of the photographers, just like we are on the camcorders so we can try to calibrate what we're seeing in the film and get a better idea of what kind of spectral data we can pull out. In the ideal case, we'll be able to take some of that still photography and clearly show that we have aluminum burning in the plasma or maybe silica or maybe RCC. We'll see.

ADM. GEHMAN: Did you want to talk about the Kirtland photographs?

MR. WHITE: I can talk about that a little bit. I've been working on a tiger team to try to understand the images there. There were a number of images acquired at Kirtland by the folks there who were doing that on their own time, not using the Starfire Optical Range equipment. They did have some pretty sophisticated home-built stuff, but it wasn't the Kirtland Starfire equipment.

They did manage to get four videos and three stills. I think some of those have been in the media already. We are trying a number of ways to deconvolute those photos to try to make them as precise as possible to see what sort of images we can get off of them at that time. There do appear to be some irregularities in the shape that we see from the still. We have to still run that down and find out, you know, what exactly the shock wave field should have looked like from that point of view around the Orbiter at that time, whether or not we would have expected to see it look like that, whether we would have expected to see it be different. As you know, we've already shed quite a few pieces of debris by the time we got there. We were also able to pull one more piece of debris out of the Kirtland video in the two flares that I talked about.

MR. HILL: Let me put that another way, Admiral. We are capable – using the same techniques we used for measuring relative motion from the video, we are capable of drawing pictures of exactly what the Orbiter should have looked like to its Kirtland photographers, whether it's for their still photo or for their video. We're then capable of using computational fluid dynamics and projecting what the flow field should look like around those pictures and then we're also capable of taking that and handing it off to plasma physicists like at the Ames Research Center and generating what the plasma wake should have looked like around those still images. Then we can compare those against what's in video and what's in that still photo.

I would caution anybody in reading anything into either the video or the still photograph until we've gone through that process. The vast majority of people that have studied those images are imagery experts. They're not experts at what the Orbiter looks like during entry, the flow field around the Orbiter, the plasma dynamics or anything like that; and we're definitely premature trying to read engineering conclusions into any of those images before we've gone through that process.

ADM. GEHMAN: Thank you very much.

Members of the Board here.

DR. WIDNALL: I want to make sure I understood something that you said. I asked you about whether there was a time gap in the coverage. You said there was – basically you don't have any video pretty much between Kirtland and the more spectacular big events.

MR. HILL: That's correct.

DR. WIDNALL: You said that you thought there you expected that during this time gap that there probably was continual debris shedding but that we just didn't have pictures of.

MR. HILL: I think it's reasonable to assume that.

DR. WIDNALL: But it also might be possible that there was, in fact, a catastrophic event such as losing a wing or something like that.

MR. HILL: While I can't say that technically –

DR. WIDNALL: But it can't be ruled out on the basis of the data that you have.

MR. HILL: It would definitely surprise me personally that we would have something significant like loss of a wing that is not covered in the later video that we have of the main body breakup, based on what we have in telemetry and we know how the vehicle was flying and we know the sensor data that we have. My personal expectation is we capture that in the video, just based on what we see in the time line.

DR. WIDNALL: So where is loss of signal relative to the gap that you have in the video?

MR. WHITE: Loss of signal is over Texas. So we have data from the vehicle.

DR. WIDNALL: You're saying you have data from the vehicle that covers this region in time –

MR. WHITE: Yes, we do.

DR. WIDNALL: – where you don't have video.

MR. HILL: That's right. These red dots you see here, all of these represent actual GPS vector measurements.

DR. WIDNALL: So you do have data during that period.

MR. WHITE: Right. We do have data through that video gap period. So, yeah, it's highly unlikely that any large piece of the Orbiter like a wing would have come off, because we still have data from all of our systems that show that, even though they were failing, they were still there.

MR. HILL: Another way of saying that, if you look at the map, is these blue lines show you everywhere we had video. Everywhere where a line is red on the ground track, we had data coming down from the Orbiter. Then where it's yellow is the LOS time frame.

DR. WIDNALL: Okay. Fine. Then the second question really concerns this Debris 6 and the flash. As I understand the observations that were made in California of the flash, the flash was unusually persistent and it also was stationary in the atmosphere. So the question is: What is it? What do you think it is? Do you think it is aluminum burning in the atmosphere?

MR. HILL: It is possible that it is something that burned and came off the vehicle. It is what you would expect to see if we were to, say, vent a fluid or if we were to burn something and as we gave off combustion products, significant combustion products, not something on the order of, say, one of our reaction control system jets, but if we were actually burning something substantial and as we put that out in the plasma wake, you expect because that would have relatively no mass, certainly compared to an object, that those combustion products would immediately go essentially static compared to the Orbiter or compared to what we consider normal ballistic behavior for an object that has significant mass. So it is reasonable to assume that something came off that was very light or that that was some kind of combustion product like potentially aluminum slag that also was burning as it came off the Orbiter and then went stationary there in the wake.

DR. WIDNALL: So are you ruling out the possibility that there could be a chemical reaction that was stationary? In other words, are you assuming that as soon as it was all by itself in the atmosphere, it was not reacting.

MR. HILL: I'm not assuming that at all.

DR. WIDNALL: That's what your words seem to indicate.

MR. HILL: All I'm trying to say is it is difficult, if not impossible, for us to get much more specific about what we're seeing technically other than we see this bright thing come off the Orbiter and there are a handful of things that that could lead you to believe as to what those objects are or what the phenomena are, like a flash or the persistence of that flash. I agree with you that the persistence of that flash certainly indicates that you either have continued plasma wake around something or some continued reaction. The fact that it becomes more or less stationary would also suggest that it is something that is extremely light, probably more like a cloud or a combustion product.

DR. WIDNALL: Okay. I just want to make sure that we're talking the same language.

ADM. GEHMAN: But the best that you've been able to analyze so far is that flash that precedes Debris Shedding No. 6 is not merely a disturbance in the hot air. It's not just a wave or of the hot air or hot gases around the Orbiter.

MR. HILL: Probably not. Just due to its persistence, it is telling you that it is more than just something crossing through the wing. Something else is happening there.

MR. WALLACE: A question on the far end of the time line. The SSMEs. I've heard some opinions that those three bright objects you see in the last daylight video might be the SSMEs. I would like your opinion on that, and I haven't heard that we've recovered much of the SSMEs. Do you expect to? What are your thoughts on that?

MR. HILL: First of all, we do expect that those three bright dots in that Apache FLIR video are Main Engines or large components of the Main Engines. If you look at how they're behaving ballistically, they are certainly objects that are very heavy, relatively high ballistic numbers; and because they're so bright, they're continuing to move really fast. We also know from radar data and from, in fact, the SSME components we have found in Fort Polk, Louisiana, that, in fact, the engines or the large components thereof did stay intact for a long period of time and did go further east than any of the rest of the vehicle. I don't know personally – maybe Doug does – how much of each of the Main Engines we've found. I know that we do have main engine components that have been found and shipped to KSC.

MR. WHITE: Yeah. That's true. I don't have a reading of how much of each engine we've found. I can get you that number.

MR. HILL: That does beg a question on what we can learn from post-breakup trajectory analysis. Everything that I have talked about is pre-breakup. My entire team's focus has been pre-breakup. Everything that we have been trying to do is figure out what's coming off as early as possible and where is it so that we have some idea of where did the breach start, what caused the waterfall of events. It is certainly the opinion of the trajectory experts here at JSC that, taking the debris field as we find it in East Texas and trying to reverse-propagate it back to the vehicle is not something we are capable of doing. Again, going back to the FLIR video from the Apache helicopter, you saw all the secondary and tertiary breakups. As soon as you have additional breakups and those objects then become free fliers, they each have their own individual ballistic behavior. They're all now going somewhere else in the sky. We take the GPC we find laying on the ground in East Texas, we can back it up into the sky to some altitude but at some point we lose all truth, we lose all accuracy because that GPC at some point was in an avionics bay which at some point was surrounded by a compartment and at some point –

ADM. GEHMAN: What's the GPC?

MR. WHITE: General purpose computers.

MR. HILL: The fact that we know it behaves ballistically doesn't mean we can take it all the way back up to the Orbiter. At some point it was surrounded by another structure. If we could take the initial main breakup and assume that all the components we found in East Texas became free fliers at that point, we could do a pretty good job backward-propagating those things all the way up to the Orbiter; but we know, in fact, that it didn't happen that way. As Dr. Ailor said, even the individual components, say, individual pieces of tile that we find on the ground, whether we find them out west or the, say, the Littlefield tile, we don't know that that tile or object came off the vehicle looking like that. We have a full expectation for something fragile like a tile that, in fact, it did come apart.

Using some of the video, we know in several cases that the object, when you go frame by frame in the video, anyway, as you're looking at the object, you see a white dot come off the Orbiter and then you see that white dot shower into a lot more dots and then you see all the light go away. Probably indicative of something breaking. Now, is that several tiles coming off together and then flying apart? Is it a tile coming off and shattering into a lot of pieces? We have no idea.

MR. TETRAULT: As you're probably aware, we have found both of the forward corners of left wheel well structure; and that's where the wheel well door interfaces with the structure itself. So you have the inboard and you have the outboard corners, each of which demonstrates some venting coming out from the wheel well itself. My question is: If that's, in fact, going on, wouldn't you have an interruption to the plasma and wouldn't that show itself, to some degree perhaps, as a flare?

MR. HILL: Maybe. I hate to not be more specific; but, again it depends how did that hot gas get into the wheel well, was it flowing in or was it flowing out.

MR. TETRAULT: We're talking here about an outflow from the wheel well at the corners, forward in.

MR. HILL: Probably.

MR. TETRAULT: And it has an effect on the tiles at least, it's a guesstimate, 12 inches to 18 inches outboard from that venting. So it's quite a vent, if you will.

MR. HILL: Possibly. I mean, if you assume that that occurred pre-breakup and while the Orbiter was intact and still flying through the sky, it's possible that a jet like that coming out of the wheel well might change the plasma wake, might change what the Orbiter looks like to video taken from the ground; but we don't know. It depends on what direction was the shock, what direction was the plasma wake flowing in that is normally around the Orbiter, and did that jet actually make it all the way to the normal plasma wake and cause a disturbance or was it hidden or shielded behind the plasma wake that already existed around the Orbiter. We don't know the answer to that.

MR. HUBBARD: Two kinds of questions. First type has to do where all the material, raw material came from. Obviously we owe the public a great debt of gratitude for such cooperation. Can you tell us how many different submissions or contributions there have been and how many you sorted it into and a little bit about how you determined what was useful and what wasn't?

MR. HILL: Sure. Within three days of the accident, we had almost a thousand reports. Probably within a day or so of the accident actually, we were approaching a thousand different reports that varied from people calling in or sending E-mails and saying, "Hey, I looked up in the sky and saw this bright dot overhead," to, "I saw something happen and I want to talk to somebody about it," or videos where somebody called and said, "I have a video and I think I see something coming off the Orbiter," or," I have still photography and I think I see something coming off the Orbiter. Do you want it?" For the first day we spent most of our efforts sorting through a stack of close to 1,000 reports and, within about two weeks, about 3,000 reports that were all across the map. Just like that. We very quickly figured out if we were going to learn anything technically or anything of engineering value, it probably was not going to be in a report where people say, "Hey, I looked up and saw something in the sky," unless they said, "I looked up and I clearly saw something fall through the sky and smoke was coming off and that thing hit the ground close to my house." And there aren't very many of those.

So we very quickly narrowed it down to let's look for videos as far west as we can, let's look for still photography of the Orbiter in the sky as far west as we can, particularly time-lapse photography, and let's look for people that are amateur astronomers because those people are going to have a lot better secondary data like GPS coordinates on exactly where they were standing, exact zoom settings on their cameras, things like that, or exact time references, say, in the case of the video.

Within a week we had it narrowed down to about 15 videos that form the core of what we now have on this map, with the videos that actually show debris shedding that we were able to time correlate to within plus or minus a second. Then we spent some time after that first week or so prioritizing which of those we have the best celestial cues in, which of those that we think we are most likely to be able to calculate relative motion, and then which of those, like Debris 6 and 14, did we think would be so substantial that we might have a chance of getting them all the way to the ground and finding them in radar and putting boots on the ground and go and collect the hardware.

So I would say it took us about a week to sort out the initial round, maybe a week after that before we knew very well which of the videos, which of the stills were going to give us any meaningful data. From there on, it was a continuous process of analyzing the video, measuring the relative motion, generating these footprints, and then searching through radar. And without the public having taken these pictures on their own – because, to our great surprise, people are still very interested, apparently, in the space program and these folks got up before sunrise and went out on their own and stuck their cameras up in the sky and most of them also knew exactly where to look in the sky because again they were amateur astronomers – without those folks, we wouldn't know any of this. I mean, these people are definitely our heroes. And there are about 15 to 20 of these people or these videos that are probably the most key to us having been able to do any of this analysis.

ADM. GEHMAN: We join you in being thankful for that. We're also thankful for a crystal-clear morning across the entire southwest part of the United States.

MR. HUBBARD: Just a follow-up. There was a lot of debate early on about whether or not we were seeing some type of just bright gas or whatever. How confident are you, when you label the event in the time line as debris, that it actually is debris?

MR. HILL: I'm not sure how to answer that. We are reasonably confident. Again, I would say I am confident, if not sure, that many, if not all, of the things that we labeled as debris shedding events are, in fact, some object coming off the Orbiter. Can I tell you is it golf ball size or is it the size of this sheet of paper? I can't. It could very well be something as small as a marble in most of those videos and the ones that we think are so significant and that have gotten us so excited, those things could be golf ball size. We really don't know. We know relative sizes, relative motion, but we don't know specifically what they are. But we are very confident, based on the way they behave after they separate from the Orbiter, that they are, in fact separate ballistic objects or objects that have mass, in almost all cases. In the case of some of these flares, they could be something different like combustion products.

MR. HUBBARD: Just a final follow-up to this line of thinking here. When somebody sends something in, how do you determine that it's the real deal and not cooked up by a photo shop somewhere?

MR. HILL: For one thing, for most of these videos, we have had them for – we got them probably within a week. First week to ten days. Well, we got them within a week to ten days of the accident. In some cases we had them before that. It is possible, I guess, that some people could go and doctor them up. My expectation is we got most of these so quickly they didn't have the time to do that.

The other thing is in most cases we have overlapping videos, so we have redundant cues. In fact, we are taking advantage of that. We measure relative motion from one and we go back and measure relative motion on the other and we compare them. I would say they would have to be really darn smart to have doctored two opposite videos and give us the same relative motion in the two.

MR. WHITE: Our image analysts have also discovered some hoaxes that have been out there in the public and know they're hoaxes. They've also identified some things that have been anomalies or quirks of the way the photograph was taken – a jiggle of the camera, for example, that produced an effect in the photo that looked real but was not real, was an artifact of the way the photo was taken. They've also dispelled some things. Some of you may have seen what looked like a triangular shape when we were zoomed in close on the Orbiter that appeared to actually be showing the Orbiter in some detail. That wasn't it at all. So they have been able to sort out the hoaxes and the false images and the artifacts from the things that are real.

MR. HILL: Actually, most of our early hoaxes – and we did get some early on – were cars driving down the road with their headlights on. It was relatively clear to us that it wasn't something in space.

ADM. TURCOTTE: One last question from me. With your analysis of the radar and your being able to integrate the time line and the photographs together, are you surprised at the amount of wreckage that we have, i.e, do we have more than you expected from that analysis or do you think that you're surprised at that, at the amount that we do have?

MR. HILL: I'll give you two answers. Pre-breakup, I would say we continue to be shocked that we had debris coming off the Orbiter as we crossed the California coast and were dropping debris, clearly had an external breach in the vehicle and had hot gas somewhere in the left wing for that significant period of time and the vehicle flew perfectly, no indication of what was going on at flight control and virtually no indication of what was going on in telemetry on the ground other than we saw a few temperature pressure indications that didn't make sense to us and we had a few sensors that dropped off line. Aside from that, the vehicle flew like a champ until right up to the breakup. So that did surprise us.

Now, from things we are finding in East Texas, are we surprised that we only have 15 to 20 percent by weight of the Orbiter? I don't think so. I think when you first see the debris count and you see how many individual pieces of debris, our first reaction was one of surprise, how could we have gotten that much of the Orbiter down from 200,000 feet intact. Of course, I think you've also seen at KSC what they have is a whole lot of little, tiny pieces of what used to be an Orbiter. If you go look at it laying on the ground at KSC, you don't have a spacecraft lying there; you got a whole lot of nothing. I think that does fit in with what our conventional wisdom was prior to this happening.

GEN. DEAL: Follow-up to Scott Hubbard's question. Are you still expecting any more imagery, or do you think the well has run dry?

MR. HILL: No, sir, I think for the most part the well has run dry. Again, most people contacted us right away. We had most of the video in hand within a week. Overall, the support from the public has just flat been overwhelming. So I would expect not to get any more in.

Now, there have been two isolated cases out west of two individuals who strung us along for several weeks before it finally became apparent to us that they must have been under the impression they were going to collect on the *Columbia* gravy train. And it did take us a while to figure out while they trickled an individual image to us or an individual video to us that is, in fact, what was going on. They must have discovered this was their 15 minutes, but they are huge exceptions to the rule. The overwhelming support has just been fantastic, and I think we have it all.

GEN. DEAL: In the early days when the Admiral took us to Nacogdoches, there was talk about everywhere from offering a bounty money incentive for people turning in parts, you know, going out in fields and looking for parts, to certificates from NASA to thank them. Are any of those still under consideration, or are we just in a debris collection mode?

MR. HILL: To my knowledge, we are not planning on offering any rewards to people to incentivize them to come forward if they have not already. I can tell you the folks here that are doing work have every intention, when the dust settles, to come up with some formal recognition. We have various folks we want to recognize. In my team's case, we definitely want to recognize the people that took these images for us and made all this possible; and there are various things that, at the working level, we are kicking around that we would like to do. Now, I'm sure the Program will do something when this is all over.

GEN. DEAL: Great. Thank you.

One more for Doug. You gave us an excellent tracing of all the sensory. You've had plenty of time now to do some reflections on it and some lessons learned. Anything that you've already considered that we ought to be thinking about as far as sensor wiring, sensor location or junction boxes and how they're constructed?

MR. WHITE: I'd have to say no. It's probably too soon to speculate on any type of redesign that we might want to do with our instrumentation. As you know, the instrumentation wasn't designed to have flow inside of the wing; and so it probably failed in the way we would have expected it to. So as of yet, we have not considered any sort of internal redesign to better protect that instrumentation or even make more instrumentation available.

ADM. GEHMAN: Gentlemen, on behalf of the Board, we want to thank you for appearing today; but I hope you will also take back to your working groups, of which I know you are the tip of an iceberg of literally hundreds of people that are working with extreme zeal and professionalism to try and solve this riddle – because many of us have visited your working groups and we know how many people are working on this – please pass on to all of them our deepest gratitude and our deepest respect for the work that you all have done and will continue to do. We appreciate it very much. We haven't solved this thing yet, but someplace in your work we'll find the answer and we appreciate it very much. Thank you for appearing here today. We appreciate it.

(Hearing concluded at 4:24 p.m.)

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March 18, 2003
Houston, Texas

Columbia Accident Investigation Board Public Hearing
Tuesday, March 18, 2003

9:00 a.m.
Hilton Houston - Clear Lake
3000 NASA Road One
Houston, Texas

Board Members Present:
Admiral Hal Gehman
Major General John Barry
Mr. Roger E. Tetrault
Dr. Sheila Widnall
Dr. James N. Hallock
Mr. Steven Wallace

Witnesses Testifying:
Mr. Steven Labbe
Mr. Chris Madden
Mr. Jose Caram
Dr. John Bertin

ADM. GEHMAN: Good morning. We'll go ahead and get started. This morning we're going to talk about aerodynamic and thermodynamic events that took place when the Columbia reentered the atmosphere. We have two panels this morning. The first panel consists of the NASA engineers and scientists who are trying to find out what happened to the *Columbia*; and then the second panel is an outside expert, as we usually do.

This morning we have Mr. Stephen Labbe, the chief of the Applied Aeroscience and Computational Fluid Dynamics Branch of NASA; Christopher Madden, the deputy chief of the Thermal Design Branch of NASA; and Joe Caram, an aerospace engineer in the Aeroscience and Flight Mechanics Division of NASA.

ADM. GEHMAN: Gentlemen, thank you very much for helping us through this. Before we begin, we don't swear people in; but I will read you an oath of affirmation and ask you to state that you will give information that's complete and correct, to the best of your knowledge. So before we begin, let me ask you to affirm that the information you provide the board today will be accurate and complete to the best of your current knowledge and belief.

THE WITNESSES: Yes.

ADM. GEHMAN: All right. Would you, please – in order, please – introduce yourselves, tell us a little bit about your background and your current job and not only your full-time job but your role in the MRT.

MR. LABBE: My name is Steve Labbe. I'm the branch chief for the Applied Aeroscience and Computational Fluid Dynamics Branch here at Johnson Space Center. I've been with NASA since about 1981. Prior to February 1st, our branch was not really heavily involved in the Shuttle program because it was primarily – it's an operational system. We were working on the future. Since February 1st, we have been heavily involved in the investigation and supporting the efforts with a team that crosses the agency and the country.

ADM. GEHMAN: Thank you.

MR. CARAM: My name is Joe Caram. I work in the Aeroscience Flight Mechanics Division. For the last six years, I've been the chief engineer for the X-38 project for my division. So prior to February 1st, that's what I was doing. Prior to that, I was in Steve's branch, working in the area of aerothermodynamics, where I focused on the shock-shock interaction region of the wing and boundary layer transition.

ADM GEHMAN: Thank you

MR. MADDEN: I'm Chris Madden. I'm deputy branch chief of the Thermal Design Branch in the Johnson Space Center. My background includes thermoanalysis of TPS systems for reentry spacecraft. Some of that's included analysis of Shuttle flight anomalies and other consultational roles on the Shuttle. For the mission, the Columbia mission, our branch was providing consultation for the work done by USA and reviewed for that.

ADM. GEHMAN: Thank you very much. Gentlemen, you may start. Who's first? Steve?

MR. LABBE: I'm going to start this morning.

Good morning to everyone. I just wanted to thank you for the opportunity to come and present our efforts that have been in support of this. We have a whole bunch of material. So I suggest we just get started.

Go to the second chart, please.

What we're going to cover today, I'm going to give you kind of an introduction and then describe our analysis process, the current approach, what we're doing. In our approach right now, we're starting with an assumed initial damage and then trying to propagate that to reproduce the aero and thermo response. We're assuming the damage existed. We're not trying to find necessarily the root cause. Once our results are then completed, we hope that they will point towards the root cause, but we start with the damage. We're also looking at about the first 600 seconds of entry. We're trying to get from what happened from entry interface to the point where we believe there's a breach in the wheel well and the temperatures start rising. So, if we can get that solved, we feel we'll have made a significant contribution to the investigation.

The reason the three of us are up here together is it's an integrated approach. We don't believe that just aerodynamics or aerothermodynamics or thermo by itself would be a good answer. We need to all be consistent, and our results have to all work together. So there's the three of us here, and we're part of that integrated team.

Next chart, please. This is just a brief snapshot of the organization, and it's really trying to give you a picture of the breadth of the scope that we're working. We have support from numerous NASA centers, the Boeing Company and its different divisions, Lockheed Martin, Sandia National Labs, and the Air Force research lab at Wright Pat. So we have quite a range of expertise, and they are supporting us in a large variety of areas that we represent.

Next chart. Okay. The approach. Basically we're trying to, as I said, start with damage and then take specific actions to investigate how the scenario that comes up can be used and explain the key data events. The first poster board there attempts to illustrate that on the left here, the tall one. I guess the easiest way is to just talk about it from here.

What we have plotted along the top – and I'll go into much more detail there – is the change in aerodynamics that we saw during the mission. It's versus time, and you can see that it's not zero. It's drifting negative and then eventually drifts positive. Down below, it starts here and then drifts so.

What we then wanted to do was find some key events in the instrumentation that corresponded with those changes. So the first thing that we noticed was this first off-scale low temperature – I'm sorry, the bit flip in the wheel well was the first thing that we noticed, the change in the temperature. This is the brake line temperature in the main landing gear wheel well, and at this point it started an upward trend that continued. So this was the first point; and we correlated that, tried to correlate that to the aerodynamic events.

The second point is when we see our first off-scale low temperature. So the first, Point A, suggests a breach, a first initial breach into the wing. There must have been an ingestion of hot gas in order to create that change in the wheel well, and we're going to get you into the details of why we believe that. The second one is a burn-through of the wire bundle that holds all of those instruments, so that whatever was being ingested had to be able to burn through that wire bundle.

When we get to the wheel well breach here, we see a significant rate of change. Instead of just drifting up, now we see a large increase in the rate of change. Also that corresponds to a change in the aerodynamic trend where it was drifting negative and now is starting to go back positive.

So that's the idea. Line up these key events and analyze each one of those and more or less provide what we're calling a piecewise integration of the event as opposed to some time-dependent, multi-physics solution that would explain it from time zero through. We would never get through that analysis.

ADM. GEHMAN: Pardon me for interrupting. On the top chart, I presume that's time after EI along the X-axis?

MR. LABBE: That's correct.

ADM. GEHMAN: In seconds?

MR. LABBE: Yes.

ADM. GEHMAN: Hundreds of seconds. What's the vertical axis? On the top.

MR. LABBE: This is a residual or change in aerodynamic – it's a coefficient form, but it's rolling moment. We express that in coefficient terms. I'm going to show you a lot more detail on this, but this is the change. We would expect it to be drifting, bouncing back and forth around zero. Instead, it's biased off to one direction.

ADM. GEHMAN: What is the big fluctuation right at the beginning up there?

MR. LABBE: Very early in flight, the dynamic pressure is so low that the technique we use here, we're not going to be able to resolve down to this coefficient form. You're essentially – accuracy of the data available. The initial spike there that you see is a roll. This is the first bank maneuver.

DR. WIDNALL: You and I have talked about this before, but have you applied this analysis to earlier flights and satisfied yourself that what can be identified as off-nominal is, in fact, accurately off-nominal? I know we've talked about that before.

MR. LABBE: Yes. We applied the same tool to STS-109, which was the previous flight of *Columbia*; and where we see similarities in these types of traces, we assume that that is just the accuracy level of our capability. Where they drift apart, that's when we start believing that we're seeing something off nominal.

DR. WIDNALL: Okay. I'm sure you'll share that data with me and us.

MR. LABBE: Absolutely.

MR. TETRAULT: You talked about a sensor bit flip. Would you define a bit flip for us?

MR. MADDEN: The bit flip is just the resolution of the instrumentation. So if the temperature change is 1 1/2 degrees – 1 degree, you may not necessarily see that change. So it has to change over about a degree and a half, then it will register a change in the data system. So that's why you see the step-wise plots. It's not a smooth plot because the resolution of the data isn't that tight. So when we say bit flip, we are just saying a change in temperature of about a degree and a half Fahrenheit.

MR. WALLACE: These rolling moments when we see later on what I thought to be yaw corrections, is it a pure rolling moment, or is there a yaw element to it?

MR. LABBE: There's both. There's actually all three axes – roll, pitch, and yaw. There are some deltas that we extracted. This is just the roll axes, but I'll be showing you both the yaw and the roll axes.

ADM. GEHMAN: Referring to the top chart again, the big spike is a roll reversal or something like that?

MR. LABBE: The spike here is a roll reversal and the technique that we use is not as accurate during a roll reversal, but you get a lot of rates in the vehicle.

ADM. GEHMAN: You say you're going to go into that in a little more detail?

MR. LABBE: Yes, sir.

ADM. GEHMAN: Okay. Fine.

MR. LABBE: I think we've pretty much covered what's on this chart, and the next chart is really just another version of the poster. So I'd like to move on to Chart 6.

This is just a definition of what we're defining as these key events, A, B, C, and D. I kind of alluded to this, but there's a hole damage size, there's a breach in the wing at what we call 488 seconds. That's when we see that bit flip. So what can we do? What kind of hole or damage can be created from entry interface to 488 seconds that could produce that initial change in the instrumentation?

Then we go on to the next step. Step B is we burn through that wire in another 42 seconds. So if we pick a location and we have a burn-through, can it then also burn through the wire 42 seconds later?

Then we have the breach into the wheel well at 600 seconds where we see the rate of change. Of course, that has to be consistent with the initial breach and the burning through the wire. So you can see how we're trying to piece all of these together. Then finally we see this change in the fuselage wall temperatures; and whatever is producing that, is the damage consistent with that and how we've propagated it to generate that. Aero, thermal, debris – everything has to be correlated or we did not prove a specific scenario.

Okay. Chart 7. Just another way of looking at the same thing. I really just spoke to this. We're looking at all the data, the flight data, whether it's debris evidence, flight profiles. We're more or less handed a failure scenario from the failure scenario team that's developing those, and then we go and do our analysis and tests in the aero and thermal analysis and tests. We produce our results; we get them back together. Are they consistent? That's the flow of the whole integrated analysis, and what I'm going to do now is take you through the aerodynamics side of that analysis.

If we go to Chart 8, which is also represented here in the poster board. So this is going back to the very beginning, February 1st, you know. What we were trying to do is what happened. We needed to reconstruct the flight, essentially. We had data. We had flight data that was telemetried to the ground. We knew the mass properties. We took that data. We had some tools that had been developed from various programs, the X-40 out at Boeing and the X-38 here at JSC, to get delta aerodynamics from that flight data. And I'm going to explain to you how we do that.

Out of that tool, we get our change in aero. We put that into the flight control simulation and then we compare what the flight control simulation predicts the response of the control surfaces, the ailerons, the elevons, the body flaps, the jets firing, to what was actually indicated in flight. And we iterate around that loop several times to make sure that we have a good comparison. That was our early focus, and that probably took two to three weeks for us to get worked out. We're working on actually working on a final iteration right now.

We do have a good match, and so we're now transitioning into what we call the damage assessment phase. This is, again, more or less saying the same thing. We have this assumed damage. I'm going to go build a model, whether it's in the wind tunnel or computationally, with that damage. I'm going to take measurements or make predictions or make calculations. I'm going to look back at what it's producing. Is it consistent with my change in aerodynamics that I reconstructed? I'm also going to be looking back to the integrated team to make sure we're consistent with each other and the other inputs. And we went down to the Cape on Friday to look at the recovered debris and to try to understand that so that when we're looking at different scenarios, we're also considering what's been found there. Ultimately, if we're successful, we have this piecewise integration of the change in configuration.

Next chart. Okay. How do we reconstruct the aerodynamics? We have a data base, a very well-defined data base. The Shuttle's been flying for 0 years; and this data base has been established through wind tunnel testing, flight testing. It's well defined. We take the flight data, the flight conditions, the Mach number, the angle of attack, the mass properties, the control surface settings, where they are. We feed those into our data book, and it will predict the nominal aerodynamic coefficients that we should get out of that configuration and that flight condition.

We also take the flight data in Step 2 and we put it in the equations and motions for aircraft. Out will come from that what was happening in flight. Now, in flight we were what we call trimmed. The vehicle was not yawing or rolling. It was in a steady, controlled flight, even though it was experiencing these moments. So the second part of that equation essentially becomes zero and the delta – and when we go into the data book and we were putting in several degrees of aileron or is there some side slip on the vehicle, these would produce a moment. So when we delta those down at the bottom in the third step, we're going to get some change in the aerodynamics that the vehicle was experiencing in order for the flight control system to have commanded these settings on aileron and the other control effecters. So that is the process we use to define our delta aerodynamics.

The next two charts go into the details of those results. These are some busy charts, but these really tell the aerodynamic analysts the story of what was happening. This is a change in yawing moment coefficient. Just the change in yaw. Yaw is nose left and right, and it's versus time. We have GMT time on the bottom and time from entry interface across the top. What we would expect on a nominal flight is, like I say, some scatter like so, which would stay near zero the entire time. What we saw on 107 after we did our analysis was this change in the yawing moment that started off drifting very slowly, plateaued, and then sometime around 80 seconds or so before loss of signal, it started to increase rapidly. Then just prior to loss of signal, it increased rather dramatically before we ran out of essentially any available data, which is about 5 seconds after the LOS.

ADM GEHMAN: Okay, we'll just stop here for a second.

DR. WIDNALL: I just want to know whether you feel that that dramatic increase is a valid either measurement or computation or both.

MR. LABBE: I think so now. When we first looked at it, we were not sure, but we've gone back and the team that is recovering the data to support our analysis has confirmed those measurements by trying to look at two sources for it. So, yes, I believe that is really valid.

DR. WIDNALL: Also, with the earlier times. I mean, you mentioned, back one chart, with the earlier times you mentioned, you know, scatter in the data. So would you say from – I can't read your T from zero from here. Is that 13: 50 something or other. Way back at what would be time equals zero on that graph.

MR. LABBE: It's time actually about 300 seconds from EI. 13:50.

DR. WIDNALL: Is that little drop towards negative and then that slight negative plateau, is that a valid indication of off nominal, or would you consider that part of the noise in your data?

MR. LABBE: I would consider that part of the noise for this. When I went back and looked at STS 109, it showed the same signature time frame.

DR. WIDNALL: So, in some sense the valid begins at 13: 52.

MR. LABBE: That's correct. 13:52:17, which also happens to be – we did not look at the data first, but that happens to be when the brake temperature bit flip is also occurring.

A little bit more. There's several lines here that represent the Boeing simulation or analysis technique and the JSC analysis technique, and then the black line represents the model we gave to the flight control community for them to use in the simulation. I won't go through each one of these, but the idea was we correlated things with time on the time line. Yellow is off nominal. Green is a nominal event such as starting of alpha modulation or a roll reversal. Then this red box, this is a design limit for asymmetries. We do expect to see some asymmetries in flight and occasionally we see those, but you can see its level is here and near the end of flight we are on the order of five times that level. So something very dramatic happened at that time which led to the loss of control.

GEN. BARRY: Could you put some of this in context with dynamic pressure? If I remember right, at loss of signal it's about 80 pounds per square foot. Now, that would equate to about 180 miles per hour, right?

MR. LABBE: At sea level. I think it's a little – about 150 miles per hour, somewhere in that neighborhood, though. Yes.

GEN. BARRY: Of course, the air molecules are so far between. We really do have low dynamic pressure. Can you give us a context of, you know, if there's any kind of movement of the Orbiter, how much of a transient force is going to have to be in this case a roll or a yaw moment to be able to counteract this? We know the RCS jets are still functioning here.

MR. LABBE: Here we're in about, say, 10 to 20 thousand what we call foot pounds. So you're pushing with 20,000 pounds a foot away, and that's the kind of moment. That's just a couple of degrees of aileron. One jet firing can manage that. Near the end when we go off in this total value here, that's about0 160,000 foot pounds. That requires all four jets, three or four degrees of aileron, the side slip. Everything the vehicle had to try to counteract that moment, it was using. That's what the flight data shows, and that's what our simulation shows. So that's a very large moment.

GEN. BARRY: If you were to put this in context, if you were trying to put your hand outside in an airplane at 180 miles an hour, you would get some kind of feel for not only do we have the flight control elements on the Orbiter trying to control, but you also have the RCS jets doing the best turn they can to try to hold this in control.

MR. LABBE: That's right. If you hold your arm outside the car, you can feel that trying to pull your arm back. That's the moment is what you're feeling about your shoulder and you're talking maybe, you know, 10 pounds and a couple – 20 pounds of moment. 20 foot pounds of moment. Not very much at all. And we're talking about several – over a hundred thousand foot pounds of moment.

ADM. GEHMAN: Steve, you've got it marked right here is the roll reversal. This spike right here is a normal spike associated with the roll reversal and the stop of the roll reversal.

MR. LABBE: That's correct.

ADM. GEHMAN: I don't know. I mean, the magnitude of it may be a little greater than normal, but a spike normally occurs.

MR. LABBE: Yes. The techniques work best when you're in trim. When you're actually doing a maneuver, you're not exactly trimmed; you're producing rates and roll and yaw, so the technique shows a residual there. It's really the accuracy of our data base during a dynamic move versus static trim flight.

ADM. GEHMAN: But, this one over here is not explained by the roll reversal, though.

MR. LABBE: No, it's not, although we believe that is a normal response that has been seen on previous flights post roll reversal where there's either a change in the density in the atmosphere or the vehicle is adjusting. And we have gone back and seen – the flight control team specifically has seen that type of signature in other flights.

ADM. GEHMAN: Yesterday we heard that there's kind of a magic altitude of around 42 miles or 40 miles which, of course, works out to about 220,000 feet, something like that, at which reentry vehicles seem to hit a wall. Could you tell me about what the altitude of the Orbiter was at that time?

MR. LABBE: I believe it's around 210,000 to 220,000. Very close, I could get you an exact.

ADM. GEHMAN: But we're close. I mean, we could go look it up.

MR. MADDEN: Right. 210.

MR. LABBE: About 210,000 feet, roughly.

ADM. GEHMAN: You've got debris shedding down here. This is kind of Debris 1 through 6, as I read that. Correct?

MR. LABBE: That's correct.

ADM. GEHMAN: Debris 6, as we learned yesterday, was the first large thing that came off. Then Debris 14, have you got that marked here?

MR. LABBE: I do not have that on this particular chart, no. It's later in the time line. Do you have a time for that?

MR. MADDEN: Debris 14.

MR. LABBE: 14 is roughly 13:55, 56 time frame. About a minute and a half later there.

ADM. GEHMAN: 13:56.

MR. LABBE: Right in there. Yes.

ADM. GEHMAN: Okay. So the two big pieces of debris come off and it doesn't appear to trigger an aerodynamic reaction.

MR. LABBE: Can we go to the next chart?

ADM. GEHMAN: Okay.

MR. LABBE: Okay. This is the same plot, but now I'm looking at a change in rolling moment. That was change in yawing moment; this is change in rolling moment. Here you do see a definite correlation between that large debris. Somewhere between Debris 5 and 6 is when we see this event where the rolling moment was drifting negative, the change in rolling moment, and it changes direction. It starts its positive trend. We think this is a very key point for us in trying to understand what happened. Something changed about the configuration, some damage. Since we know we were shedding debris, something significant happened there to change the trend on rolling moment.

Debris 14, a minute and a half later. Again, we don't necessarily see that.

ADM. GEHMAN: It's right about here. Now, what kind of a change in the aerodynamic, the external aerodynamic posture of the vehicle would cause a change in the slope from going one way to going the other way? I mean, damage on the opposite side?

MR. LABBE: I don't think so. You know, you've asked the $64,000 question there, I believe. That's what our work is going to be. You know, what it suggests early on is that I was losing lift on the left wing and then something changed to start creating lift on the left wing or pushing up on the left wing. Whether or not that's opening up a large cavity on the lower surface, I'll show you some results from the wind tunnel that would suggest an opening of a fairly large cavity on the lower surface actually results in what I can think of is the damage is so significant it's creating locally a very high pressure that is on the lower surface of the wing and starting to push up on the wing as opposed to just disturbing the flow.

ADM. GEHMAN: As a non-aviator, let me ask kind of a basic question. Is it possible that the aileron trim, elevon trim, which is, of course, a measurement that you use which is not standard with airplanes, but it is possible that the Orbiter, trying to correct one difficulty, created lift under the wing by the way the elevons are set?

MR. LABBE: I'm not sure I follow the question.

ADM. GEHMAN: Well, in other words, in an effort of the guidance and control system to correct the yaw, for example, that the Orbiter trimmed itself in such a way as to actually – you know, like putting your flaps down?

MR. LABBE: Right. The way the Orbiter flies hypersonically is not your conventional aircraft. There's no rudder available. It's mass, because you're up at a high angle of attack, and you're using aileron and side-slip and then the jets, of course, as your third effecter to try to trim both in two axes, yaw and roll. Everything that we've seen about the flight says that the vehicle was doing, the flight control system was doing the proper response to these changes in these moments to trim out both yaw and roll. So, we were not trimming one and sacrificing the other; they were both being trimmed.

ADM. GEHMAN: You answered my question.

MR. LABBE: Okay.

ADM. GEHMAN: So we don't have an explanation for this?

MR. LABBE: No, not yet, that's our damage assessment work.

ADM. GEHMAN: It's not consistent with other indicators.

GEN. BARRY: You understand, of course, the roll reversal occurs at 56:30. We've got it on the green box there. We have most of our shedding occurring before that because Debris 14 goes off at 56:55. And we have a roll reversal and, of course, what you see after 56:30 there after the roll reversal, how much off nominal is that, compared to other Shuttle approaches? The roll reversal is normal; but at 56:30 she goes right off, you know, starting to gradually increase.

MR. LABBE: Right. How much off nominal here?

GEN. BARRY: Yes. Exactly.

MR. LABBE: I guess, you know, one thing to say, with all that damage, the vehicle executed a perfectly nominal roll reversal in the middle of the flight. So despite all the damage, the flight control system still was commanding the vehicle to do exactly what guidance was telling it to do. In this level here these are small and during that time period are not anything significant. It's almost like the damage has returned the vehicle back to its original flight characteristics; but then, of course, starting here we see a rapid increase and then essentially going off the cliff there at the end.

Okay. We move on to page 12. It's really just a summary of what we found. I think we've discussed just about everything here. The one thing I would like to point out is that the results – we see initially a negative roll and a negative yaw, and there's been a lot of discussion about asymmetric boundary layer transition. When you experience that on the Orbiter, these two increments will have opposite sines. So if you have positive yaw, you'll have negative roll or vice versa. We saw the same sine on this. This indicates to me that whatever was happening early on is not asymmetric boundary layer transition; it's some damage. And just basically the bottom line is at the end, just before loss of signal, we were at or approaching rapidly the trim capability of the vehicle.

Okay. The next topic I want to discuss is now our damage assessment, what is causing this. We have our events, our A, B, C, D and loss-of-signal events where we're trying to look at the aero characteristics I just showed you and now go and try to produce some damage and do some tests and analysis that will generate those signatures. We have wind tunnel testing being done at Langley in their facilities, and we're employing computational fluid dynamics from very simple tools to our highest fidelity tools. Like I said before, we are assuming damage and then creating a model and then measuring or calculating that and then mapping it back to the events.

On page 14, this is just a chart from Langley. They've been doing an outstanding job in supporting us, and we also have a poster of this. Basically this summarizes the three hypersonic tunnels they have there that we are employing in our investigation. There's a Shuttle trajectory here versus Mach number and altitude and then we have the Mach 6 tunnel here and they have a Mach 10 tunnel and then you've heard about maybe the CF4 tunnel.

We do our initial screening in a Mach 6 tunnel, and there's a lot of questions about how that was applied since *Columbia* was at Mach 20 and above when we were seeing these events. When you're at Mach 6, you have all of the physics of hypersonic flow – and they are listed there – but you don't have chemistry. Because of the speed and temperatures, there's a lot of chemistry that goes on.

One way to simulate that chemistry is to go into this CF4 tunnel, and it changes what we refer to as the ratio of specific heats. But what that does to the vehicle is brings the shock much closer, the bow shock much closer to the vehicle. Expansions are much deeper. Compressions are much stronger. So by going into the CF4, we can take a step much closer to flight. We still don't get up to this point here. Loss of signal is actually at Mach 18 or so, but that's where we can employ computational fluid dynamics to get to that next step.

DR. WIDNALL: I want to understand just a little bit more about the CF4 tunnel. When you say it changes the specific heat, how is that actually accomplished? Is that because the gas is actually at the real temperature or because there's a different kind of simulation?

MR. LABBE: Maybe Joe can help me out here. All we've done is change the gas from air to CF4.

DR. WIDNALL: What is CF$

MR. LABBE: It's freon

DR. WIDNALL: So you've basically changed the gas to freon; but, for example, the same temperature on the vehicle would be a low temperature.

MR. LABBE: Relative.

DR. WIDNALL: Relatively low. So it's not like an arc jet simulator or something like that.

MR. LABBE: Okay. I just wanted to give you a snapshot of the tunnels and how we're applying them.

The next chart shows some damage. Here's a picture of the Mach 6 tunnel. There's the model inside the tunnel, and we have a model here for you to also look at. They're about 10 inches long, so they're about three quarters of 1 percent in scale. We've been taking IR images, so we can get thermal imaging of the model at the same time we get aerodynamics. And we've gone in and just done some damage where we notched out the wing leading edge or drill some holes behind the wing leading edge to represent carrier panel damage or even this is like a side shot of the wheel well cavity where we've created a cavity in the lower surface of the wing. What I'd like to do is show you some results of that testing.

Next chart, please. It's again another complicated chart, but what we have across the top is our thermal imaging. You're looking at the lower surface of the wing, and you have missing RCC Panel 6. You have a gouge or essentially what's representative of tile damage right in the middle of the main landing gear door and then you have the holes drilled through the wing, which would represent damage to the carrier panel. What you see here is that the state of the boundary layers essentially indicated by the thermal imaging where you see the increase in heating, we know that we've tripped the boundary layer and it's gone turbulent for this particular run. These are very preliminary results. We like to use those tunnels. We want to use Mach 6 and CF4. This was just the Mach 6 aero results. So it's premature to draw too many conclusions just from this set of results.

We have just completed similar testing in the CF4 this week, and we'll be looking at those real soon. But what this shows is basically we're not getting much in the wind tunnel, not much change to the aerodynamics, even for taking out a notch that would represent an entire missing panel, and yaw or roll.

MR. WALLACE: Just to clarify, when you simulate the missing RCC panel, your model doesn't simulate any flow through the wing and exiting?

MR. LABBE: That's right. It's just an external type of notch, and it's a limitation of the testing.

MR. TETRAULT: Let me pursue that question a little bit. We have debris that is both of the left wheel well forward corners, and the debris indicates that there was a flow coming out from the wheel well outward at those corners. The inboard corner was flowing inboard, and the outboard corner was flowing outboard. What would that do to the flow field? Would that create lift, or what's your sense of how that would affect the flow field?

MR. LABBE: Like I said, we were there Friday and we saw the debris and we were puzzled by the flow patterns. I think if you have a jet, if it's coming out with a strong enough rate that you create a jet or create enough flow out of there, it will set up a shock in front of that which will create a high pressure which would be on the lower surface which would push up on the wing and would probably create more lift. Obviously by the time we've gotten to that point, though, there must be other damage. So exactly how those all work together is our challenge.

MR. TETRAULT: But it could create a lift, as long as that jet was still there?

MR. LABBE: Yes. I would think so. Now, we're looking at all that debris. We are, in our own minds, wondering what happened prior to breakup and what happened post breakup.

ADM. GEHMAN: Let me ask another layman's question here. The patterns that we see up there don't change whether you're in the right-wing-down or left-wing-down pattern?

MR. LABBE: That's correct. These are all, you know – angle attack, the plots show angle attack of three angles of attack – 39, 40, and 43, I believe, is what was tested. The aerodynamics of the vehicle are a function of angle of attack and angle of side slip and Mach number, not bank angle. We bank about the velocity vector. So whether you're left wing down or right wing down, what the vehicle sees is the same from the aerodynamic standpoint.

ADM. GEHMAN: That's intuitively not obvious.

MR. LABBE: I understand.

DR. WIDNALL: Just to pursue that a little bit, in your reconstruction you have really verified that beta, the side slip angle was zero – in other words, there's no question about that, that the side slip angle was zero?

MR. LABBE: In our reconstruction, beta starts out at zero early in flight. But sometime around the time when we see the first change in aero, it starts drifting negative. By loss of signal, it's hanging out at about 1 degree negative.

DR. WIDNALL: Right, I understand that. But do you have beta through the roll reversal, all the maneuvering, so you have a graph of beta as a function of time?

MR. LABBE: Yes.

DR. WIDNALL: And it's what, less than 1 degree?

MR. LABBE: Right. During roll maneuvers it might go up to several degrees.

DR. WIDNALL: I'd like to have a copy of that.

MR. LABBE: We'll get you that.

DR. WIDNALL: Great.

MR. LABBE: Okay, so not a whole lot of damage. There is some CFD results here –

ADM. GEHMAN: Excuse me again. Since we can't really read the scale on that chart, can you give us some kind of indication of whether that's a little heat, a lot of heat, severe heat,
life-threatening heat?

MR. LABBE: I'll let Joe answer that.

MR. CARAM: As you look at the images, you can see that the areas we see of red are indications of fully developed turbulent boundary layers. So you have two types of boundary layer characteristics – laminar or turbulent. The turbulent provides higher heating, on the order of two to three times what you see for the laminar heating.

DR. WIDNALL: You used a key word, and I want to make sure that I understand this chart. The dashed line on the graphs is your calculated differential aerodynamics that you would hope the wind tunnel tests would go to?

MR. LABBE: That's correct.

DR. WIDNALL: So your wind tunnel tests are the solid lines with the dots on them, the dashed line is what you had hoped to get out of that particular wind tunnel test to explain, and then that triangle you said was a CFD – is that what you said?

MR. LABBE: That's correct.

DR. WIDNALL: Okay. That's very interesting. So you're saying that the CFD actually predicts what you hoped the wind tunnel tests would show. Is that what you're saying?

MR. LABBE: That's what I'm saying. And if we go to the next –

DR. WIDNALL: Wait a minute. I mean, I know this is a nasty question because I understand the limitations of CFD, but to what do you attribute the difference between CFD and the wind tunnel tests?

MR. LABBE: Okay. The CFD, this is an Euler calculation –

DR. WIDNALL: It's a challenging calculation.

MR. LABBE: But this is a calculation that doesn't have a boundary layer. And I believe what's happening is when we are tripping the boundary layer here, we're getting offsetting changes. So when I do this computation, I don't have a boundary layer and I'm not getting the offsetting changes.

DR. WIDNALL: Okay.

DR. HALLOCK: My experience is primarily below Mach 1, but one of the issues you have when you're dealing with wind tunnels is matching Reynolds number. Here I see it is 10 to the 6. What is it really for the Shuttle itself, and is that a problem there?

MR. LABBE: This was run at roughly 2.4 million, which is based on the length of the Orbiter. When we are in the flight regime that we're studying, where we're interested is about half a million up to about 2 million.

MR. CARAM: 2 million.

MR. LABBE: So this particular test was at a little bit higher Reynolds number.

GEN. BARRY: If you could just put to bed one final question that we keep getting. Is there anything that could have been done, whether the Orbiter rolled left or right, to minimize the heat as it was reentering, based on any of the testing you're getting on wind tunnel or otherwise?

MR. LABBE: I don't believe bank angle changes your heating profile at all. So the answer would be, no, I don't believe so.

MR. CARAM: No.

MR. LABBE: Okay. The next chart does show just a snapshot of that CFD analysis. This is again done by Langley, using a code call FELISA, and we took out the same RCC Panel 6. You can see the flow patterns, essentially showing the pressure distribution. There's a shock forming. These three thermocouples on the side of the fuselage that showed temperature increases, the shock is in the vicinity of that. We're doing this at Mach 23.8. So it's very close to flight conditions. These figures here show the blue is a clean configuration and then the red would be with the notch and we're showing that the stream lines are tending towards the fuselage. So there's a lot of indications here that wing leading edge damage is consistent with some of the patterns we're seeing in the data.

DR. WIDNALL: Could I have a question? I mean, I think that's a very exciting result. So what you're saying is that the temperature increase on the side of the vehicle could be explained by a shock coming off of this notch in the leading edge? That's the first time I've seen this.

MR. LABBE: Okay. And Joe is going to show you a lot more of that. But, yes.

MR. TETRAULT: Does that explain the temperature that's far forward, the temperature increases in the dump values?

MR. CARAM: No, that does not.

MR. TETRAULT: It does not get to that, it only gets to the side body aft.

MR. CARAM: That's correct. The flow's not going to be moving forward on the vehicle. It's only going to be moving aft.

MR. LABBE: Okay. The next chart just goes into a little bit larger damage. Basically we talked about the wheel well. They took a metal model at Langley and machined out a representative cavity that would represent the main landing gear wheel well. And there are two depths to that, basically, a very deep and then a more shallow. That's what the H over L is representing. It's kind of hard to see; but if you look closely, this shock that's forming in the wheel well in this cavity is much stronger for the shallower.

ADM. GEHMAN: You'll have to describe what we're looking at here.

MR. LABBE: Okay. I'm sorry. This is a Schlieren photograph. What we use that to do is to see the shock structure in the flow field. So what you're seeing is a bow shock on the Orbiter vehicle and then embedded inside of that is a secondary shock where this cavity is and you can see there's this faint line that goes up here is indicative of the shock forming in the wheel well. Those are forming when you have abrupt changes in the flow field. You end up forming shocks, and that would be an area where you could expect high pressure.

So the results, this is a later time in flight. Now we're 860 seconds and again the same format on the plot that Sheila pointed out where we have the flight data and we think we should be approximating with this type of damage and then the wind tunnel results. And we're getting in the neighborhood. In the rolling moment, the yawing moment, we're only producing about half of what is expected. But that's essentially the technique. We'll look at this. We'll map it back. We're going to get these results out of the CF4 tunnel which will be closer to flight. We talked about the changes. This bow shock will be much closer to the body in the CF4, which would be much more like flight, which should change some of these characteristics of what we're measuring.

ADM. GEHMAN: But that particular measurement was if there was no landing gear door, landing gear door is gone and you've just got a hole there because the landing gear door has been ripped off.

MR. LABBE: That's right. In this particular, we've done calculations with landing gear and main landing gear deployed – or testing. I just don't have those charts.

DR. WIDNALL: I was confused by this chart, are these two pictures of two different landing gear configurations, one deep and one shallow?

MR. LABBE: That's correct.

DR. WIDNALL: And on the two graphs, is that rolling moment and yawing moment?

MR. LABBE: Rolling moment, yawing moment, and we actually tested three depths.

DR. WIDNALL: Okay. Fine.

MR. LABBE: So you're seeing the shallow, the deepest, and then there's an intermediate.

DR. WIDNALL: Okay. So everything is on this single page for these two different kinds of tests or actually three, I guess. Three tests.

MR. LABBE: Three different tests. And the shallowest actually produces the largest change. I think Joe might be able to explain that in the future chart.

Okay. That was just a snapshot of the work we're doing, and we're just getting started on this damages assessment. So my last chart is just kind of a summary. We've looked at these things. One thing that surprised us is when we put this initial damage in the Mach 6 tunnel, we got very small increments and not big enough to explain flight. The CFD suggests maybe there's still something to that. We're going to evaluate those and resolve those differences, apply our higher fidelity tools.

DR. WIDNALL: Well, would that single notch explain perhaps some of earlier part of the off-nominal aerodynamics before you get into the catastrophic failure?

MR. LABBE: Yes, it could. What's puzzling is that if it's also explaining the side wall temperatures, those don't happen until 600 seconds or so.

DR. WIDNALL: Good point.

MR. LABBE: So that's one where we're not integrated with the thermal and so maybe it's not wing leading edge early on or it's a different panel. So we're going to be looking at multiple panels missing and other panels missing, and that's really where our future work is focused, is to first do a survey of the wing leading edge and then start looking at other damage scenarios that try to produce that and then eventually get our higher fidelity CFD analysis tools to get to the actual flight conditions and high fidelity models of this damage.

GEN. BARRY: As you do the piecewise integration, so just your aerodynamic element, just some quick answers. One RCC does not account for what you see. Yes or no?

MR. LABBE: No.

GEN. BARRY: Okay. How about four?

MR. LABBE: To be determined.

GEN. BARRY: Okay. How about a landing gear with an RCC, landing gear down?

MR. LABBE: Landing gear down, we didn't do both; but I guess if you could put them together, landing gear down increments look very similar to just prior to loss of signal.

GEN. BARRY: Okay. Final question is: As I think you told us, if the main landing gear door is gone, the gear is still up, that will not give you enough to qualify, from what you've seen aerodynamically?

MR. LABBE: It would be sometime earlier in the flight, where the increments have not grown to the large level we see just prior to LOS.

MR. WALLACE: Some of your initiating scenarios seem to be distinct. I mean, are you looking at sort of things in combination? I'm also curious as to whether does it remain an issue of *Columbia's* historical wing roughness as a factor.

MR. LABBE: As far as the scenarios, most of the scenarios that have been developed start with a single damage that was relatively small that grew. I believe the scenario team is now, as we bring in some results, starting to rethink some of those, could it have been something more substantial early on; but that's kind of the iterative nature of this evaluation.

As far as the roughness on the Orbiter wing, I think, Chris, maybe you could – from a TPS standpoint, my understanding is that that was recognized and there was a lot of effort to make the *Columbia* wing as smooth as possible by eliminating the sources of that roughness. So it was a very smooth wing.

MR. MADDEN: As far as, you know, the signatures we saw were not anything related at all to any sort of early transition.

MR. TETRAULT: Do any of your future test plans include multiple breaches in the wing?

MR. LABBE: Not right now, but I am open. Our test plans are very fluid. So right now we are trying to – I think the next thing we're going to do after we get the wing leading edge is drill large holes in the wing so you actually have a flow from the lower surface through the upper surface and see what results we get out of that.

MR. CARAM: As well as multiple panels.

GEN. BARRY: A follow-up on Steve Wallace's question. The last STS flight by *Columbia* that had a really early transition from laminar to turbulent flow was STS-73, I think. That was like 893 seconds. Every one after that was pretty nominal. Now, that can be qualified by working the issue and trying to smooth out the wing and in between flows and at the maintenance, is that correct, when we do the OMM?

MR. LABBE: It's either that or – Joe, I mean –

MR. CARAM: I would agree; but when you go back to STS-73, the cause of that that we established is a protruding gap filler. The material that resides in between the tiles sometimes displaces and can reside there in the flow, and it was on the order of about a half inch to an inch in size and sitting about 20 percent along the center line, down the vehicle length. And that we've shown in ground tests, that we can achieve boundary layer transition because of that kind of disturbance.

MR. WALLACE: How do you identify whether there's boundary layer transition? What's the signature?

MR. MADDEN: Well, you would see it on the surface temperature. You would see an immediate rise in temperature on the surface. I was referring to the off-nominal events we saw. Clearly things are happening well before even the earliest transition we've ever seen; and in terms of the roughness, I think what we should do is get you a little report or a white paper on what's been done on the Orbiters to make them smoother.

ADM. GEHMAN: I definitely want to let Steve get off stage here; but I, too, have one more question an that is - One of the first things you said was that you know pretty much about nominal Shuttle reentry aerodynamics – normal. But in my experience, I have experience in aircraft development and procurement – and I won't mention anything specific, but I remember being in a position of authority in the US Navy when an aircraft we were buying had several hundred test flights, several thousand hours of test flying, and we discovered a new, completely new and unexpected aero control problem, which was all in the front page of the papers and everything like that. It caused us a considerable amount of heartache to fix it and convince Congress that we had it fixed. So, I must admit that I require a little convincing that after 113 flights and a few thousand seconds in transition that you say you know a lot about Shuttle reentry and the aerodynamics of all that.

MR. LABBE: I'll offer you one thing and see if this – what makes the Orbiter different from, say, a military aircraft is that while we have a very broad flight envelope in speed, we fly the exact same profile over and over and over again. So, each flight is essentially flying the same profile. We're not trying to expand to this envelope that has very large differences at flight conditions. And we've learned a lot. Believe me, we've had a lot of instrumentation. We by no means had it figured out on the early flights. We did flight maneuvers and so, because of the repetitive nature of the entry profile, along that profile we have it very well figured out. If we diverge from that profile, then what you say is exactly true.

ADM. GEHMAN: I think we better get on to Joe here or whoever is next. Thank you very much, Steve.

MR. LABBE: You're welcome.

MR. CARAM: Page 20, please. Again, this is just revisiting our flow charts. So now we'll be taking about the aerothermodynamics environments.

Next page. This is just a simple chart to try to explain to you the process that we go through when we provide aerothermodynamic environments to the thermo community. Our answers in and of themselves, aren't the final product. We have to provide those to the thermo analysts for the analysis of the structure.

As inputs to us, we need the trajectory conditions, how the vehicle's going to be flying through the atmosphere, its speed and density profile. We also need the configuration – both the nominal and, in this case, what kind of damage scenarios are we assessing.

So as I think Dr. Bertin has already gone through with you, heating is a result of the exchange of kinetic energy of the vehicle to thermal energy in the gas. So you have now high-temperature gas flowing around the vehicle. As it flows around the vehicle, it departs that energy to the surface. So when you consider what you have to do and look at when you're providing aero heating environments, you have to consider the physics and the chemistry of the flow. The physics phenomena, bow shocks, as Steve was talking to you earlier, the shock interaction on the wing, the boundary layer, the state of the boundary layer, whether it's laminar or turbulent and the transition in between the two, any kind of separation zones and reattachment – for instance, the body flap, if that were to deflect down into the flow – the flow upstream of it would separate and then you would have a reattachment point on the body flap where you would see higher heating. So anywhere there's a geometric difference or change, we need to consider that in providing those heating environments.

Chemistry aspect. After I have the physics modeled, we want to take a look at the chemistry. As the air passes through the bow shock, it is heated up to approximately 8,000 to 9,000 degrees Kelvin. At that point the air molecules, the N2, the nitrogen molecule, and the oxygen molecule can split. So they dissociate, and that requires energy to occur. So it's an endothermic reaction.

So now you have these atoms flying around the vehicle. So that changes the chemistry of the flow, and that can have certain effects. You look at the shock angles. The shock angles can come closer to the vehicle. The pressure distribution can change slightly. And you're looking at the difference in heating.

When you talk about TPS environments, thermal protection systems that have partially catalytic coatings on them, you can gain an advantage by not absorbing the heat in the flow field because it doesn't allow those atoms to recombine on the surface. That's called partially catalytic heating, and the Shuttle's TPS is coated with those coatings. So during those times when you have this dissociation, you can gain some advantage in the heating environment. So you have to account for these various physical and chemical phenomena before we provide heating environments for the thermal analysts.

DR. WIDNALL: I'm very interested in this question of surface catalysity, I'm going to pronounce this wrong.

MR. CARAM: Catalysity

DR. WIDNALL: Close enough, I've got some data actually that NASA did and it came out of Professor Bertin's book, but you guys had the courage to run on one of your flights – I think it was STS 2 – where you painted, oh, seven tiles on the Shuttle with a surface catalytic coating, a coating that allowed this recombination of O2 and N2 to occur on the surface.

MR. CARAM: That's correct.

DR. WIDNALL: Roughly speaking, the temperature on the surface of those tiles went up by about a factor of 2 to 3. That's the result.

MR. CARAM: Okay. I would believe that to be true because what you're doing is recovering the energy in the boundary layer.

DR. WIDNALL: Right. And just to get these temperatures sort of on the record, when the gas dissociates from behind the bow shock, roughly speaking, you're looking at a temperature of, what, 3200 degrees Rankine? I mean, that's the temperatures that I got from Steve.

MR. CARAM: At the edge of the boundary layer.

DR. WIDNALL: At the edge of the boundary, there's what I would call stagnation temperature. About 3200, and the reason it's as low as it is because of this, dissociation has taken place.

MR. CARAM: The partially catalytic nature of the material.

DR. WIDNALL: Well, no, you ripped the gas apart, so the temperature's gone down; but you still have this energy potential, should you have a fully catalytic surface, to drive that temperature back up.

MR. CARAM: That's correct.

ADM. GEHMAN: Let me follow on to that. The TPS system, a particularly high reusable system, it's painted to prevent that catalytic action.

MR. CARAM: It's coated.

MR. MADDEN: Reaction-cured glass coating.

ADM. GEHMAN: That's right. But how deep is that coating and is it possible that that coating could be torn or damaged?

MR. MADDEN: Well, okay. The short answer is that every mission there is multiple small damages on the tile. So practically every mission has tiles with coating damaged, which would imply chipped and missing. From that standpoint, the tiles are very robust to survive having the coating missing.

ADM. GEHMAN: What I'm getting at is, that not only does damage to the smooth surface of the TPS create aerodynamic little spots, it also provides an opportunity for catalytic recombination.

MR. MADDEN: Yes. And also without the coating, the tiles suffer from reduced infrared re-radiation cooling effects. So it's a bit of a double whammy, but the bare tile, even though it's not coated, I don't think is very catalytic either.

ADM. GEHMAN: Okay. That's what I was getting at.

MR. TETRAULT: To go back to Sheila's train of thought and inquiry, if you had an exposed wing spar, wouldn't you have a catalytic surface?

MR. CARAM: Before the surface itself oxidizes, yes. But as it heats up and the oxygen penetrates that surface, it will perform an oxidation layer. And Chris has some material on that for y'all today. And that oxidized layer is partially catalytic.

GEN. BARRY: Let me just ask a question on the RCC. At the boundary on the surface of the RCC, temperatures can get as high, between Panels 7 and 12, what?

MR. CARAM: 2950 degrees Fahrenheit.

GEN. BARRY: And how far in front is the boundary layer and what is the temperature, let's say, 6 inches forward of that?

MR. CARAM: Well, as you get to the wing, you're starting to expand over that wing and the boundary layer is getting thinner.

GEN. BARRY: At the edge of the boundary layer, what's the difference in temperature?

MR. CARAM: Probably around 3,000 degrees Fahrenheit, so not significant. Not a significant difference in the edge of the boundary layer.

GEN. BARRY: Maybe I'm asking the question wrong. When you get in front of the boundary layer, what is the temperature? We were told at one time it may be as high as 10,000 degrees.

MR. CARAM: I'm sorry, yes, the gas temperature can be as high as between 9 and 8 thousand degrees Kelvin.

GEN. BARRY: So you go from the edge of the RCC to just 6 inches forward and the difference is almost 7,000 degrees.

MR. CARAM: That's correct.

GEN. BARRY: Okay. Now, if you get a nick or a little bit of damage to the RCC and you have this recombination that you just discussed, does that bring that 10,000 degrees closer in and reduce that 6 inches?

MR. CARAM: No, it does not. It's what the available energy is in the boundary layer itself. It's not bringing that shock layer closer. It's just how you exchange the energy in the boundary layer around the vehicle. So at the boundary layer edge, you're seeing around maybe 4,000 degrees Fahrenheit; but that is also changing as you go down through the boundary layer.

DR. WIDNALL: Wait a minute. I've got a question. The material that John is talking about, if the leading edge is damaged, is carbon. Carbon reacts chemically with the available oxygen and that will, in fact, release –

MR. CARAM: I didn't understand that he was mentioning –

DR. WIDNALL: Yeah, he was talking about a damaged leading edge.

(To Gen. Barry) I think you were. Weren't you talking about a damaged leading edge?

GEN. BARRY: Exactly

MR. CARAM: I misinterpreted his question. This is really more in Chris' area, but you could start oxidizing the carbon and that can result in the carbon receding or ablating.

MR. MADDEN: An uncoated carbon panel – I think that would have been briefed on this – an uncoated carbon panel will oxidize because the carbon's going to react with the oxygen. And it's quite rapid, but as far as surviving a mission, I think, even though you get some damage, in most cases you don't eat through the entire thickness of the carbon. There's catalysis and oxidation on top of each other.

MR. WALLACE: Did you see that in your observation of the debris in Florida?

MR. MADDEN: No. The debris in Florida is – we don't know what happened when there.

DR. WIDNALL: I have another question.

MR. MADDEN: But there was a lot of bare carbon that looked fresh and shiny. It didn't look like it had been oxidized very much at all.

DR. WIDNALL: You seem to be using the word "oxidation" and "oxide" as if it forms a protective coating. Another word for oxidation is "burning." I mean, the experiments that I've seen that NASA has done indicate that damage to the leading edge of a carbon-carbon burns a hole completely through the carbon-carbon structure.

MR. MADDEN: An existing hole would grow, and then a damaged panel would oxidize the bare carbon and eventually would grow a hole.

DR. WIDNALL: Yeah. You would eventually get a hole in the carbon.

MR. MADDEN: It depends on which panel you're talking about and how rapid.

DR. WIDNALL: The only question we're talking about is: What does eventual mean? How many seconds is eventual? That's what we're talking about.

MR. MADDEN: We performed analysis for the investigation on panels with existing holes and how fast they grow and how fast they eat away at the spar.

DR. WIDNALL: I realize that you're going to present later; but as we're talking about this thermal environment, I would also raise the same question with respect to aluminum. I mean, it certainly is true that in our common experience of aluminum, oxide is a protection for aluminum. Otherwise we wouldn't have airplanes and we wouldn't have chairs and all the other things that are made out of aluminum. But aluminum oxide at a temperature of 3,000 degrees Fahrenheit is not a protection. The melting point of aluminum is 700 degrees.

MR. MADDEN: Right. It's going to melt and go away before you see that effect.

DR. WIDNALL: Yeah, very quickly. It is not a protective coating for aluminum at the kinds of conditions we're talking about, and I think that is a subject we want to pursue in more depth.

MR. MADDEN: Well, we've got a chart or two on that, as well.

ADM. GEHMAN: Okay. Board, let's let them present.

MR. CARAM: Next page, please. Page 22. Just to go over some of the models and techniques we're applying in order to provide these environments. The Orbiter has an existing external heat data base that we're using to provide the local heating around the various damage sites that we're considering. We're also using a plume model that was developed for micrometeoroid penetration, so I mean small penetrations on the Orbiter. However, for total environments, both the convective and plume, the models don't exist for the size and scale of damage that we're considering. So, we are having to develop those techniques as we go.

We're also using engineering analysis or correlations that we have available to us, and I'll show you an example of that on the following page when we're dealing with cavity flow heating. We're also using what we have for existing computational solutions on the Orbiter. We have the orbital experiment data from STS-2 that's been calibrated with the computational data. We also have pre-use test data.

We're also using, as Steve described earlier, the current activities at Langley and the wind tunnel testing that we're doing to look at the local heating environments as a result of damage to the early metal. What we're trying to do with the more high fidelity tools such as computational fluid dynamics is to verify those environments because we are going through different environments as we're coming through the atmosphere. Early on, it's more applicable to use a direct breakthrough as the Monte Carlo technique; and since we are assessing damage that existed, we're assuming, at entry interface, you want to verify that the heating environments that we're providing are accurate in those regimes.

So the following page gives you an example of the cavity heating models that we're using. The cavity heating – for instance, many of you have one tile lost or three tiles lost. The heating down in that cavity will vary, a function of the length over depth ratio. And that ratio changes the heating. If you have a ratio of 14, over 14 you have a closed cavity and under 14 you have what's called an open cavity flow. It does not say it's penetration. It's a description of a flow inside that cavity. So with open cavity flows you tend to have less heating on the floor than you do with closed cavity flows because with closed cavity flows the flow has the opportunity to reattach to that floor and then start heating up the floor there before it separates again and reattaches on the outside of the cavity.

You also have to consider whether the boundary layer is laminar or turbulent upstream. That can change how much energy is being provided inside that cavity. So, it could change the types of coefficients you're using. Typically, you apply coefficients down the cavity and you assume upstream is the nominal heating. So you have the nominal heating factors times the cavity factors, and that's how you derive your heating.

Most of this data was established with 2-D environments, 2-D testing. There's some data with three-dimensional effects, but that data is just along the center line of the three-dimensional object. Why I mention that is because if we're assessing cavities on the carrier panel tile areas, that flow is sweeping outboard on the wing leading edge and it's highly three-dimensional. There's a lot of cross-flow. So again, I want to be sure that the environments we're providing are accurate.

So, the next page is an example of how we're doing that. Again, this is the schematic of the open cavity flow typical for a single lost tile. On the right you see a close-up view of the pressure distribution from a CFD solution from an STS-2 CFD solution using the LAURA code at Langley. Forward, the nose is this direction. Outward is the wing. You can see the outline of the main landing gear door. The symbols in red are higher pressure. The blues are lower pressure. And the high pressure in this region is a result of the shock interaction zone. So you have a higher pressure leading up from the leading wing edge and then flowing inboard and aft from that region.

So, we take information from the external flow field and provide that as input conditions to a cavity flow solution. And this solution here is a direct simulation Monte Carlo solution of 2-D cavity flow at high altitude. Why I wanted to present this is because what the direct simulation Monte Carlo is doing is giving you an indication of what the high altitude effects are doing in your cavity flows. So you can see it's almost a merge between what you have for open cavity flow, between that and a closed cavity flow. So we want to know that information in order to make sure our heating environments that we provide the thermal guys are accurate.

Next page. This is an example of the wind tunnel testing we've been conducting at Langley. These particulars runs are from a Mach 6 air facility. I will be showing you runs from the CF4 facility. Again, as Steve mentioned, we've been looking at notched wing leading edges. On the left, you see a nominal configuration Orbiter, a side fuselage heat transfer. This was done with the infrared system at Langley. In order to acquire heating rates, we measured the temperature, assume a short delta time in the tunnel where the image was taken, and then 1-D thermal analysis to back out the heat transfer coefficients.

The two reds dots indicate the side-wall fuselage temperature measurements that showed off-nominal behavior. The red zone is the shock interaction zone on the wing leading edge, and this area here is the attachment of the flow coming around the chine of the vehicle, scrubbing along the side of the vehicle. So this is what it pretty much looks like in a nominal configuration.

When you take out Panel 6, as Steve showed you previously, you then have this shock impinging on the side fuselage. In this case since we're in the air facility, so we're at Mach 6 at air, you see that it doesn't show that it interacts with the sensors at this location.

So we also took a look – next page – at Panel 9. Again, here is Panel 6 in comparison to going further out on the wing, removing Panel 9. Again, Panel 9 is in the region of the double shock interaction zone. So not only do we have the effect of Panel 9 but you also have the effect of the higher energy because of that double shock interaction zone. So can you see between the two that Panel 9 moves the disturbance further aft on the vehicle.

DR. WIDNALL: You said these were Mach 6?

MR. CARAM: These were Mach 6 at air.

DR. WIDNALL: Okay. I mean, at Mach 20 those shocks are going to lean over.

MR. CARAM: Next page.

DR. WIDNALL: You got it.

MR. CARAM: In order to do that, we're first using the CF4 facilities; and we're also using our computational techniques, as well. As we talked about earlier, this is a comparison between the air facility Panel 6 and Panel 9 to the CF4 facility, which simulates the high-temperature gas effects. Again, what we're trying to do with that by changing the gas is to model the high-temperature gas effects; and what you're getting there is that the shocks are moving closer to the boundary, to the body. The pressure distributions are changing slightly, and this is the result. So you see that even for Panel 6 you see the heating – or in this case this is just a temperature map. This is qualitative data only at this point in the analysis, but the high-temperature area moves slightly aft from Panel 6. With Panel 9, it moves further aft and the distribution changes. So you're getting the effect of the simulated high-temperature gas in this facility and at this point you can say that Panel 9 shows the influence over those gauges.

ADM. GEHMAN: Joe, speak about heating forward.

MR. CARAM: Okay. We really aren't seeing any changes forward of these damaged locations, other than this flow right here. Forward, where the vent nozzles are, you're not seeing any changes where those are occurring. Now, you have to realize when you're doing this experimental technique you're taking snapshots of the image right after the model's inserted into the tunnel. These imaged times can vary. The model baseline temperatures can vary. So you might see small differences in the reduced heating that you get out of the test, but in this case we're not seeing hardly any changes as expected within the uncertainty of the test techniques aft forward. Most all the effect is on the side wall and aft.

DR. WIDNALL: Did you go above Mach 6? That's my question. My question is a geometric question, not a real gas question. If you were able to – and I understand the limitations of tunnels – if you were able to run such an experiment at Mach 20, your shock would be way leaned over from Mach 6 geometrically.

MR. CARAM: No, because the –

DR. WIDNALL: Are you saying it gets into a Mach number independence regime –

MR. CARAM: At a point. But then you have the chemistry effects that take over. So those chemistry effects will change your Mach angles, your bow shock angles. So it's not going to change significantly. When we obtain heating data in both these facilities, it matches within flight within 15 percent. So you're not seeing a large change in the way the flow is flowing around the vehicle. It accurately models the hypersonic flight environment.

MR. TETRAULT: Would you bear with me a minute because I don't know much about wind tunnel testing. I know nothing. So, let's start from there. What you're doing is looking at the external or exterior environment here. Can you use the wind tunnel test to test the internal environment? Like you just put a notch in the wing. Can you go up and down the wing and see what the thermal conditions, say, inside an RCC panel is, using this mechanism?

MR. CARAM: This is the scale and type model we are testing.

MR. TETRAULT: Well, you could drill holes in it, right?

MR. CARAM: We could drill holes through the wing, but it would be very difficult to obtain the heating and the proper scaling inside that area, on a larger scale? Possibly.

Next picture. Next page, please. All right as a follow-on, again, we're trying to verify these environments; and we're using the higher fidelity techniques. This gives you an idea of where we're at currently in this process. We've established a common service grid. Since we have these multiple organizations working on this problem, one of the issues with computational fluid dynamics is that we can have differences just because of the grid topology. So we've established a common one between all the organizations, and so all the organizations will be using a similar topology.

We can use that same grid system to implement or embed damage in various locations on the wing leading edge, along the fuselage of the vehicle. And we'll be using those to provide and verify the environments for the damage scenarios. So we can do both the nominal geometry and damage. We're also continuing to do the wind tunnel testing both in air, as an initial screening, because that facility is able to turn around the tests faster than the CF4 facility, so we'll do initial screening in air and then go to the CF4 facility to observe the simulated high-temperature gas effects.

So out of this, we get not only updated heating environments going to the thermal analysis group but we also provide inputs to internal heating environments. We have the outside boundary layer conditions at the local areas where the damage or breach is occurring that we're trying to model. And since we are accurately trying to provide the heating distributions, as a by-product you have the pressure distributions and from there you can provide the aerodynamics. So we can provide that information to the aerodynamics communities for the various damage configurations that we're looking at.

DR. HALLOCK: Depending a lot on the CFD and also the other types of models here – and you're sort of referring to them as being the truth of what's going on – how do we know these models are actually predicting or calculating what's actually going to happen?

MR. CARAM: We're using the wind tunnel data, as well. So what we're trying to do is calibrate, for instance, at the Mach 6 conditions; we want to run those conditions, as well. If you can establish that you can correlate well with that data, then by changing your free stream Mach number and adding the chemistry in, we feel confident that we can get the accuracy that we need. We'll also have to do grid resolution studies, so to make sure that there is no grid sensitivities in the solutions that we obtain out of the CFD.

DR. HALLOCK: Do these models include the chemistry effects also –

MR. CARAM: Yes.

DR. HALLOCK: – or are you actually adding that upon the normal solutions?

MR. CARAM: No, they're embedded into the solutions.

Now, we've talked about the external environment. I want to move on to the internal environments. This is a more difficult, I believe, and less established approach. Now, I know this is a busy chart; but it's actually quite simple to go through. It just gives you a road map of how we're trying to handle the internal environments.

Again, one of the customers for the internal environments group is the external environments, so they feed right into the internal environments group. What the internal environments group does is provide heating environments not only for plumes, but looking at, beyond the plume flow field, what is the internal convection inside the wing, where is the energy being distributed inside the wing and the wheel well. To do that, we're requiring several phases of the analysis.

We've already provided this 1-D heating methodology. This is a plume model that gives you the heating along the axis of the plume only. It's fully equilibrium heating. So it's going to be the worst-case heating and also captures the turbulent reattachment. So it is the worst-case heating as far as plume heating is concerned; but in order to look at the various scenarios, we need to have models that provide off-axis heating. So you have to assess whether, if your plume's not impinging directly on the object that you're worried about – for instance, the wire bundles – we have to provide heating environments off axis. So that's what this is attempting to do, and we'll be updating our models for that.

Then there's other kind of configurations of plumes. You have wall-bounded jets. So there's a jet orifice that is immediately adjacent to a wall. So the heating along that wall is going to be different than what you would see with an asymmetric plume.

DR. WIDNALL: Can you tell me how you would do the calculation of a flow impinging on a flat, bare aluminum plate that is, in fact, a leading edge spar?

MR. CARAM: If we can go to the next chart, I think I can try to do that. Basically what you're looking at is a description of a plume entering, for instance, the interior area or the spar of the vehicle. On the outside, you have the boundary layer. Then you have this external pressure. It's that external pressure in combination ratio to the internal pressure, which will obtain what is your geometry of your plume. And this plume can exist, this core environment can exist up to 20 diameters or greater, 20 whole diameters or greater downstream. And that's where you're getting your high heating area.

DR. WIDNALL: Roughly speaking, what is the stagnation temperature of that jet and what is the gas composition?

MR. CARAM: Again, well, it depends on what your external conditions are and how big the hole is. So a large enough hole, you can probably swallow the entire boundary layer. So you can have gas temperatures up to 9,000 degrees Kelvin entering –

DR. WIDNALL: Then you're assuming the gas is not dissociated.

MR. CARAM: No, it can be dissociated at that temperature. It is dissociated at that temperature. It requires that temperature for dissociation.

DR. WIDNALL: Right. But the outside gas, the stagnation temperature is basically 3200, based on the fact it's already dissociated.

MR. CARAM: But if you're swallowing the entire boundary layer and beyond that, you can get basically the post-shock gas temperatures.

DR. WIDNALL: Anyway, order of magnitude. Fine. Okay. So you're saying that you could have a dissociated gas flow at a temperature of 9,000 degrees Kelvin hitting some structure.

MR. CARAM: Yes.

DR. WIDNALL: Then what boundary condition would you assume for that structure?

MR. CARAM: As far as the chemistry is concerned?

DR. WIDNALL: Yeah, as far as the chemistry is concerned.

MR. CARAM: We're applying equilibrium heating. So it's fully catalytic.

DR. WIDNALL: Okay, and reactive.

MR. MADDEN: Not right now.

DR. WIDNALL: Not right now. Okay.

MR. CARAM: At this point when you have fully catalytic, you're obtaining all the heating from the chemistry that you're going to –

DR. WIDNALL: So assuming no chemical reaction.

MR. CARAM: No chemical reactions with the material. That's correct.

MR. TETRAULT: Is one RCC sufficient to, as you said, swallow the boundary layer, the entire boundary layer so that you're getting the 9,000 K in?

MR. CARAM: I would say so.

MR. MADDEN: Just because you swallow the entire boundary layer – you still have to transfer heat from that gas. So, just because the gas is 10,000 degrees doesn't mean this surface it's impacting is 10,000. That heat has to be transferred via another boundary layer.

DR. WIDNALL: You also have stagnation, which is going to raise the heat.

MR. MADDEN: It still has to transfer the heat.

DR. WIDNALL: Yes, but it will raise the temperature. The stagnation will raise the temperature; and then you're, I would say, halfway there.

MR. MADDEN: I don't understand. What you do you mean, halfway?

DR. WIDNALL: Well, if you stagnate a high-speed jet, you're going to get an increase in temperature.

MR. MADDEN: Correct.

DR. WIDNALL: Then the viscous process that transfers through the boundary layer –

MR. CARAM: It's true. It's almost like having another bow shock.

DR. WIDNALL: Yes, exactly. It's like having another bow shock.

MR. CARAM: Agreed.

DR. WIDNALL: So it's an internal reentry problem, unfortunately.

MR. CARAM: Which again, on the scales that we're talking about for this type of damage, we're having to create these models because if you have a large enough damage – for instance, in this picture you have, eventually you will get turbulent mixing with the available or ambient flow in the cavity; but if your hole is large enough or you're close enough to the structure, you can have underdeveloped plume heating and that can be on the order of two to three times higher heating than you would see with a fully developed core flow. So again we're building these models. We're updating them for these phenomena for off-axis heating and for wall boundary jets. So these tools are in work, and we provide those environments to the thermal community.

Next chart, please. Part of this analysis also involves, outside of the plume environment, where is the energy going inside the wing. Currently we're using the Orbiter baseline venting model to provide that information. You have the various vent locations in the fuselage, in the mid wing going aft to the aft wing and then out the spar. You also have the vent going into the wheel well.

What this doesn't provide us is information on what the high-temperature gas effects are because now that you're ingesting high-temperature gas, it can change the way the mass flow is being distributed inside the wing and the fuselage. So what we do is, in conjunction with thermal analysis that Chris has been doing, we can get an idea of where the energy is being distributed inside those volumes. We're also looking at the possibility of what we call unmodeled vent areas such as drain holes or gaps between closeouts. To the venting guys, these are just bonuses; but to us it's critical because that will determine where the mass flow is going inside the vehicle and where the energy is going. Our colleagues at Marshall are developing complete Orbiter venting models that account for these high-temperature gas effects using a quasi approach. It's not modeling the chemistry precisely but if you're changing just some of properties of the gas as it goes through the volume. The idea with this is that we can then capture the phenomena and then couple it with a thermal model so we can get an idea of how that energy is not only being distributed inside that volume, but also being deposited onto the various surfaces.

Next page, please. This is an example of that. This is a thermal model of the internal wing. You have the truss structure and the spar areas. Each of those are being modeled thermally, and coupling that with a venting model will give us an idea of where the energy is being distributed. We need this in order to reduce the number of scenarios that we have. Yes, we can burn through a wire bundle; but where is the rest of the energy going? We have sensors inside the wing, the fuselage, that don't respond. So we're using that not only to test against the data that went off nominal but to test against the data that remained nominal until LOS. So it gives us a way to differentiate the different surfaces. So we're coupling this model of the mid wing and aft to a wheel well model in the forward glove, and this is being done at the Marshall Space Flight Center.

Next page. Again, this is just a summary of the forward plan. I pretty much discussed all the items here and where we're headed. We've already provided a simple plume model to assess heating at the core. We are expanding that for off-axis heating, taking a look at different types of plumes. We're using as calibration these benchmark cases you were mentioning earlier, Dr. Hubbard, to verify that the modeling that we're doing is accurate before we're applying the flight conditions and then using that information to upgrade our engineering model.

So we're not applying the CFD directly, we're using it to build the engineering model so they can apply it in the thermal analysis. We have the wing-venting model coupled to a thermal model in work. We're also looking at CFD of the wheel well so we can get an idea of what the internal flow structures would be when you have a penetration of the main landing gear bay.

ADM. GEHMAN: It seems to me that this is a real challenge because in the case of the external thermodynamic heating models that you do, you have the aerodynamic forces to bounce them against. In other words, you've got kind of a check and a balance here.

MR. CARAM: Exactly.

ADM. GEHMAN: But internally, you've got no check. You've got nothing other than the temperature sensors. It's a one-dimensional theme here. And you could hypothesize any internal rearrangement of those spars and sturts and thin aluminum walls in there and you've got nothing to check it against. Other than the heating scenario, you don't have a second scenario. And as we have hit on pretty hard here, once you get the very, very hot gases in there, the aluminum doesn't stand up very long.

MR. CARAM: No.

ADM. GEHMAN: So, you could make yourself a new thermodynamic path in seconds and you've got no second part of analysis to check that.

MR. CARAM: That's correct. That's why we think that these temperature plots and our interpretation of them is important in how we define our scenarios. We have the first bit rise as indication to us that there was a breach, but later on you have a rapid rise in those temperature measurements. At that point, we are saying there's a breach inside the wheel well so that the hot gas has penetrated at that point. So that's just the various parts of the piecewise analysis that we're doing.

ADM. GEHMAN: Are you finished?

MR. CARAM: Yes.

DR. WIDNALL: Can I have a question? I just wondered at what point in your CFD analysis would you allow the aluminum to interact and react with the dissociated gas.

MR. CARAM: I don't think we have currently models to account for that in the computational area.

DR. WIDNALL: Do you have the resources to find out?

MR. CARAM: I'm working with some of the folks at Boeing Huntington Beach who are looking at the combination of the heating and the thermal response.

MR. MADDEN: We're going to get a group of guys together to go and address that. Now, I don't think it's coupled with CFD per se, I will be we're going to look at hole growth and the effects of oxidation, any possible –

DR. WIDNALL: This is obviously an extremely difficult area. I mean, nobody would ever build a reentry vehicle out of aluminum. So clearly you're trying to do the kinds of calculations that we have just never thought about doing. There are some resources. In fact, a lot of this early work was really done by NASA Ames. A lot of the expertise that exists in this area belongs to NASA.

ADM. GEHMAN: Okay. Before Chris gets started, I'm going to declare a ten-minute break here so we can pay attention. For the members of the press in the room, please, this is not a press conference. So leave them alone. You all are excused for ten minutes.

(Recess taken)

ADM. GEHMAN: Gentlemen, thank you very much. We are not concerned about time up here. We've got to get this right, and you're a great source of information. So the only time constraint I have is that we don't want to overstay our biological warning signs that we're not paying attention anymore. So thank you very much for bearing with us.

Okay. Chris, you have the floor.

MR. MADDEN: My name is Chris Madden. I'm in the thermal design branch. I just wanted to start off with a summary of what we've been doing. Our branch has been part of this investigation, performing thermoanalysis and support of test planning and analysis.

What I'm going to show you is a series of preliminary results. The first several slides, you'll start to see that, with enough damage, you can breach the vehicle in several different ways. And this is the way we attack the problem in the first few weeks of the investigation is: Hey, can this damage blow a hole in the wing? Can this do it? Can this do it? And the answer always kept turning out that, well, if the damage is big enough, sure, big enough damage is always going to breach the wing. You'll see some of that in the slides.

So I just want to caution everybody that if you see a slide that says a hole burned through in 500 seconds, it doesn't say that's it; it says that could be it. And what we've done is evolve from that and after getting frustrated with everything shows that it could be the culprit, we started going to this plan where we're saying, look, okay, while the configuration is semi-stable before we have the debris shedding a little before 600 seconds, what can we learn or what do we know.

So, there are several knowns that we've had to make engineering leaps in saying that, okay, at 488 seconds when we saw our first bit flip that was the breach. So that's a time hack we're going to have some level of faith in for the time being so that we can perform some analysis based on that. Based on that 488 seconds, 42 seconds later the first measurement was lost. So, I'm going to show you a plan on how we're going to take that 42 seconds to determine where the damage site was and how big it was. We've also got another time hack at the wheel well temperature rise. We're going to say, okay, our engineering leap is that was breach of the wheel well. So now you've got 12 seconds between the first breach in the wing to the breach in the wheel well, and we'll try to figure out how that happened.

ADM. GEHMAN: I think that the board understands the assumptions you're making for the purpose of building a mathematical and an engineering model of what happened, but I can assure you we don't necessarily agree with those assumptions. What I mean is the breach could have occurred two weeks before that.

MR. MADDEN: Sure. And it certainly didn't happen after.

ADM. GEHMAN: We understand the mechanism of why you've got to pin something down so you can do the analysis. So we're with you.

MR. MADDEN: Okay. I appreciate that.

Okay. So the next slide, this is part of the energy balance stuff we did at the beginning. I'm going to show you a series of slides of what we've done. This is explained, the early bit flip or a small temperature rise on the brake line. The analysis assumed here that you boil a hole, and here we did it at 480 seconds. This is the amount of energy in BTUs per second that enters into the wheel well.

Okay. The next slide shows the predicted based on that energy coming into the wheel well via the healthy vent would, indeed, see a temperature rise on the same order of magnitude that we saw in the flight data. So the shorter answer is that, yes, a sudden ingestion of hot gases into the wing, flowing into the wheel well, would be indicative of the bit flip that we saw on that very first measurement. So this is kind of lending credibility to something happened at 488. Now, agreed, it could have happened earlier and you're just now seeing the heat coming in because the gas, although as I think we discussed before, has a high heat transfer rate to the surface, the amount of mass involved in the gas is low and therefore the amount of BTUs the gas molecules can contain is low. So you may not see the temperatures until this time, anyway in the wheel well.

MR. TETRAULT: You're using just a 5-inch diameter vent hole to calculate this? You have not added any of these additional transfer patterns?

MR. MADDEN: Right, this is a healthy wheel well assumed. The other thing you see from this analysis is that, at least for this measurement, later on you're going to need additional heat to explain the temperature rise. There is another measurement here on this poster that it start going up at about 600 seconds. There's some other ones that begin rising at 600. For some reason this brake line was delayed a little bit. This was behind a fiberglass cover, so that could explain that.

DR. WIDNALL: Could I just raise a question? Sort of philosophy, could you back up one slide. I mean, I think this is the point where one then needs to begin to challenge the model because you have a conclusion on this slide; and your conclusion is additional heat is required to explain the flight data. So I think that's a point at which we need to challenge the model because then I would ask the question: Does your model include a directional jet or is it what I would call a heating and vent kind of analysis that you would use if you were trying to build an air conditioning system for your house? It's kind of a different kind of analysis.

MR. MADDEN: And this model is certainly challengeable because this is an engineering method where we just broadcast. All we know at this time is that this amount of BTUs per second came into the wheel well. How it's distributed, we have to wait on CFD. So at this point all I was trying to say was: "Can be explained."

ADM. GEHMAN: Maybe I misunderstand, and I'd like to understand it. What I read from this, though, Sheila, is that this graph supports your position. What I mean is that just by the model he has here, which he has a healthy wheel well with nothing broken except heat's getting into it, works for a few seconds but then after that it doesn't work anymore.

DR. WIDNALL: Right. No, I think that's right. It's just that when you see something like this, you really have to make sure that you understand the model and that it's pointed out that the model itself is the simplest level of calculation that one can do.

MR. MADDEN: Sure. Excellent point. This is a very simple energy balance type analysis.

ADM. GEHMAN: One of the things that I'm really interested in, of course, is that I'm interested in the very first off-nominal reading.

MR. MADDEN: And this is it.

ADM. GEHMAN: I understand that, but you have little red dots here that show flight, actual telemetry data. Of course, you didn't put all of them on there; but you've been monitoring that temperature for days.

MR. MADDEN: Right.

ADM. GEHMAN: So the point is that you started here at – this is EI. Is that correct?

MR. MADDEN: Correct.

ADM. GEHMAN: So you started here because that's kind of where the interesting part is.

MR. MADDEN: Right. It had been decaying down slightly; and you see that in this plot, too.

ADM. GEHMAN: My question, though, is that because these temperatures were essentially nominal and even though you are in extraordinarily thin atmosphere with very few air molecules, the Orbiter is heating up out here.

MR. MADDEN: It's heating up on the outer surface.

ADM. GEHMAN: Yes. Where does peak heat start? Do you know where peak heat starts?

MR. MADDEN: At about 300 seconds. Okay. What you're seeing is inside the well wheel.

ADM. GEHMAN: I understand that; but I'm saying even back here at 200 seconds or 250 seconds and 300 seconds, even though you're not at peak heating, as the orbit decays, as the Orbiter comes down, the heating increases. External heating.

MR. MADDEN: Okay. Correct.

ADM. GEHMAN: And so what I'm trying to get at is whether or not we should feel that whatever access allowed the external heat to get in, whether it was a preexisting condition or whether it started – whether that access opened right about here.

MR. MADDEN: That's challengeable. Whether or not that this was the first bit flip just because – the hole was there the whole time in the wing and you just see the bit flip just because that's the period of time it took for this low-density gas to raise a high-density brake line to 1 degree.

ADM. GEHMAN: Or, if just 1 or 2 seconds or 10 seconds before here is when the fault manifested itself.

MR. MADDEN: Right.

ADM. GEHMAN: We don't know.

MR. MADDEN: We don't know; and that's why we're making these assumptions, to see if the whole story fits. If it doesn't, we'll have to revisit everything.

ADM. GEHMAN: Are we on the same sheet of music here? In other words, in my mind I don't know. And, of course, it bears on a lot of things because if the fault just manifests itself right here, even though the aerodynamic pressures are practically nothing but might be enough to remove something or cause something that was weakened, then all this stuff about on-orbit photography and stuff becomes irrelevant because if there was no fault that you could see – I mean, it was a weakness clearly and something failed. So, I mean, it's important to know whether or not the Orbiter had a preexisting condition that started, you know, way back over there, which then didn't manifest itself heat-wise until you got enough heat.

MR. MADDEN: Right. There is another piece of analysis that we don't have in our charts that we did make that assumption that, okay, let's say the hole was there the whole time. Those transients, the analytical transients didn't really jump up. There's no reason for them to jump up at that time. In fact, it wasn't enough heat for them to really respond until out here at 7 or 8 hundred seconds. So, that's another little piece of data that kind of suggests that something happened there. I'm not saying it's a fact.

ADM. GEHMAN: Right. If I could ask Steve a question here, back to this first graph over here. You say that these numbers – 'cause they're ratios and they're ratios of irrelevant numbers at that particular time – but because of this bias that the Orbiter had in its control surfaces, where, compared to 400, 300, 500 seconds after EI do you start believing your own data?

MR. LABBE: For what's on that particular plot, I would say it's more like 500 plus seconds.

ADM. GEHMAN: Yeah. So it's right in here.

MR. LABBE: It's close to that, but maybe a little bit further, maybe another 20 or 30 seconds after.

ADM. GEHMAN: So even though we have an indication of temperatures, we have another indication of what the aero surfaces were doing that are to the left of whatever, this 480 seconds after.

MR. LABBE: So it's close. I would say if you look at that plot where you see the downward trend where you see the slope really go away from zero, right there, that's where I'm saying I have a clear indication. What's happening before that...

ADM. GEHMAN: That horizontal but left bias, you're less confident.

MR. LABBE: I'm less confident because when I did STS 109, I got very similar results.

ADM. GEHMAN: Very similar things.

MR. TETRAULT: Would you help me with regard to bit flips – I'm going to go back to this – that's the indicator that shows that you're going off nominal. Can you tell me, off nominal to what? Is that the average for that STS for that Orbiter in terms of prior history? Is it average of the entire fleet? What is it off nominal to?

MR. LABBE: It's off nominal, to what would be to our data base, which is for the entire fleet. So it's off nominal from previous flights. Now, we haven't gone back and applied this analysis to every single flight; but what you would expect to see again is even if you had a slight bias down like that, was that that would stay there, maybe drift back towards zero. It's not going to get significantly away from zero.

MR. TETRAULT: This is important because a slight change in when you make a call of what's off nominal can change the entire time line of where the heat is coming from. So I would like to continue to explore this just a little bit. In terms of when you make that call – and I've looked at some of these plots that we have and they appear absolutely straight to me and all of a sudden there's a call that it's off nominal – how accurate do you feel that call that it's off nominal is?

MR. LABBE: Okay. I think what we've done and what's not shown here is you look at the rolling moment, you look at the aileron response, you look at the side slip –

MR. TETRAULT: I'm talking about off-nominal calls on just temperature sensors.

MR. MADDEN: Well, the previous missions have – they've always kept decaying down. Although the surface of the wheel well on the door is being heated to very high temperatures, that heat soaked back into the structure of the door and then, via radiation, into the brake lines. It doesn't occur until much later. So this we're pretty confident was a beginning of off-nominal event. There have been bit flips before, but they've always kind of came back down. So the typical response is a downward trend and you might see a flip up but it would come back down and stay down.

ADM. GEHMAN: Of course, you have the same measurement in the right wheel well.

MR. MADDEN: Correct.

ADM. GEHMAN: Which doesn't show anything like that.

MR. MADDEN: Right. And it does the typical decaying down until much later.

Next chart, please. The next few charts are the quick assessment of how extensive the tile damage would need to be to burn through the skin of the wing. In this case we can predict, and what you're seeing is temperature versus time for the outer face sheet and inner face sheet of the sandwich. Our simulations can predict the burn-through in this case is late.

Next slide, please. This shows it on the landing gear door; and this, based on the configuration of the structure itself and the heating rates and heating factors and the size of the damage, it's earlier. That's more around the time where the breach was observed. I'm not saying it's the door. I'm not saying it's the wing. It's just showing that it's highly dependent on the damage you have to assume. Like I said, at some point there's going to be enough damage to burn through the wing.

Next slide.

GEN. BARRY: Chris, let me ask you a question on temperature inside the wheel well. What's your best guess, if you have any, of the temperature getting about 700 degrees? The reason I'm asking that question is the pyrotechnic inside the wheel well is supposed to be cooked off at about 700 degrees.

MR. MADDEN: There are massive pieces of structure from the flight data on the strut actuators that don't rise over, I think, 120 degrees or so. You would think that the pyro would be the same order of magnitude. We will have a chart. I'm very unsure of the math model. We have a math model of an entire wheel well, and we will confirm that the pyro didn't go early.

GEN. BARRY: Did not go early.

MR. MADDEN: Right now I think it's very unlikely.

DR. WIDNALL: I have a question. You did a calculation of burnthrough of the skin, and obviously what's the skin made out of?

MR. MADDEN: Aluminum.

DR. WIDNALL: You know what I'm going to ask. What sort of boundary condition did you use for the surface catalysity and/or reactive behavior of the aluminum?

MR. MADDEN: The reactive behavior was not simulated. There's no oxidation for those analyses.

DR. WIDNALL: So you basically got a melting hypothesis as opposed to burning.

MR. MADDEN: Right. And thermomechanical effects were not simulated. So we're just trying to see can you get to the melt temperature; and, of course, you can.

Okay. This is analysis of the thermal barrier and pressure seal around the door, if the tile adjacent to the thermal barrier is severely damaged and you basically expose that cavity in the pressure seal to the external environment. You see two different assumptions here, but basically they both do the same thing. The pressure seal will fully demise a little before 500 seconds. So again, bad enough damage, you can breach the wing. And this one is via the wheel well. We're not concentrating on this one so much anymore because of the timing between the wire burn and the pressure or temperature rises seen in the wheel well.

Next slide. Okay. This is analysis to explain the side wall temperature rise. What we did here is at 600 seconds we applied ten times the normal convective heating environment to the exterior of the TPS in this region; and that, we actually back-calculated it ten times. That shows that the analysis can predict the flight data with ten times the heating rate to the surface. That ties into what Joe's studies have done. His team has shown that you get a bump factor two to ten times. This is at the upper end of that, but it's the correct order of magnitude and in the same ballpark. So the conclusion here is that this could be explained by external heating due to shock hitting the side wall.

GEN. BARRY: But it could be explained by convective heating.

MR. MADDEN: Internal convective heating. There is enough heat, if it's distributed to this zone. We don't at this point know how the air flows within the mid fuselage and whether or not it would make it back to this region and heat the back side of the sensor, but certainly it is possible.

DR. WIDNALL: Can I ask a question? Is it also, I know I could not do these calculations myself, but are you also considering thermal conductivity through the structure itself?

MR. MADDEN: Right.

DR. WIDNALL: So that's part of it.

MR. MADDEN: Right. We looked at that, the conduction effect. We looked at a very hot wing, can it conduct up to this sensor quick enough to see this response; and it really couldn't conduct fast enough to see this. So conduction was ruled out.

Next slide, please. Okay. This is just to show you that we've also been looking at leading edge damage. The Huntington Beach guys have developed math models of damaged RCC in support of the micrometeoroid studies that were performed. They've used those techniques to assume a hole size and then they simulate the thermal response of the insulation and the fittings and the spar and they're able to show that Panel 9 with the four to six initial holes starting at the beginning here, could burn through the spar within about 500 seconds, in other areas where it wasn't predicted to burn through and oxidation was not accounted for.

DR. WIDNALL: Do we know what the catalytic properties of that pillow insulation are?

MR. MADDEN: That's Inconel covered, and it's likely catalytic.

DR. WIDNALL: Okay. Was that considered in their analysis?

MR. MADDEN: The plume, yes.

MR. CARAM: Anything that was applied was fully catalytic.

DR. WIDNALL: Fully catalytic.

MR. MADDEN: I'm glad to make it to the next chart where you've got a couple of bullets on chemistry. As you pointed out, we didn't design for aluminum to be in this atmosphere. So areas where we have addressed it is for reentry of space debris. We have done some co-development and studies of that, and we have included chemical convective heating in those simulations. It's an engineering method where it's basically ratioed to the heat rate and the heated formation of aluminum oxide.

I ran that code when I understood you were curious about this. This simulation is a ballistic trajectory. This isn't the Shuttle flight. It's just an aluminum sphere on a ballistic or reentry flight; and it's showing that the heating due to the oxidation of the aluminum, assuming it's bare, was 10 percent of the total heating. And it's pretty constant the whole way up. I also included aluminum nitride formation. That's exothermic as well, and that was another 7 percent. The assumptions that went into this analysis assumed that all the available oxygen and nitrogen contributed, all of the heat from the exothermic reaction itself is liberated to the surface and not carried on into the flow. So a worst case, if you will. So, in engineering terms, it's a fairly small percentage of the total convective heat, at least for this case, a sphere.

DR. WIDNALL: I obviously want to look at that more closely.

MR. MADDEN: And I do and I will point that out. So if you're looking for a reason why, you know, that's one of the reasons why.

The Koropon could also hinder it while the debris still had Koropon on it. That likely goes away at 400 or so degrees, though. Then I here try to point out that, well, the aluminum oxide could self-arrest basically and perform a protective coating on the surface of the aluminum and knock that chemical heating back down. I don't know how much of that happens. I'm certainly not an expert in that area.

DR. WIDNALL: Well, it was kind of interesting because yesterday we got a very different picture from the reentry of, what was it, a steel tank from the Delta 2, I guess.

MR. MADDEN: And that was Dr. Ailor pointing that out. And I think it was a titanium tank.

DR. WIDNALL: Right. Some tank made out of –

MR. MADDEN: Right. Titanium, I think the reactions there are an order of magnitude higher in terms of heat.

DR. WIDNALL: No, but I think what was pointed out was that an aluminum layer deposited on a titanium tank would act as a fuel and destroy part of the tank that, otherwise, would not have been destroyed.

MR. MADDEN: That's certainly interesting. The titanium use on Orbiter is very limited. I think it's limited to pressure lines, hydraulic lines and things like that. So I'm not so worried about any titanium reactions with hot aluminum. I do want to check into this more, along with some other pieces of physics, and see if we really understand how holes grow in aluminum. Right now it's been real simple engineering.

Next slide. We also understood you were curious about the catalytic heating. As Joe summarized, atomic recombination effects are going to be probably more significant than chemical heating. A lot of times, it's a 30 to 40 percent bump factor. An aluminum surface will act as a catalyst and encourage this recombination and liberate additional heat to the surface. A lot of times this could be 30 to 40 percent, if you're using finite-rate chemistry calculations. In our cases, for the plumes we're using equilibrium heating; and that's very close to fully catalytic, anyway. So I think in terms of catalysis and the plume heating analyses and analysis we're doing on the plate burning, we've already accounted for catalytic effects.

Okay. And then these points, I just wanted to point them out. The extent, to my knowledge, is pretty limited here; but things like auto-ignition, the studies that you see in the literature, I think, a lot of times it's at very high pressure and what's called oxygen-rich environments. Here I'd have to say we're oxygen poor; and you certainly, as you descend in an atmosphere during post-breakup, you're going to see these effects probably a little more enhanced than you would in the early part of the flight which we're in.

Like I say, I do want to address the oxidation, just to make sure we understand what's going on there. The melting, of course, any ignition effects, and any sort of vaporization or sublimation of the aluminum. So we're going to get a team, a group together to address that.

Okay, the next slide. I just want to summarize. The work we have going on now is what we call our engineering methods phase. Again, we're kind of concentrating in this area of time here. If you notice, we're talking about bit flips. Okay. So we're trying to explain. The ability to explain these bits flips before the configuration really goes chaotic after 600 seconds, 700 seconds, you know, it's a tough job.

So what we had to do was make these big assumptions like we've gone through. The bit flip at 488 is a breach in the wing. The wheel well rise at 600 seconds is a breach of the wheel well, and the off-scale low is a burning of the first cable. That's very likely. But these two, they are admittedly, they're engineering leaps we feel we have to make to create knowns so that we have the same number of equations and unknowns through our solution space, so we can get solutions and argue about them and refute them and discuss them and see if they make sense.

ADM. GEHMAN: But the second assumption there, the wheel well temperature rise around 600 seconds is a breach of the wheel well, that doesn't necessarily mean it's breached through the door, though?

MR. MADDEN: Correct. And for these solutions, we're going to try to breach the wall, the internal wall. Okay. And what that means in terms of brass tacks? Burn a cable in 42 seconds. We're going to figure out how to do that and the wheel well wall in 112 seconds. Again, we'll cross-check the aerodynamics and the forensic data.

These charts are a sample. There's a whole series of analysis we're doing on varying the distance away of the hole size. There are a lot of parameters. So I just wanted to show a sample of a plume being applied to a flat plate; and we get a temperature response on the next slide, 46.

For various hole sizes, you'll see the temperature transients versus time; and the ones that exceed the aluminum melt temperature in around 0 seconds are going to go into the next series of plots on the next page. You see that show up right there. That would be hole size you need. In this case the spar and the distance away it would need to be to burn that wall in 112 seconds. From this distance away, we can go and look at each panel. Okay. This is Panel 5 region. The hole size needs to be 3 inches. Okay. Then now we are going to cross-check that to the wire-burning analysis and also the aerodynamics.

Next slide, I think, is the wire burning. Here it's kind of explaining how the cables of what we call the bundle, which is the whole series of wires that you see in the pictures, those consist of smaller harnesses and then cables. So we're developing this math model and correlating it to some burn tests that were performed to make sure that we at least macroscopically and engineering-wise can predict when these cables fail.

The next slide shows some initial results from that type of analyses. You see the time to failure and the distance away from the plume. These types of data will be compiled into very similar plots that you saw for the flat plate, and they'll be cross-checked to see. Because we have to burn a wire in 42 seconds that's right next to a wall that we burn in 112 seconds, assuming we just have one plume. So we'll make sure that those make sense with respect to one another.

GEN. BARRY: Chris, the wiring you're burning is Kapton wire, right?

MR. MADDEN: Correct. Kapton coated.

DR. WIDNALL: Another question. You are going to run some experiments on Kapton. Are you planning to run any experiments, say, with an arc jet with dissociated oxygen and the right kind of –

MR. MADDEN: Yeah, we're starting to think about arc jet tests.

DR. WIDNALL: – of aluminum plates or honeycomb or structures and compare that with your analysis?

MR. MADDEN: Yeah. I guess two things. We have started thinking about arc jet tests for burning the wires. It consists of a test where you have a hole, you blow the arc jet gases on it and see how fast it burns the wires. Coupled with that test, we could look at – we were initially thinking of having that hole in the plate that the hole goes through water-cooled, but we could do tests where we –

DR. WIDNALL: Basically burn it.

MR. MADDEN: – cool it and see how fast it grows. So we certainly should think about that.

GEN. BARRY: Let me ask you about the assumptions on the wire bundles. We understand that in *Columbia* it was different. In the well wheel area, there were like four large bundles as opposed to the other Orbiters have like seven; and there's a lot of wires in there that were disconnected that didn't go anywhere because they had been disconnected from sensors over the years.

MR. MADDEN: Right.

GEN. BARRY: Did they have the right diameter and the right combination?

MR. MADDEN: Well, we think so in terms of diameter. The cables that are in those bundles, I think there were only seven that were being recorded; and all seven of those eventually failed. Where they were within the bundle is unknown. So that's another thing we have to deal with. What we're going to do is assume that some of them are very embedded into the bundle and assume those are the ones that go later and slower; and, in fact, the tests that the guys at JSC are performing on the burning include the effect of being inside the bundles.

GEN. BARRY: When you do the testing, is it going to include not just going to the center of the bundle but going through the different sides of the circumference, I would assume?

MR. MADDEN: Well, it's got to hit a side.

GEN. BARRY: But it could be at an angle and not go right to the center, is what I'm saying.

MR. MADDEN: Yes, of course. What we have to assume here is that the plume is hitting, is smart enough to hit cable. And that's likely not the case but it's certainly bounding. This is going to give us the farthest distance away that the hole in the skin needs to be to burn that cable in X amount of time. If it's off axis, it would have to be closer in.

GEN. BARRY: Or hotter.

MR. MADDEN: So we'll be able to determine a region that could exist –

ADM. GEHMAN: Or bigger or hotter.

MR. MADDEN: Yes, sir.

MR. TETRAULT: Let me go to the RCC panels. As I understand it, you've run two thermal analyses, one on a 4-inch hole and one on a 6-inch hole. Why aren't we looking at things like T panels and an entire RCC section and that sort of stuff?

MR. MADDEN: Let's see. The cases we're running for thermal analysis were holes in the panel. Why aren't we looking at missing panels?

MR. TETRAULT: Yeah, or T sections. Does anybody know what the equivalent size of a missing T section would wind up being, if you took that line that then becomes available for air to pass through?

MR. MADDEN: With the missing T seal? Of course, that's a function that's to protect that gap between the panels.

MR. TETRAULT: Right. So what would the gap be in an equivalent hole size?

MR. MADDEN: You still haven't breached the wing in those cases. And there's a whole other set of analyses that kind of the earlier part of my slides that were trying to explain how do you get from the entry interface to the letter A.

MR. TETRAULT: It depends on how all the RCC panels line up. In fact, if the T seals are missing, it may give you a gap.

MR. MADDEN: Well, it will give you a gap.

MR. TETRAULT: A gap in the leading edge.

MR. MADDEN: But not the spar.

MR. TETRAULT: Not at first.

MR. MADDEN: Correct.

MR. TETRAULT: I'm trying to compare a missing T seal to the analysis that you've run based on a 4-inch hole or a 6-inch hole. I mean, what kind of –

ADM. GEHMAN: Order of magnitude.

MR. TETRAULT: Is it less than a 4-inch hole?

MR. MADDEN: I would say it's less than.

And, Joe, would the heating effects be reduced because it's not concentrated?

MR. CARAM: It would be distributed around the leading edge panel. The T cell, as I recall, is about a quarter-inch thickness. So you have to fit it in between two panels. So you're talking three tenths, four tenths of an inch in thickness for a gap. So then you have to account for the area around the circumference of the leading edge. But the characteristic dimension would be your smallest dimension. That would be the size – that would dictate the size of the jet that you're getting in between there, would be the slot width and not the circumference area.

MR. TETRAULT: Just one other comment. You talked about your calculation on the wheel well seal and it's probably not as significant at this particular point as it might have been. But if you look at it from the fact that the heat and the pressure did enter the wheel well and then escaped out the corners, as the debris seems to indicate, then the seal well had to have failed at some point in that. So it may, in fact, be an important number at some later point, so put that in your time line.

MR. MADDEN: Maybe so. But the debris that you see, the evidence you see in the debris is an outward flow. And that would obviously come from higher pressure on the inside and erosion from the inside.

MR. TETRAULT: Right. That's exactly what I've said.

MR. CARAM: Which meant you already have the penetration into the wheel well and the damage is done at that point.

MR. MADDEN: And we're talking about areas out here now in terms of time and we're really trying to figure out what the condition was right here.

DR. HALLOCK: Have you been looking at the fact that when you have the roll reversal occur, it looks to me like that we are seeing the plume actually moving grossly and just because of the fact that it has gone back now and is on the left wing, which may sort of start some of these calculations all over again in different locations? Have you seen that effect, or are you looking at that issue?

MR. CARAM: Again, when the Orbiter is flying, it's typically the heating distribution around the vehicle is dictated not by roll angle but by angle of attack and angle of slicing.

DR. HALLOCK: Now assume that you're actually in the wing itself at this point, so you do have this plume moving around, trying to figuring out where to go. If you look at before and after when you do get the roll reversal, it's a different regime where some of the problems are happening. In one case you're seeing the shorts of the wire and in the other regime you're starting to see all the temperatures starting to change. It's as though the plume was at one point and then when it completed its roll, it's suddenly pointing somewhere else and finding a new path.

MR. CARAM: Well, the wire bundles that he's talking about run right alongside the wheel well wall, on the outboard side of the forward bulkhead. So the plume doesn't have to move around much to get to both.

MR. TETRAULT: Let me be sure you understood the comment I made last time. On the corners, if there's a vent that's there, the wheel well door had to be there, otherwise the vent wouldn't have occurred. So breaking the seal and the time line for breaking the seal may play into your overall scenario to tell you how long the door was there. So I just wanted to be sure that you understood the comment that I was making.

MR. CARAM: Valid point.

GEN. BARRY: Let me put you on the spot a little bit. Now that we've gone through the analysis that you've gone through on a basic attempt to put all this together synergistically, what can we eliminate as an entry point for the heat? If we follow the heat, what can we eliminate right now as an entry point?

MR. MADDEN: We diamonded the door. Okay. From the list of scenarios that the team at JSC has come up with, there are several of them that we called diamond; and we basically tabled them and concentrated on three or four scenarios that we felt were more likely. One of the ones we diamonded off was any sort of breach through the door. The main reason for that was the wires. If you see in a time line, the first wire was burnt before you see hardly any temperature rise in the wheel well. So for a jet to find its way through the wheel well, out a vent, and find a wire and raise that to 900 degrees before seeing any indication in the well itself, we felt, was quite unlikely; and so we are tabling those sorts of analysis at this time.

GEN. BARRY: But you haven't tabled either in front of the main landing gear under the wing, either in front or behind it, but you have eliminated on the main landing gear door.

MR. MADDEN: I wouldn't use the word "eliminate." Probably we might get ourselves into trouble reporting this, but I'd let the Shuttle program maybe answer those types of questions.

GEN. BARRY: Okay. We're getting closer.

ADM. GEHMAN: Okay. Anybody else?

Well, thank you very much, gentlemen. I appreciate your patience with us today and your energy and the zeal and the professionalism by which you are approaching this. We admire it very much.

I've made several notes here. Several of the board members have mentioned what about this and what about that and what about this and the other thing. It occurred to me that we are now at the point where some of these future tests should be mutually agreed upon because if we have some favorite scenarios that we want explored, we should let you know about that so you can take them into account when you're designing tests and things like that. So I think that's very important.

The second area that I noted is the area of the initial assumption concerning a breach. It's not clear to me – and I don't want to settle it right now, just in the interests of time – but it's not clear to me how you have a scenario, the real scenario, the data from the *Columbia*, which suggests to me a changing geometry, and yet what we're trying to do is take a single event and backtrack it. In other words, you take a 4-inch hole. Well, it might have been a 4-inch hole at one point, but it might have been a half-inch hole at the time and an 8-inch hole later on. So I'll have to reconcile in my head how you propagate a casualty over time versus one of those graphs. I don't want to get into it right now, but I think it's very interesting.

I would like for you to also pass on to your colleagues – I know that you represent the tip of an iceberg of a lot of people who are working very, very hard and diligently to try and solve the riddle of this tragedy. We realize that, and I would like to have you pass on to all of your colleagues our admiration and our thanks for all the work that they are doing. They don't get to go to press conferences and things like that like we do and they don't get a lot of notoriety, but I know how hard they're working and I know how hard they want to solve this, too.

Thank you very much. You are excused. And we will call John Bertin, if he's here, and we'll go right to work.

JOHN BERTIN testified as follows:

ADM. GEHMAN: Dr. Bertin, welcome. Would you please introduce yourself and tell us where you hang your hat and what you do for a living.

DR. BERTIN: On Continental Airlines, coming back and forth to Houston.

When I graduated from Rice with a Master's, I went to work for the manned spacecraft center across the street and then got my Ph.D. part-time at Rice and went to UT Austin and taught for 0-something years. I did some research on the Shuttle before it flew. Did some things with reentry heating, tile misalignment, shock-shock interactions. And then after it had flown, we did some analysis on asymmetric transition and anomalous findings from some of the flights, with some of the people who have been giving the presentations up here today. After my kids grew up and they were all out of the house, I left Austin and went to Sandia for a few years; and I teach now at the Air Force Academy.

ADM. GEHMAN: Thank you, sir.

DR. BERTIN: Can you get 18 up here for the viewgraphs?

I thought since we talked about temperatures and we talked about catalysity and we talked about some in degrees Kelvin and some in degrees Fahrenheit and some in degrees Rankine, I thought what we might do is talk about the flow field in general, with one set of nomenclature and what have you.

So if you look at the Orbiter coming in in this orientation, it's at approximately 40 degrees angle of attack. So the velocity vector is coming in like this and the flight path angle, and it's not rolled or yawed or anything like that. It's at an angle of attack of about 40 degrees. So you see it in this picture here.

Okay. This is a wind tunnel test and they talked about Mach 6 in the wind tunnel and it didn't do this and it didn't do that. So let's look at and talk about Mach number and hypersonics and some general features. So if you're going to be flying in a vehicle in the atmosphere, the Mach number is going to be velocity over the speed of sound, whether you're in the wind tunnel or in the atmosphere. So in the atmosphere, no matter what altitude you're at, the speed of sound is about a thousand feet per second. So the Mach number is about the velocity at which you're flying in thousands of feet per second divided by a thousand. So if you're at Mach 6 in flight, you're flying 6,000 feet per second.

Now, there's a lot of kinetic energy in that flow and as the flow approach – so if you're doing a wind tunnel test, to have that much energy, you damage the wind tunnel. So what they do is they run the speed of sound down to where it doesn't simulate the same gas chemistry. So the gas chemistry in a wind tunnel is very, very different than the gas chemistry in flight, even though both flows are hypersonic. Okay.

So now the vehicle is flying along at, say, 6,000 feet per second. It's at an angle of attack of 40 degrees. Why isn't it flying at a low angle of attack like airplanes, which fly about like that, right? Because the heating goes as density to the one-half velocity cubed divided by the bluntness. Since the velocity cubed is large, the heating is large. So what you want to do is counter that by giving as blunt a vehicle as you can. Okay.

So as the vehicle is flying through the air, the air is coming rushing along at 6,000 feet per second and it has to turn to go parallel to the flow's surface. To do that, it goes through a shock wave. I'm sure you've heard the witnesses talk about hearing the sonic boom. The sonic boom is caused by that shock wave. See this thing going up here? That's the bow shock wave. Now, it decelerates and turns the flow. So as the flow decelerates from a high kinetic energy flow to one of low kinetic energy, the temperature is going to go way, way up.

So the temperature is going to be the atmospheric temperature up here a few hundred degrees and it's going to be much, much higher back here, depending upon what part of the vehicle you're in and where you are. But in this region near the nose, you're going to see the highest temperature and we'll use equilibrium and we will use degrees Rankine. You're going to see temperatures of 10 to 12 thousand degrees Rankine.

Now, obviously there's going to be some chemistry going on with these kinds of temperatures and you're going to see a strong shock wave. So the density is going to be changing very dramatically. The pressure's going way up, the temperature is going way up. Density is changing very dramatically. And you know how when you look in water and a fish is here but it looks like it's over here because the light rays are bent? Well, that's what's happening here. The light rays are bent. They pass light rays through the tunnel, and the density changes allow you to see the light being bent. So you can see the density changes downstream of the shock wave; and they're caused by, like I say, the pressure changes and the temperature changes and what have you. So up here the temperatures are on the order of, say, 10,000 degrees, maybe 10,000 maybe 12,000 and again there's some chemistry, there's some non-equilibrium, there's some things going on.

Now, the flow expands around from the nose. Just like when you put your hand out the car window and stuff like that, you feel the force on your hand. Well, the flow accelerates as it goes around your arm, right, and you feel the velocity. You drive down the street and in the windshield you see the stream line patterns taking place in running rain across your windshield. So there are stream line patterns coming here and the flow accelerates to where the temperature of the air in this region is more like 6 to 8 thousand degrees Rankine because the pressure has dropped. The flow is accelerated. The pressure has dropped and the temperature has dropped, so 10, 12 thousand up here. Very high heating rates because it's got a small nose radius and temperature dropping 6 to 8 thousand outside of the boundary layer.

Okay. What is a boundary layer? If you've ever gone to the beach on a cold wintery day, if you're late and it's sunny, if you lay down, you feel relatively warm. If you stand up, you feel much colder. Right? Well, what you're feeling is the change in velocity as it goes from the surface to a much higher velocity a few inches away from the surface. So that's a boundary layer. So you're going to get shear going on in that. Just like when you rub your hands together, you're going to get heating.

So the boundary layer causes the air, by rubbing against each other and fluid particles, to give you more heating than you'd expect from just a few thousand degrees. So you want to protect the vehicle from this heating. Okay. So you put high-temperature materials along the parts of the body that have a small radius, like the nose and the wing leading edge, and you can use less robust materials over areas where the heating drops.

I said the heating varies as rho to the one-half D cubed divided by the bluntness. Well, only a fraction of that energy actually gets transmitted into the vehicle. Most of it goes flowing past the vehicle in the air stream et cetera.

Now, we talked about the tiles. The tiles fill most of the area here, and they're black. They have a thin coating, and the thin coating does several things. We talked about the catalysity and non-catalysity. So the thin coating is non-catalytic, but it's also like Scotchgard. If you look at the thing, if the vehicle's sitting on the pad and it rains, if the tiles didn't have the coating, they'd soak up a lot of the water. So the coating prevents some of the water from getting in.

If you go back to your freshman physics course and you did the little heat transfer thing, the energy coming in can be radiated back out, right? And if the energy is radiated back out, what's the best color for radiating outward? Black. So the coating is a thin, black coating that gives you several type features; and it goes to a much lower temperature.

ADM. GEHMAN: Let me ask a question, Back to wind tunnel, if this is a good time to talk about wind tunnels. As I understood you, the way they achieve the very, very high speeds, the very, very high Mach numbers without tearing the wind tunnel apart is by changing the gas in the wind tunnel to where the speed of sound is a lower speed of sound.

DR. BERTIN: They run the wind tunnels, where the speed of sound is an order of magnitude, or close to it, lower than would be normally in the normal atmosphere.

ADM. GEHMAN: So they're using some other gas.

DR. BERTIN: Or they're taking the air and causing the pressure and temperature to drop way low. The temperature is just above liquefaction of oxygen. If you ran the tunnel any differently, you'd get liquid oxygen going down your tunnel.

ADM. GEHMAN: So any other properties, then, of the results that we should be suspicious of?

DR. BERTIN: That's going to give you some changes in the density ratio. And the density ratio, I think Dr. Widnall talked about how the shock wave is going to change its inclination as you go up in Mach number. It's going to change its inclination as you go up in density ratio. And the density ratio in the flight case is near 20. Maybe 12, maybe 15, maybe 20. The density ratio in the wind tunnel is going to be 6. So it's going to have a much different shock structure. We're going to talk about that in terms of the shock-shock interaction in the Kirtland photos.

ADM. GEHMAN: That was going to be my next question. I'll wait.

DR. BERTIN: Okay. So we have these things going on. So the wind tunnel is just a simulation of parts of the flow, and what you want to look for is some general overall things that you can then compute and then correlate them in some fashion.

Okay. So we have basically now these tiles over much of the surface, and they're giving us many features. We've got carbon-carbon along the wing leading edges, and they go to higher temperatures. So the boundary layer is relatively thin, maybe a few inches by the time you get to the end of it, and so the flow going over that adjusts into – from the zero velocity, the Mach 2 or 3 locally, so the Mach number in this region is supersonic. So if you had a disturbance way down here, it would not feed forward.

You asked the question about would any of this explain what happened to the water being dumped. I don't think so. That may be a problem, but it would be a different function because the disturbances won't propagate upstream unless you have some strong shocks that make the flow subsonic.

ADM. GEHMAN: Well, the reason I asked the question was because one of the gentlemen said that in experiments with the body flap that they had – the first time they entered, they had the wrong pitch set in the body flap and when they started moving the body flap, there were some changes in the shock pattern, the properties of flow.

DR. BERTIN: There will be some changes in the shock pattern, but they'll be limited to the region within a few distances of the body flap. So they can propagate upstream because you're having shock waves, but they won't propagate unless you've got a spectacular flow. They won't propagate very far upstream. So you have that.

Then if you look at the model from this standpoint and you rotate it about its velocity vector so it's still a 40-degree angle of attack, if you look at this picture, it's going to have a shock wave over the bow, the fuselage, the nose region, right, and the shock wave is going to wrap at fairly close angle, like this. If you imagine that you just rotate the model from like this to like this, you'll have shock waves that occur that kind of envelop, form an envelope over the fuselage.

What's going to happen when those shock waves reach the wing leading edge? Because the bow shock wave will be at about this point on the body, right? So what's going to happen? There's going to be a shock wave set up for the wing leading edge. And when the shock wave from the bow shock wave intersects the shock wave from the wing leading edge, you're going to get an interaction that could cause the heating to go up, depending upon what the sweep of the wing is relative to the oncoming flow. So it works out where this kind of delta-wingish type thing has relatively low severity in the shock-shock interaction. If the wing were onswept, you would have great severity in the shock-shock interaction because you're taking a flow going this way and causing it to intersect a flow that has a much stronger shock that's going this way.

So if you are missing maybe not one panel but maybe two panels and maybe it's downstream from the initial column that you had and stuff like that, then you've got like two teeth missing from the leading edge and you've got a little notch in there. Now the flow can go in that notch and create a shock pattern that, in my mind, kind of looks like what the Kirtland photograph might be telling you, in that something is not missing, something is added. And it could be the density gradients of the shock waves in a shock that's been changed shape because you've had some damage that has grown in time. So that would explain some of the additional features.

Then the other thing is, if you look at airplanes flying in high-humidity air, the pressure is higher on this side, right in general, and lower on this side because you're generating lift. So when you get to the wing tip, you form a vortex, wing tip vortices if you're a pilot for the trailing weight and counter hazard. If you're an engineer, you've got these beautiful pictures and wind tunnels and stuff like that. If you're in CFD, you've got beautiful pictures and colorized computer outputs. But for a variety of reasons, you have a vortex. And the vortex is basically a horizontal tornado and the velocity can be very, very high speed and circulating, just kind of like the flow going down your sink or a tornado that's being spawned by a front coming through and stuff like that. So you look at that.

Now, if that tornado came from someplace in here through your gap in the shock wave, not only do you change the shock wave out here but you get the possibility of some kind of vortex coming and striking part of the vertical fuselage. And it could be only limited.

I remember back when I looked at the data from the Gemini project, the GT2 was an unmanned test vehicle and the Gemini had umbilicals that brought the electronic wiring from the booster into the command module or the spacecraft and the umbilical – the Gemini came in at a slight angle of attack and a vortex pattern that had been set up by the flow over the umbilical caused minute holes to occur in the surface of the Rene 41 of the Gemini and they had little holes.

So the vortex can be very localized and it can be very hot, depending upon where it touches down and how much it touches down and what the shape of the vehicle is. But you can see a progressive situation where if you lose a panel or two, you'll get a vortex that could scrub the vertical surface and you get a shock that forms with the shock-shock interaction that creates the image of something that is different than just the main planform of the vehicle. I say you could, 'cause I need more looking at that.

Okay. Is that kind of good as far as – overall as far as where temperatures are high and how they change and what they do?

ADM. GEHMAN: You covered this but I want to be sure I understand that when we're talking about these boundary layers, in accordance with this picture back here, for example, we're looking at boundary layers which are kind of spreading apart and are measured in tens of inches or something like that toward the tail but at the nose we're talking about –

DR. BERTIN: It's going to grow. And this is a 100-foot long vehicle. So it will grow over the length of the vehicle so that it's, say, fractions of an inch, so negligible at the nose, grows to a few inches and greater toward the trailing surface. That's why when you have surface roughness like misaligned tiles, a misaligned tile toward the end of the vehicle is not nearly going to have as dramatic effect as a misaligned tile or a chip in the front of the vehicle because the boundary layer is so much thicker that the disturbance doesn't –

ADM. GEHMAN: But on the front edge of a leading surface like the RCC or the nose of the vehicle, these boundary layers are compressed down to fractions of an inch. So the distance between the temperatures that the vehicle sees, 2750, 2900, and these 10,000-degree temperatures which are measured in little bits of –

DR. BERTIN: The differences between the temperature at the edge of the boundary layer, being 6 or 8 thousand degrees and the temperature wall being 2 or 3 thousand degrees are going to take place over fractions of an inch, which is why the heat transfer rates become so large because those temperatures are gradients.

Then another thing that's going to happen, like I say, is if you imagine rubbing your hands together, you're going to get some friction and the temperature within the boundary layer may even be greater than the temperature at the edge or at the wall because you have this frictional dissipation going on. Because you're going so fast. Your air particles are moving so fast that the rubbing together creates the heat transfer that's unique to hypersonic flight.

DR. WIDNALL: John, you're talking about basically the temperature distribution around the vehicle for a gas that is fully dissociated.

DR. BERTIN: In equilibrium.

DR. WIDNALL: In equilibrium. Dissociated.

DR. BERTIN: The numbers I gave you were equilibrium. If you had non-equilibrium or you're fully dissociated, your temperatures would be a little higher. If you had recombination, your temperatures would be a little bit different again. So your temperatures – mine were based on kind of an equilibrium model.

Now, if you do some computations and you keep it to simple global areas, because the heating is such a small fraction of the total energy available, within about a 20-some percent model, whether it's fully catalytic or non-catalytic over the length of the thing will not change too dramatically. Now, if you compromise a leading edge and you expose some metals and stuff like that, then, yes, your catalysity probably is a major factor like you've been suggesting.

DR. WIDNALL: You know, this is a subject I absolutely hated in graduate school, I have to tell you; but my reading and study of this indicates that the effective surface catalysity has a larger effect on temperature than it does on heat transfer rate.

DR. BERTIN: Well, now, you remember the temperatures were backed out of the heat transfer rate. I mean, the heat transfer rate's going to be backed out of the temperature.

DR. WIDNALL: Right. But the temperature that's being affected is sort of stagnation temperature and recombination.

DR. BERTIN: Okay. You're referring, I believe, to some tests that were initiated at Ames Research Center.

DR. WIDNALL: Well, not only that. Just thinking about a stagnation point.

DR. BERTIN: Now, at a stagnation point you're going to have the velocity of the gas is going to be different than it is going to be moving around the vehicle. So your residence time is going to be a little different and so your effects are – so you would have to take that into account. You'd have to take the shape of the vehicle into account and you'd have to take whether you were looking at the stagnation point, whether the stagnation point was catalytic and the local surface was not and things like that.

ADM. GEHMAN: Speaking just aerodynamically, forgetting all about heat – even though I've already learned you can't do that. Shuttles have returned safely from voyages in which as many as a dozen tiles were missing and weren't even there. Based on the presentations that you've heard and based on your knowledge of this leading-edge shock wave kind of thing, on the Shuttle what kind of a deformation – I'm not asking you to predict what was missing – but what kind of deformation in order of magnitude should we be looking for? Are we talking about inches or feet in order to significantly change the shock wave and, therefore, the shock wave also determines the exterior heating wake?

DR. BERTIN: If I were trying to relate the aerodynamics – and most of the stuff I've done has been aero-thermo and it's been with the heating and transition environment and not with the small increments to the aerodynamic coefficient – but if you had one of the T fillers missing and stuff like that, I think the mechanism for heating would be different than if you had two or three of the RCC panels missing. Because I think with just a filler bar missing, I think you'd start the process and you'd have some situation where you would have to do some analysis of flow in a narrow gap. Because if you just did from a two-dimensional analysis of like the flow in a cavity like he was showing some of those things with the flow coming, the flow would pretty much skip over a T cavity.

So you'd have some flow getting in and it would have to start a process that led to more damage, in my mind, to get significant changes. I think people who have looked at the data that they're obtaining at Langley have said that having the one little RCC missing, the No. 6 one, did not give them the aerodynamic changes that they saw later on. And I would believe that. I would believe the T would give only slight changes, that what it grew into when it lost maybe – like I say, if you were going to suppose or opine – when it grew into something that had multiple RCC pieces missing so that you had kind of the bow shock changing significantly, that would change your aerodynamic forces significantly and that would be consistent with some of the later things going on.

DR. HALLOCK: Can I ask you a question? We've heard the term "shock-shock interaction" used many times. I think it would be useful if we could define what that means; but also, as part of that, go back to the fact that, as you mentioned, you can even see this in a photograph at Kirtland. The question is: Why can you see this in a photograph?

DR. BERTIN: If I have a vehicle like this, it's going to have a shock wave that looks –

ADM. GEHMAN: You're welcome to sit down, even though I know all professors do better waving their arms around.

DR. BERTIN: I'm Italian.

If you look here, this is the shock wave standing off from the surface; and it causes the flow to change direction and the pressure to increase. And with the pressure increase, the temperature increases. So it would be about some small distance off the surface. If you rotate it and look at the picture in this plane – you're not rotating the model, you're just looking at the picture in a different plane – you'd see also the shock wave having about the same standoff distance, right? So it would come in and intersect this surface. Right? But what's going to happen to the surface out here? Because that shock wave is only changing things inside within its dimensions. So it's only changing things between the shock wave and here. So when it hit the wing, it wouldn't affect this at all out here. Right?

So another shock wave has to form to cover this part of the body, and it would depend on what the angles were and the radii and how fast you were going. So you'd have a situation where you had a shock wave up here and a shock wave here. Now, when they intersect, you have changes in pressure that are different in here than they are out here.

So there has to be something happening in the fluid mechanics to change so that the pressures become continuous and you don't have just sudden gaps in your flow and stuff like that. So the interaction you get depends on whether the wing is like this or like this or like this. So the bow shock is going to be – and then all that is changed by the fact that if something happens so that you get a stronger shock, you'll move the flow.

So if I put a gap in here, a significant gap in here, when the flow comes down here, it strikes not the wing first but it strikes the teeth that are missing and kind of flows into that cavity and splashes up against the rear wall of this. So it creates a shock wave going out. And that shock waves gives you density gradients just like these and they'll cause light waves to be bent and give you a different pattern in the flow picture. So you could see – and they unfortunately didn't have them. But if you roll the model to where you were in basically at a 90-degree bank, you would see the shock-shock interaction structure.

On the X-15 back when they did the last flights of the X-15, they hung a hypersonic research engine underneath it and they hadn't taken into account the fact that there was a bow shock wave coming off the main fuselage of the X-15 and there was a second shock wave, completely different, coming off the hypersonic research engine which was kind of underslung off the ventral fin. And when the shocks came together, it caused a strong change in heating. The perturbations in heating can be factors of 10, 30, and more when you get the shock-shock interactions, depending on what the sweep angle is.

For the Shuttle without damage, the sweep angle is such that the interaction effects are relatively benign. So that while there's a shock-shock interaction, the highly swept leading edge prevents you from having strong interactions. If had an unswept leading edge, you would have strong interactions and very large heating going on. So that would be something to look at.

MR. TETRAULT: Doctor, I'm told that the shock-shock interaction occurs normally at RCC Panel No. 9 on the Shuttle. Is there anything that would cause that to move, say, to a different location, say, closer to the fuselage or further out on the wing?

DR. BERTIN: I'm assuming that RCCs possibly were lost in time so that, in a very early one, maybe one would be missing, maybe more, but then because the understructure is exposed, that some additional damage occurred and other ones would have come off in some fashion. Just from my standpoint, with just one missing, you could get the damage that maybe was observed eventually; but for seeing the Kirtland one, I think you'd have a pretty good piece missing.

MR. TETRAULT: I wasn't talking specifically about any damage. I'm just talking about in normal flight, I'm told that the RCC Panel No. 9 is the location of the intersection of the shock wave.

DR. BERTIN: Oh, yes, 9.

MR. TETRAULT: My question is: Is there anything that could happen in flight that would change where that shock wave location would be? I mean, if you are experiencing yaw, for instance, would it tend to move closer to the body?

DR. BERTIN: If you're going to change the orientation of the vehicle, the angle of attack, the yaw angle, these things – the shock-shock interaction pattern would be a function of geometry. It would be a function of angles.

MR. TETRAULT: But simply going from right wing down to left wing down would not change that intersection. Is that correct?

DR. BERTIN: If all you were doing is changing the bank angle from this to this where you had the 40-degree angle of attack, you shouldn't change. Now, if you change the yaw and roll angle at the same time –

MR. TETRAULT: Then it would move.

DR. BERTIN: – then it would change some things. If you change the angle of attack, it would change. If you significantly changed your Mach number so that the gas chemistry changed, that would change. In other words, the shock-shock interaction pattern at, say, Mach 15 in the wind tunnel might be substantially different than the shock-shock interaction pattern in flight because in flight you would have significant real gas effects, you'd have significant dissociation. In the wind tunnel, you'd probably have a perfect gas and a density ratio of 6.

ADM. GEHMAN: Doctor, you heard the previous presentation in which Steve Labbe mentioned the *Columbia* data showed a relatively early roll and yaw bias to the left or showed control surfaces trying to control that relatively early, earlier than previous – different from other flights. Can you draw any conclusions or insights from that? Particularly what I'm interested in is the statistics, the chart that he showed where it showed this left bias very, very early, before the first temperature rose, before the first debris came off.

DR. BERTIN: The only thing, based on my limited experiences with the aero increments, the only thing that I was looking for when I talked to him about these very items was he talked about – I was thinking that one of the possibilities would be premature boundary layer transition due to damage on one side as opposed to the other. 'Cause I was worried about that being one of the multiple players in a breakup scenario. So I believe he – in fact, several people on the panel, in my conversations with them – I believe the fact that they got the same sine for the increments of the yaw and roll and for – when they got asymmetric transition, they always got opposing sines, that that was one factor that says, okay, it's probably not premature boundary layer transition on this particular flight.

Another thing. If you go back and look at all the things, the sensors that went out, there were several near the trailing edge near the elevons and stuff like that; and it worried me that maybe that's a sign that those were going out early because of premature boundary layer transition. But if you look, almost all the ones that went out early went out because they came from bundles that were near the left main gear area. So you could trace the ones that went out near the trailing edge back to bundles that went near the damage area, and the other ones that stayed on came from other parts of the vehicle.

Then, the third piece of collaborative information. The vehicle broke up at altitudes that I think are just above where we had ever seen the earliest transition, or close to it. I don't think we had ever seen transition that early, even in the anomalous flights. So for those three reasons, in my mind, I kind of said, okay, damage notwithstanding, there was not a premature transition event that led to some additional failures.

MR. WALLACE: In the anomalous flights – and I understand there were cases with *Columbia* where the boundary layer transition took place maybe at numbers as high as Mach 18 versus typically Mach 6 –

DR. BERTIN: I think it's Mach 8 and 150,000 feet. Mach 8, give or take one, and 150,000 feet, give or take about 10,000 feet.

MR. WALLACE: Give or take in those anomalous events, if you know, did the boundary layer transition happen sooner on one side or the other?

DR. BERTIN: There was one that was significantly asymmetric and I think most of them could be traced – it's been a long time since I looked at those data, but I think asymmetry was significant as far as its resulting affecting of force on one flight. In two others, it was just early.

MR. WALLACE: Is it fair to say we have the piece – well, you talked about the shock-shock and we have the shock-shock on either side. So I guess my question is, having stood under the Orbiter down at KSC, it looks like one big wing to me –

DR. BERTIN: Yeah.

MR. WALLACE: But does the – boundary layer transition can happen really distinctly separately on either side?

DR. BERTIN: Boundary layer transition is the growth – occurs because of disturbances grow to where the flow breaks down to where it kind of swirls and twirls. So you could have a piece of damage, a tile bar filler – I believe that was one of the sources of one of the flights where they had – the gap filler sticking up about half an inch or more, and it would trip the area. It would affect the flow downstream of it because, again, we're locally supersonic so disturbances won't propagate upstream. So if you put a gap filler bar up in front, you would have the transition promotion in kind of a wedge downstream of that.

MR. WALLACE: So you could just have kind of a localized area where –

DR. BERTIN: Localized but broad coverage. But it would start at the bar and go down in some kind of wedge.

DR. WIDNALL: John, how would you calculate the temperature at the stagnation point of an aluminum sphere that was reentering the atmosphere at Mach 20?

DR. BERTIN: I assume you mean the entire temperature profile and not just the temperature at one –

DR. WIDNALL: Well, I'm interested in the temperature stagnation point. I assume that's easiest to do relative to everything else you might want to calculate.

DR. BERTIN: Okay. But just like with the vehicle in general, the stagnation point has a temperature at the surface and it has a temperature of the air outside –

DR. WIDNALL: I'm interested in the surface temperature.

DR. BERTIN: At the surface temperature, I would assume you would use, depending upon your altitude, a Navier-Stokes code or the classical fluid mechanics code with chemistry. And I'd be willing to bet people at Ames have a code like this to calculate the chemical reactions which would be dependent upon the density and the velocity of the vehicle at which you were flying. And then you would have to do a kind of thermal surface response. But you would have a non-equilibrium flow with a surface catalysity of the material in there and you could get a pretty good idea of what the temperature would be of the material. And like I say, I think it would be very sensitive, if you had an aluminum sphere, as to what your thermal mass was because the aluminum sphere would not only be catalytic, but it would be a good conductor. So some of that energy would be immediately conducted into the vehicle.

DR. WIDNALL: Well, let's make it a thin-shell aluminum piece.

DR. BERTIN: Okay, a thin shell with an adiabatic back piece?

DR. WIDNALL: Whatever.

DR. BERTIN: Okay. Then you could do some similar things like in your heat transfer model just have the thing go up in response to the environment you put it in. You could do a non-equilibrium computation with a reacting surface. And, in fact, I would think codes exist for simple shapes like the sphere that you're talking about.

ADM. GEHMAN: In the debris associated with this tragedy, there are some 25 spheres which have been recovered. All the fuel thanks, 25 out of 30.

DR. WIDNALL: But I don't think any of them were made out of aluminum, were they?

ADM. GEHMAN: No, but the question I'm asking is when you take everything into account that you know that this Orbiter was subjected to, starting at about Mach 17 and then finally breaking up at Mach 15, something like that, and you take into account the discussion we've had about chemical reactions and catalytic reactions and ionization, what would you suggest that we should be looking for in the debris? I mean looking for in the chemical sense – that is, in the sense of deposits and discoloration and oxidation. What kind of testing and metallurgic kinds of evidence should we be particularly sensitive to that might give us a clue as to how this thing started, particularly if we can juxtapose the left wing and the right one?

DR. BERTIN: Jim Arnold and Don Rigali and Howard Goldstein and I were here about a week ago, and we talked a little bit about forensic-type looking at the deal. And I think several people have talked about how the flow – there's indications along the front part of the wheel well, the left landing gear box, the door covering, that indicate the flow was actually from the inside out, that there were stream line patterns in the surface there and there were dark – and you could see little stream line patterns. So this would indicate that, in my mind, that at least at this time frame there was flow going through, whether it was, as you talked about, a 4-inch hole, an 8-inch hole that had grown to whatever, flow had gone in there, was impinging on the tires and kind of coming back out, not necessarily filling all the cavity but impinging on the tires and coming back out. So I would think you would want to do some things with – doing some analysis of the surface in that area, find out if it was aluminum from some places, if it had tire type things in it. There was, like I say, a recommendation made by Jim Arnold that I thought had some good things in it.

Backing up in time to find out when things first started, I would think you would want to do something like some free molecular flow calculations in some scenarios where you either had a pock of damage or a crack or a split or some kind of realistic T bar filler missing, some kind of realistic thing to see how that would affect the back surfaces and the aluminum facings and stuff like that to start your damage pattern. And then something in the middle where you had a jet of hot air going in through the damaged substructure and creating more havoc. But then you're in kind of a continuum environment at about 70,000 feet, even before you get to peak heating. That's kind of what I put together. And then you have the tests at Langley with the RCC panels missing to see if you could kind of reproduce the Kirtland photograph.

ADM. GEHMAN: My last question is, based on your fairly extensive knowledge of both aero- and thermodynamics, do you feel that after this total of 13 flights in this regime that is winged, manned, recovered flights, that our knowledge of this region is – are we at the beginning, the middle, or are we fairly mature in our knowledge of this region of science?

DR. BERTIN: I think it was Mr. Caram that said that – or one of them said that almost every trajectory flies right down the same path. So it's not like we've had, you know, each one as a new environment. They kind of go along the same path. If you overlaid the velocity altitude time, there would be very nominal type performance. So in that sense we have a lot of experience with what happens nominally if nothing has broken, if nothing has come off or if what has – if a tile is missing, it's a tile in some place that's fairly benign or there's a structure underneath that caused the tile, the heating to be conducted internally and not out. So I'd say from that standpoint we have a lot of information.

From the standpoint of what could happen if something came off and hit something and damaged it in ways that had not been done before, it's a very unique and very harsh environment. I doubt that we'd know even something as simple as the initial flow field that caused, say, the initial – say, a T gap had been missing or a small hole in the RCC. I think that would be a challenge to look at and say, okay, I think this happened in detail.

Like I say, if it's nominal, everything's sealed and things going on, we've got a lot of information. If we're substantially away from nominal, it's a very, very harsh environment and very, very sensitive to the individual details, I would think, of what actually happened.

MR. TETRAULT: Let me ask you one question. As we heard in the last presentation, very late in the event, the rolling motion seemed to change and that change required that there be lift under the left wing. They talked about running analysis based on the wheel well door being open which might have created it. Can you think of anything else that would create lift on –

DR. BERTIN: The shock-shock interaction – if you look at the Orbiter like this, the normal shock-shock interaction, like I say, is going to trace a bow shock that comes along here, intersects the wing in about here, and then another shock that's going to be like this. If we had the two or three pieces missing by that time here and we had a shock that looked like that, which is kind of what the Kirtland photograph – and again, don't overinterpret this – that needs more work. If the Kirtland photograph is saying we had a shock-shock interaction like that, that's going to be a much stronger shock. The modified one, the one with the pieces missing is going to be a much stronger shock with much higher pressures than the original shock would have been at, say, while the vehicle was still intact.

MR. TETRAULT: And that could have gone under the left wing?

DR. BERTIN: That could have caused the pressure to be higher and giving you an asymmetric force.

DR. WIDNALL: I was rather intrigued by the suggestion that came up earlier that perhaps a jet coming out of the wheel well door could create a local shock in the area around the wheel well and lead to increase pressure. Of course, obviously it depends on the volume.

DR. BERTIN: It would do that also because obviously if the flow is coming from inside out, that's not a normal passage. So as it comes oozing out or flowing out or however fast it was coming out, it would thicken the boundary layer; and thickening the boundary layer would change basically the flow over the surface some small amount.

DR. WIDNALL: But it could lead to a shock, a local shock.

DR. BERTIN: And it could lead to a shock interaction that would cause locally higher pressures in the area of the landing gear well.

MR. WALLACE: The prior panel described their various scenarios that they were then going to try to fit into the aero picture, the thermo picture and on the sensory. Any thoughts on those scenarios in terms of other scenarios that you might suggest?

DR. BERTIN: I think they're working with the tools they have in a logical sequence of steps. If I were kind of setting up things, I might try to spend a little additional time kind of coming up with cartoons of what the flow might look like if it were coming out of the wheel well and saying how much does that modify the flow, to try to get me some additional things that I could compare with some of the observations that you made. Like if the thing was generating more lift at some point, could I get a shock-shock interaction to explain that, could I get flow coming out of the wheel well creating a shock.

So in addition to the things they are doing, which are certainly good steps along this line, I think I would try to get a cartoon strip saying like this is what's happening here, this is what's happening here, this is what's happening here and try to get some engineering assessment.

It's a very, very difficult problem to do either experimentally or computationally. So you kind of want to, like I say, have some pictures of what do you think is happening and then run some tests or do some computations to see if that's what you get out of your models. I think somebody pointed out the fact that the model fell apart later on. Well, why did it fall apart? If one third is going like that or one third is going like that. So do you need to upgrade your model? Do you need to improve the rigor of what you're looking at?

MR. TETRAULT: I know you haven't had the opportunity to see some of this debris, but let me describe at least one of the vents that's coming out and maybe give you a sense of how large it may appear to be. Then maybe you can tell me whether you would think this would be fairly significant in terms of disruption to the boundary layer. It appears that the vent goes out and actually covers three adjacent tiles, which would mean that it would be probably in the range of 18 inches. It actually melts the tops and surfaces of those tiles. So it would have to be extremely hot. And it is perpendicular to the normal flow that you would expect the boundary layer to be going over the aircraft at. And you see it –

DR. BERTIN: You're talking about the main landing gear cover?

MR. TETRAULT: Right. This is the forward inboard corner.

DR. BERTIN: Yeah. I looked at some pictures. It actually even erodes away the metal.

MR. TETRAULT: It erodes away the metal structure on the inside. That's the aluminum structure, which you expect because it's obviously very hot. It's hot enough to actually erode ablative tiles.

DR. BERTIN: No, but you can see on what's left of the tile patterns, you can see black surface which is stream lining out. Yes, I would think that would cause a significant increase in the boundary layer thickness, some strength of a shock – 'cause, I mean, if gas is coming out, it's changing the surface of the pattern.

In other words, if gas is coming out, it would be like if I took a little jet in here and blew air into the surface. That's going to cause the flow to turn, have to turn around the jet. So you would have a shock wave. You would have a shock boundary layer interaction. How strong that was would depend on how much flow you were coming out with, but that would certainly be a parameter that would be in addition to the notch on the wing leading edge. And it goes in the same direction.

ADM. GEHMAN: Thank you very much, sir. I'm going to ask that, by virtue of being the chairman here, I'm going to ask one last question. Then we'll close up shop here.

Based on your knowledge, would you make a recommendation to us as to how much latitude there is in the reentry profile, you know, to reduce heating or to reduce stress, even if you wanted to increase heating but reduce stress or something like that? How much latitude is there in the reentry profile?

DR. BERTIN: That's not one of my areas of expertise. But in talking to others, your entry angle is somewhat limited because if you – and you have the weight of the vehicle. So unless you can throw things overboard to significantly change the weight of the vehicle, your entry angle has a certain range that you can come into. And you're going out there. I mean, you're orbiting at 20,000 feet per second. You've got a lot of energy. You've got to dump a lot of that energy, and there's only so much drag you can do, it's my understanding, with the flight path. So limited.

ADM. GEHMAN: That's what we've been told by several people, and I just wanted to get your opinion.

Dr. Bertin, thank you very, very much for helping us solve this mistery. Your knowledge and your professionalism and your ability to explain complex things to us is very, very greatly appreciated. We appreciate you taking time to help us with this; and if we have any further questions, we probably will get back to you. Thank you very much.

The press conference will start promptly at 1:00 o'clock, for any of you that are interested. For those of you that aren't, have a nice day. For those of you who don't have any choice, be here anyway.

(Hearing concluded at 12:32 p.m.)

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Appendix H.4

March 25, 2003
Cape Canaveral, Florida

Columbia Accident Investigation Board Public Hearing
Tuesday, March 25, 2003

1:00 p.m.
Radisson Hotel
8701 Astronaut Boulevard
Cape Canaveral, Florida

Reporting: Keith L. Vincent, CSR
Esquire Deposition Services
Houston, Texas

Board Members Present:
Admiral Hal Gehman
Rear Admiral Stephen Turcotte
Brigadier General Duane Deal
Mr. G. Scott Hubbard
Mr. Steven Wallace
Dr. Douglas Osheroff
Dr. John Logsdon

Witnesses Testifying:
General Roy Bridges
Mr. William Higgins
Lieutenant General Aloysius Casey (Ret.)

ADM. GEHMAN: Good afternoon, members of the board. We'll continue our fourth in a series of public hearings. This afternoon we're going to be looking at processes and procedures down here at Kennedy Space Center; and we're going to lead off with the director of KSC, General Roy Bridges.

General Bridges, welcome. Thank you for being here this afternoon. Before we begin, I would like to ask you, Director Bridges, to affirm that the information you provide the board today will be accurate and complete, to the best of your current knowledge and belief.

GEN. BRIDGES: I so affirm.

ADM. GEHMAN: All right. Thank you very much. Would you please give us a brief statement as to your background and how you got to be the director of KSC and how long you've been here.

ROY BRIDGES testified as follows:

GEN. BRIDGES: Yes, sir. I took the job in March of 1997. I had previously been on active duty in the United States Air Force for a little over 31 years, having retired in 1996, in July.

During part of my 31 years with the Air Force, I served as an astronaut, with a six-year assignment in Houston at the Johnson Space Center. I flew once on the Challenger, Mission STS 51F.

Following on my return to the Air Force after the Challenger mishap, I was the Test Wing commander at Edwards, was the Eastern Space and Missile Center Commander here at Patrick Air Force Base. That was the predecessor organization to the 45th Space Wing, and I was also the commander of the Air Force Flight Test Center at Edwards.

ADM. GEHMAN: Thank you very much. If you have an introductory statement, we'd like to ask you to go ahead; and we're all ears.

GEN. BRIDGES: Well, thank you, Admiral Gehman, for the opportunity to make a statement to the Columbia Accident Investigation Board; and then afterwards I would be pleased to respond to your questions.

The Kennedy Space Center is actively involved in assisting the CAIB with recovery efforts, with approximately 250 people in the field in East Texas. We have an average of 120 others on reconstruction in the hangar here at KSC and 100 working on the engineering investigation.

KSC's role in the nation's space program derives from our two assigned mission areas -- space launch operations and spaceport and range technologies. We're responsible for processing the Space Shuttle from wheel stop until launch, when we hand over the reins to the Johnson Space Center for mission operations. All Orbiter major modifications or OMM since March of '02 are performed at KSC, as well. We're also responsible for providing the facilities and capabilities for the processing of Shuttles, the International Space Station, and expendable launch vehicle payloads.

These payload processing services vary with the desires of the customers, which can range from being a host to doing detailed testing and assembly. We provide host support, processing, and testing services for a wide variety of microgravity research payloads. As a consequence, we often become involved in assuring the success of these science missions in every way we can where we have resident expertise.

We're also NASA's agent for the procurement of ELV launch services for all NASA payloads and managing ELV launch campaigns for our customers at various launch locations such as Cape Canaveral, Vandenberg Air Force Base, and Kodiak, Alaska.

Briefly, in our role as provider of spaceports and range technologies, we design, develop, and sustain ground facilities and ground support equipment for customers as well as science research payloads and advance technology development projects focused in the areas of fluid systems, spaceport structures and materials, process and human factors, command control and monitoring technologies, range technologies, and biological sciences. We're experts in applying advanced technology to solve our customers' problems.

From here on, I'm going to focus on the Space Shuttle program exclusively; and let me detail how we're organized to support the program, as well as summarize my role and responsibilities and that of my direct reports. All of the support that we provide to the Shuttle program from a vehicle processing perspective is performed by the Shuttle Processing Directorate, led by Mr. Mike Wetmore, who is my direct report or a direct report to me.

Mike and his organization, consisting of 377 civil servants, provide government insight and oversight at KSC of the Shuttle contractor, United Space Alliance, or USA. USA performs all the hands-on work until we enter the final countdown at approximately three days before launch. At that point our launch director, Mr. Mike Leinbach, who reports to Mr. Wetmore, takes charge and directs the final countdown and launch as a NASA-led activity. The one other NASA-led activity or mission activity is the post-landing operation from wheel stop until we have the vehicle safe and towed to the processing facility. The NASA landing recovery director or LRD leads that task. The LRD reports to the launch director.

As you know, the USA contract is managed at the Johnson Space Center. We provide technical management representative or TMR services. That's in the contracting officer's vernacular. The specific duties are delegated to us, and we provide a final assessment of the contractor's performance at KSC to the award fee board run by the Shuttle program. The essence of these duties is that we are responsible for day-to-day insight and oversight of USA for the Shuttle program at KSC, as well as for leading the specific activities that I've mentioned above.

In addition, we host several JSC and Marshall Space Flight Center directed activities. These include such things as the Marshall Space Flight Center directed recovery and refurbishment of the Solid Rocket Boosters and the JSC-directed Shuttle Program Integration Office and Orbiter Project Resident Office, which provide program level and design center support at KSC.

Several of my direct reports are responsible for providing typical installation services to the Shuttle Processing Directorate, such as communications, facility maintenance, and propellants. Finally, KSC provides independent safety and health oversight of the Shuttle Processing Directorate and its activities via the Safety, Health and Independent Assessment Directorate.

As the Center director, I'm responsible for the activities I've outlined above. I receive frequent status reports of major activities involving the Shuttle program from Mr. Wetmore and a detailed summary of our status before each Flight Readiness Review or FRR. I sit as a senior member of the FRR with other Office of Space Flight Center directors, and I sign the Certificate of Flight Readiness or COFR after that review.

As you know, I report directly to Mr. Bill Readdy, associate administrator of the Office of Space Flight. I have frequent contact with not only him but with the Shuttle program director, Ron Dittemore, and his boss, General Mike Kostelnik. I view KSC as a customer service organization with respect to our relations with the program. I speak with all of these gentlemen about how well we're meeting their expectations as well as how to address typical problems that arise in a complex program such as this. All of the day-to-day business is conducted between the program and my people at the appropriate level.

KSC's No. 1 guiding principle is safety and health first. I'm very active in leading activities to improve our safety performance in all areas of our operation on a daily basis. Our formal tag-up on these activities occurs on a quarterly Safety and Health Council, which I chair. The council consists of the heads of our civil service and contractor organizations such as USA.

Finally, let me summarize briefly by saying that I'm honored to be a part of the KSC work force. It comprises the best launch team anywhere. Our reputation is for making a system work and keeping it safe, and we're all eager to find the cause of this accident so that we can return to flight.

Again, thank you for giving me the opportunity to address the CAIB. As I told you earlier, Admiral Gehman, I'm fully committed to serving you and the CAIB in doing your important work. I believe that everyone at KSC shares my commitment and stands ready to respond to your call for service and information, as needed.

Thank you.

ADM. GEHMAN: Thank you very much, General Bridges. I'll ask the first question, and then we'll pass it around the board here.

I'm interested in chain-of-command responsibilities and authority kinds of questions, and you mentioned the people who are direct reports to you and that you report to Mr. Readdy. Is that correct?

GEN. BRIDGES: That's correct.

ADM. GEHMAN: I understand that. Now, my question is, in parallel to that line, the authority line, could you describe where you get your money from and how you and your -- first of all, does it follow the exact same chain and how do you justify or compromise or adjudicate differences in priorities, and who does that?

GEN. BRIDGES: Well, the budgeting process is complicated, as you, no doubt, realize; but it does involve an iteration of our requirements to the Shuttle program via Mr. Wetmore and his business office and a feedback from the program of how they receive those requirements and where they felt that they fit within the overall set of priorities for the entire program. There are several iterations of that as we hone the budget to the point where it's ready to go to Mr. Readdy at the enterprise level. If there are any disconnects, typically I discuss them with my counterparts in Houston at my level; and if we are unable to resolve them, then I typically give a briefing either to Mr. Readdy as a preparation for briefing the administrator. And typically I'll have two or three budget issues that I'd like to see done differently. I think I've been noted for being a champion and an advocate for KSC's top-level issues here that I thought merited that kind of support.

ADM. GEHMAN: Then as far as your responsibilities to the Shuttle program, then, you essentially -- if I'm not phrasing this correctly, you go ahead and put it in your own terms -- but you essentially charge the Shuttle program for the work you do here -- or "charge" may not be the right word -- but you and the Shuttle program agree on the size of the portion of your budget that they're going to pay for to do so much work?

GEN. BRIDGES: Yes, sir. That's generally true. It will certainly be true under the full cost environment, that they pretty much get what they pay for. There are some complexities under the so-called business-as-usual budget structure in that my group of civil servants are funded as a whole funding category and then it's up to me to decide where to deploy those civil servants in order to get the total job done.

ADM. GEHMAN: The second half of my question, then, is whether or not you receive any funding directly from NASA headquarters for perhaps infrastructure or something in here that's not directly related to the Shuttle program.

GEN. BRIDGES: Yes, we do. Our construction-of-facilities budget in the past, we on the institutional side have covered a lot of the Shuttle infrastructure. They covered some in so-called program direct. As we move into the full cost environment, most all of those things have been rationalized again and things that are uniquely serving the Shuttle program are going to be handled as program direct, but over the past few years I've been responsible for quite a few construction-of-facilities projects on behalf of the Shuttle program.

ADM. GEHMAN: Okay. Thank you for that. That's responsive. That's what I'm trying to get at.

Can you elaborate more on the kinds of programs that you feel are necessary down here to support our space program that you get funded directly from headquarters rather than the Space Shuttle? I mean, for example, does the Shuttle pay for the guards on the gate, do they pay to have your grass mowed, or do you get that right from headquarters?

GEN. BRIDGES: Well, we charge the Shuttle program for everything that I can direct charge them for. Anything that we can meter, we're already direct charging; and we do feel like that's the best way to go because then they're in charge of the consumption and have a self-interest in helping us regulate that. But there are a number of things that I provide as an institution. Security is one of those things. I do get money from the headquarters as part of my installation budget to take care of security. And as you know, over the time since September the 11th, those requirements have been reassessed and we have improved our security environment significantly, at some cost. I maintain a fleet of four helicopters here which are primarily used for supplementing our security force; and those things do, of course, come at the expense of some other things that I could do with that installation money. So that's one example.

We run a number of laboratories here where, for example, we can do non-destructive evaluation of materials, other chemistry and physics type evaluations. Those are partially supported by the Shuttle program, but in the past we had a fund called ETB, another acronym, Engineering Tech Base, that provided some of the upfront funding for those laboratories and it was up to me to keep those healthy for our program. So I'm just giving you a couple of examples out of the whole portfolio of things that we do for all the programs here.

ADM. GEHMAN: Okay. Thank you very much.

MR. WALLACE: General Bridges, as a former director of the Air Force Flight Test Center, we've heard a lot of discussion about the relative risks associated with operating the Shuttle and whether it is correctly perceived as developmental or flight test activity or whether it's operational or somewhere in between. I would just like your thoughts on that, particularly bringing the perspective of someone who ran the Air Force Flight Test Center.

GEN. BRIDGES: Well, this is a difficult one to answer because, frankly, flying in space and flying in the air are totally different and the vehicles are totally different. There's a small part of the time from 50,000 feet down to the surface which involves a few minutes of Shuttle flight where things are pretty much the same. The rest of it is a different ball game, from my perspective.

The Shuttle is a combination of some things that are operational and some things that we are still learning about because, after all, we only have a little over 100 flights on the vehicle; and as you well know, in a typical flight test program, that's just barely getting started.

Some of the just avionics equipment that we fly on our aircraft now such as our forward-looking infrared sensors and targeting devices that we use, we flew 2,000 sorties on those pods, getting them ready for a combat environment. So modern airplanes do take an awful lot of wringing-out before we're ready to put somebody in them on a dark and stormy night with somebody shooting at them.

We don't have that opportunity in the space business. So it's quite a bit different from that respect.

MR. WALLACE: I'm going to switch topics here. The simple explanation we've been given as to where control shifts from KSC to JSC is T minus zero; but KSC has a role, we understand, in the immediate post-launch video analysis. My question is: Do you then have a role in identifying and making sort of a final call on something that is irregular or an anomaly or a funny or any of those terms that you use?

GEN. BRIDGES: Well, certainly we are an element of the program and we do have certain responsibilities such as foreign object damage, inspections before flight, and then analyzing the film to see if we see anything in it and reporting that to the program. If there are problems perceived, obviously the program brings in other resources to make engineering judgments about how serious those things are.

You're all well aware that we do write up these debris reports of what we see as well as what we observe on the vehicle once it comes back. And those things are documented and I think you've all seen copies of those. That's all certainly of concern to us, any type of damage to the vehicle that we see after a flight and where it came from. So I'd say our people are pretty intense about doing that.

MR. WALLACE: You know, I understand. Of course, people are heavily focused, and have been, on this famous falling foam. In addition to providing the video expertise, I mean, do you have a role, then, in deciding whether this thing ultimately gets specifically identified as an in-flight anomaly or not?

GEN. BRIDGES: Not to my knowledge. I'm, of course, aware of those kinds of discussions going on within the program. During this mission I was certainly aware that we had some debris that caused a shower of particles on the wing. I saw the photographs during the mission and I was also advised once that judgment had been made about how the program felt about potential for damage. To my knowledge, we didn't have any direct role in that particular analysis.

GEN. DEAL: I'd like to ask you one general question about being center director, then I'll follow up on Mr. Wallace. Can you kind of describe your relationship with the Marshall Space Flight Center and Michoud in particular with regard to the external tank and, if you have any out-of-family conditions or problem reports that are generated here, how your center deals with them?

GEN. BRIDGES: Well, they are a design center, just like JSC is for the Orbiter. So when we find something that's so-called "out of family," we would be dealing with the Marshall Space Flight Center on those items. In an Air Force vernacular, we're kind of the maintenance organization here. If we can take care of something within the tech order, we do it. If it's something out of the ordinary, then we have to get back with the engineers at the design center to figure out how to disposition those particular types of problems. So we work with engineers at Marshall, just like we do those at JSC, to try to resolve any problems we see with the engines or the tanks.

GEN. DEAL: The other is a follow-up to Mr. Wallace. Not as center director but your experience at Edwards and also as an astronaut. If you think that you had, for example, a test aircraft that's flying 112 flights and it's had five panels fall off, you'd probably stand down your fleet, in my opinion, to fix it so you didn't have those five panels fly off. I wanted to try and get your perspective of how we may have had five or more pieces of a particular part of the external tank fall off, yet we continued to fly.

GEN. BRIDGES: Well, certainly we're interested in anything that falls off of test aircraft and anything that could cause a problem, but I will tell you that the desert floor around Edwards is littered with so-called F15 tail feathers which were little flaps around the engine nozzles and other things like that that did not work out too well on aircraft but were not thought to cause damage. And while we really didn't like dropping things on the desert out there, in order to get the test program moving forward, we did not ground the fleet every time we had some minor thing like that happen.

So really, I think, it depends on what the potential for damage was. We have engineers out at Edwards, as well as the program office. If it was a safety issue and I thought it was a severe safety issue, certainly I would engage and recommend that we stop flying until we fix it. If it's not a safety issue, we certainly tried to come up with some kind of a fix to keep them on there because of the potential for hitting somebody or just, well, let's just take them off, you know, until we can figure out how to fix this thing, if we can fly without them. So, amazingly, with developing airplanes, I think we've been through all of those scenarios two or three times during my tenure out there.

GEN. DEAL: So you had a level of comfort, I guess, based upon the analysis presented, that the bipod ramp was not really a safety-of-flight type of issue.

GEN. BRIDGES: To be honest, I did not think that the bipod foam coming off had caused any significant damage in the program to date. I believe it came off about four times before that we knew of. I personally looked at every Shuttle that's come back during my tenure here. I've seen no significant damage from any of the foam coming off. It has certainly been a maintenance concern. It's a lot of work to go out and have to repair all of those things, and we don't take that lightly. I mean, we want to get to the root cause of those things and get them fixed.

I personally was not aware there was any safety-of-flight concern with the ramp foam coming off prior to this flight. Had I been aware of that, I certainly would have put my hand up at the FRR that we would stop flying. I think this is certainly a surprise to all of us.

GEN. DEAL: Thank you, sir.

ADM. GEHMAN: General Bridges, I'd like to go back and follow up on a question that was asked before -- that is, the role of KSC, the role that KSC has in the processing and the preparation for flight of the external tank. The external tank, it's my understanding, essentially comes here almost ready for flight but there are some processes that KSC does, is responsible for, having to do with the foam insulation on the tank. Am I not correct, that you do do some foam work on the tanks?

GEN. BRIDGES: I'm not familiar with the details of that. So I'm not going to try to get into it; but, yes, I am aware that we have done work on foam. We do do foam repairs. We have dented foam. We have sanded foam in trying to take care of problems. We do inspect to make sure the foam is okay. So there are a lot of things like that we do, but I'm not aware of the exact details of all those particular operations that have gone on.

ADM. TURCOTTE: Good afternoon, sir. Getting back to what we were talking a little bit about your role as essentially the mayor of the center. A lot of facilities. A lot of planning goes into the maintenance of those facilities. A lot of programmatic responsibilities across several lines. Over the last eight years or so, a lot of programs have been up and down. There's been extensions on the Orbiter. There's some other programs' deadlines come and gone. A lot of facilities there have been there quite a while. Could you explain to the board today the process by which you have been able to stay in front of this process and planning to keep the aging facilities going or your lack of ability to do that or the funds that you have that both come through programmatic ends and also through the direct line through NASA headquarters?

GEN. BRIDGES: Well, in preparing for that potential question today, I did review a few things; and I'll have to say that we've been beating the drums pretty loudly and rapidly, since the time I've been here, over what we saw as a coming problem with facilities that need some major maintenance and are going to require a lot of dollars. I did that with the full cognizance of my boss -- first of all, Mr. Rothenburg that was there before Mr. Readdy.

We were getting on average about $19 million a year for the four years '96 through '99 in our construction facilities budget, which we could tell was just not going to handle this problem, particularly when you have something like a VAB that by itself can eat up over $100 million to get siding and roofs and doors fixed. So we started beating the drum and in '00 through '03 we've averaged over $60 million a year in construction-of-facilities funding. While I don't totally trust the so-called BMAR or backlog of maintenance and repair, because there are some squirrels as far as how different people count things, we keep a metric on that and, amazingly, over the last four years the BMAR has been steady or declining.

The one other thing that I took on as a personal vendetta was the large number of square feet of trailers, trailers that have been here for 20 years and we have people living in them. I took a tour of some of those when I first came down here and, frankly, I was appalled and believed that it was a safety issue not only with the facilities themselves but I thought that when you have your maintenance technicians working out of delapidated and rundown facilities where they have their breaks and have their offices and then you walk over into a pristine facility where we keep the flight hardware, there was just some kind of disparity there that I thought was not right and would probably bleed over into maintenance after a while because, after all, our people are our most important asset here for maintaining the safety of the overall system.

So we started going after these trailers; and we now, with programs under construction, buildings under construction, and buildings completed, we've got over 500,000 square feet down to 50,000 square feet. The biggest of those buildings, we just started the construction process. So it will be about two years before people move in, but we've been able to make a big dent into that and we have already cut the ribbon on many of our operational facilities that support people in the Shuttle program here.

So I think we have prioritized things pretty carefully. We have gone after things that would have a tendency to pay back big-time. Like if I can quit doing repairs on delapidated trailers, that's more money in repair that I can put in on my more permanent facilities.

So we've done a lot of things like that. We've really charged hard at energy efficiency in order to reinvest that money into maintenance. And I won't bore you with a lot of the other details and programs but we have worked this very hard to stay out in front and I believe that the program and the agency understood what was going to happen and they began to program more resources to go against my facilities -- although I'll have to tell you that it was a thing I laid awake at night about a few times, wondering how I was going to get some of these things done. But we did get them done and I think things are on the right track now.

MR. HUBBARD: I'd like to turn to a more general management issue now and talk a little bit about the concept of insight and oversight, especially in dealing with a large contractor work force. Years ago some interpreted oversight as almost a shadow work force in relation to the contractor; and, of course, that's evolved a great deal. Can you tell us a little bit about how you would define those terms of insight and oversight and maybe a little bit about how it's changed with time in your six years?

GEN. BRIDGES: Well, we had to really write the textbook on that, I think, when we decided to go with the Space Flight Operations Contract and turn that into a performance-based contract and move NASA out of the hands-on, you might say, with a level of effort support contractor into doing insight and oversight of a contractor that's leading all the day-to-day activities. Well, we just had to understand this. We did a lot of benchmarking of people and did come up with a risk-based insight-and-oversight system which I think I call world-class surveillance; and it is an overall surveillance system.

The oversight is, simply put, a place where we have in-line approval. That is, the contractor does not do the work before we either approve the document or before we have a set of eyeballs there to watch the work. That was oversight.

Insight is a series of techniques, depending on the criticality. It could be in-depth observation where we want to observe a critical process in depth, you might say, from beginning to end, all the way down to customer feedback where, you know, you send out a survey and find out how somebody likes something. And there are a number of things in between as part of the surveillance plan.

What we have done within the Shuttle program is written up a number of implementation plans for our surveillance plan that define exactly where we employ each of these techniques, depending on the risk in each of our procedures. Those systems are a closed loop in that we get feedback on the critical ones into our COFR process; and the COFR signature depends on us having completed those things. So, for example, if we're supposed to do 8500 government-mandatory inspection points per flow, if we miss one, that will be an anomaly that we have to explain when we go through the COFR process.

MR. HUBBARD: Okay. To follow that up a little bit, over the same period of time that you migrated from one way of managing the contract to another, how has the civil service and support service contractor work force changed in quantity or in types of work that are done?

GEN. BRIDGES: Well, the civil service work force has gone down dramatically because of the shift in our relationship with the contractor and how many hands we need to do the work. The contractor work force has gone down slightly, primarily due to efficiencies we found, just better ways of doing things. And it was a big cultural change for us, as well. We had to work this very hard, and the transition from NASA to the contractor was something that was done very deliberately and with quite a bit of discussing and making sure that they met criteria before we turned things over to them.

So I would say, frankly, making this change is something I doubt that any other government agency has been able to do with a system this complex; and I personally am very proud of the work force here, that I think they have done this extraordinarily well, both on the civil servant as well as on the contractor side. But we worked this very hard.

MR. HUBBARD: As a final follow-up, then, how do you maintain currency or develop new government and engineering talent if they are at arm's length from the hands-on work?

GEN. BRIDGES: Well, it's a difficult problem. I don't think we've totally solved that problem yet. We're still working it. We have tried things such as taking co-ops and letting them work with the contractor down on the floor where we have very young people, fresh-outs; and we have made liberal use of cherry-picking the contractor by taking a mid-career person and hiring them after they have a lot of hands-on experience. And I would anticipate we'd make liberal use of that in the future.

MR. HUBBARD: Okay. Thank you.

DR. LOGSDON: I want to talk a little bit about flight rates, Roy. What has been the average flight rate for the Shuttle over the past two or three years?

GEN. BRIDGES: Well, we've gone down to as low as three and, I believe, as high as seven.

DR. LOGSDON: What was scheduled for '03 and through core complete on the Station?

GEN. BRIDGES: We had initially made some plans to go down as low as four; and I believe now, with the recent budget decisions, that will be five.

DR. LOGSDON: Weren't there more than that planned for between January 1 of '03 and the end of February '04 in order to get all U.S. parts of the Station up?

GEN. BRIDGES: You know, we have changed the manifest so many times this year, I'm afraid to say a number; but I do believe at one time we were about six in this fiscal year.

DR. LOGSDON: I guess what I'm getting at is: Was there any sense of schedule pressure with the date, Mr. O'Keefe stressing the date of core complete so strongly as a management tool?

GEN. BRIDGES: Well, it certainly was something that I was aware of as I was trying to make sure that we did not lapse back into that mode. I was, of course, an active astronaut before Challenger and was watching the flight rate go up to one a month about the time I was flying and was aware of the intense schedule pressure during that time frame. So I would say I was pretty highly tuned to trying to make sure we didn't fall back into that situation and worked with my direct reports here on a weekly basis to make sure that we were letting our people know that when we saw anything that was of concern, that our culture was we could put up our hand and stop. Of course, you all know the story here. We saw little cracks in the flow liners, and we stopped. That's just probably the best example of something that we saw and we put the fleet down until we had it fixed.

So I saw a completely different reaction and attitude on the part of not only the work force but all the way to the top-level management in our program of how we dealt with problems that could impact a schedule. So, yes, in fact, we did want to finish the Station; and we were on a roll. We would have liked to have finished it in February of '04. It would have been, frankly, a brilliant achievement if we could have done that; but we were not going to let things like flow liner cracks or any other items like that that popped up be, you might say, squashed in order to meet that schedule milestone. I never felt any concern that if we brought this up to Bill Readdy or the administrator, Sean O'Keefe, that they would do anything except applaud us for letting them know that we had a serious problem and we need to take a timeout to fix it.

DR. LOGSDON: Shifting gears a bit, you said you had 377 civil servants overseeing the work of United Space Alliance. How many USA people are involved in Shuttle processing?

GEN. BRIDGES: Well, the numbers I have on my sheet -- and you could ask them to get a better clue -- but the end of fiscal year '02 was 6557. That's USA plus subcontractors.

DR. LOGSDON: Here?

GEN. BRIDGES: Here.

DR. LOGSDON: What happens if your overseers are not satisfied with the performance of individual USA employees? Do you have any leverage?

GEN. BRIDGES: We can stop work. We can have the work done again. We can make sure they get a very poor award fee. We are not responsible for hiring and firing and any other discipline that USA might want to take, but certainly we don't have any problem bringing unsatisfactory performance to the attention of their management. I meet often with not only Mr. Pickavance but also other heads of contracting organizations where we will discuss things that we're not happy with; and we do that outside of award fee boards.

DR. LOGSDON: Thank you.

ADM. GEHMAN: General Bridges, the number you gave, 6500 more or less, that's contractors?

GEN. BRIDGES: 6557 was the USA plus subcontract number that I was able to glean.

ADM. GEHMAN: What is the government work force?

GEN. BRIDGES: The government work force for the Shuttle program in Mr. Wetmore's organization is 354 full-time equivalents; but counting temps and terms and other things, we've right now got about 377 belly buttons or faces on board -- I guess that's a nicer way of saying it. And across the whole center, we have, people charging to the Shuttle program, 549 civil servants.

ADM. GEHMAN: The trend obviously has been down, but how rapid is that trend? When did this start and what were the big years in transition?

GEN. BRIDGES: Well, it was a steady downward trend at the center from '92 through '99. We went from, in terms of faces, about 2498 people in '92 at the center, had a low point in '99 of 1687, and at the end of FY '02, we're at 7073. That's full-time permanent people, not full-time equivalents.

The Shuttle program, primarily because of the shift in our relationship between the government and the contractor and not needing as many doers in the Shuttle processing organization -- and it takes a little manipulation of numbers because we've reorganized a couple of times over the years -- we believe went from about 1075 down to 354 in terms of our full-time permanent work force over that period of time, and from about 1433 down to 549 from '92 to '02 in terms of people across the center charging to the Shuttle program.

ADM. GEHMAN: I'm not asking for specific numbers here; but when the Shuttle processing was shifted from Palmdale out to here, what happened to your government employee work force, in round numbers?

GEN. BRIDGES: I know that USA was on tap to hire several hundred people. I don't know the exact number. That was in negotiation with the program. We were assisting the program to try to make sure we had the right skills and the number of people here, and it was primarily USA hire-up.

ADM. GEHMAN: What about the government side?

GEN. BRIDGES: We did not hire up anything.

DR. OSHEROFF: This is my first question ever on one of these things.

Given the fact that there are roughly 20 USA employees for every NASA oversight person, I'm interested in what the relationship that you have with USA and with the parent organization, NASA, with regard to innovation and changes perhaps in procedures reflecting new information that has come to light.

GEN. BRIDGES: Oh, NASA to date has been a champion of innovation. That was certainly more true in the early days than it is today. We believe that as the contractor got more experience with leading rather than waiting on NASA to tell them what to do that they became a lot more innovative in employing new information technology and other procedures in order to improve how they did work. They were strongly incentivized to do that, particularly if it would save them money.

We in NASA have been very, you might say, pushy in terms of some of the more high-risk technology where you have to make an investment and prototype the technology before someone would be willing to put it on the vehicle. And those are typically things in the upgrade area where you're talking spending several hundred million dollars in order to prototype something and get it qualified before you put it on the vehicle. USA has been less interested in those type of things.

DR. OSHEROFF: So when it comes to, for instance, figuring out how one could deliver larger payloads in highly inclined orbits, who's taking the lead on that?

GEN. BRIDGES: That's a NASA job; and NASA, as we began to improve the Shuttle so that we could do a good job of building the Station, went through a number of Shuttle upgrades such as the super-lightweight tank that would give us a lot more cargo-carrying capability so we could do the Station job. And I would say the Shuttle program did an excellent job of that.

DR. OSHEROFF: What part did your center play in those activities?

GEN. BRIDGES: In terms of all of those upgrades, we'll tend to have some impact on how we process the Shuttle. For example, if you're putting in a glass cockpit rather than what we call steam gauges, there are going to be changes in maintenance procedures for taking care of those. So our people have to be trained. We have to rewrite our procedures and go through a process of making sure that we understand the new technology so that we can turn it around very reliably.

DR. OSHEROFF: In that process of doing that, where, for instance, does the information tend to come from that procedures have to be changed? Is it from the USA people or from the Kennedy Space Flight Center people here?

GEN. BRIDGES: Well, I would say it's a team effort. We work on these upgrades together. We have gotten far past the throw-things-over-the-fence era; and these days we are working very close together, NASA, USA, Boeing, and whatever other vendor is helping us with this particular upgrade, to put a team in that's looking at the total life cycle of how to do this. So typically things like that that are not a big surprise and are handled fairly seamlessly here. I can't remember one, single big problem with the new glass cockpit on this first launch. Very smooth.

DR. OSHEROFF: Thank you.

DR. LOGSDON: Roy, I think you said in your statement that civil servants take over three days before launch.

GEN. BRIDGES: Yes.

DR. LOGSDON: Let's expand on that a little bit. I mean, does USA totally go away at that point and the ice inspections, the on-pad inspections, and then the actual launch control is all civil servants?

GEN. BRIDGES: No, what I meant to say is it was a NASA-led activity, whereas the day-to-day processing activities are USA-led activities where we only become involved in the insight and oversight. During the last three days of launch, our launch director is directing what amounts to a badgeless team. The team is made up primarily of USA employees taking direction from NASA in the role of a launch director.

DR. LOGSDON: These are different USA employees than the ones doing the processing?

GEN. BRIDGES: They're the same people that sit on console during the processing. They're our first team.

MR. HUBBARD: I'd just like to follow up a little bit more on the changes in the work force over the last six or seven years and where the reduction of some 700 people occurred in the insight/oversight of the contract and the contractor. Were those changes primarily in engineering areas, operations, SR&QA, or was it across the board?

GEN. BRIDGES: It was across the board. In fact, this morning I was curious. I hadn't really looked at the numbers in a while and I tried to roughly see whether or not we had downsized the S&MA work force more than, you might say, the average downsizing. Surprisingly, from the numbers I read you earlier, the S&MA people charging to the Shuttle program back in this '92 time frame was about 26 percent of the processing work force. Today it's 28 percent. So it actually went up a little bit within the Shuttle program of S&MA people.

I would say we're really focused on trying to find the right number for the Shuttle processing organization. We went too far. In the summer and fall of '99, I was on guard channel with our headquarters that we're having serious critical skill problems. We had had seven years of downsizing, five buyouts, and we were well down below 1700 people overall; and I began, like I said, making emergency transmissions that I needed to have hiring authority for critical skills.

In December '99, I was advised that I could do critical-skill hiring and, in fact, that my downsizing had been terminated. The next spring we reorganized the center and stabilized the Shuttle work force at around this number that

I've given you, around 375 people. I think you'll find that since that time we have been pretty rock-solid steady.

My comment has been, well, you know, I think we have distilled this number by fire in terms of what we need. Certainly there can be changes that cause us to reassess this, problems that come up, new work, work that goes away, whatever. So, of course, we have to periodically look at it. But assuming the work requirement doesn't change, then those 375 people approximately will be here and I will be, you might say, the last guy to turn the lights out before we start dipping down into that 375. But we've been holding the line on this.

MR. HUBBARD: Just as a follow-up to that. As we know, the space exploration business is terribly unforgiving and part of where a lot of added value can come as you're preparing for a launch or developing a mission is not only looking at the mainstream of the program but also thinking about off-nominal situations. Given that you've gone down to this 375, where does that thinking occur now? And is there still enough to do this in the civil service work force or is it in some other piece of the contractor work force?

GEN. BRIDGES: Well, I think it's both. Certainly we want the contractor to be more proactive in dealing with all types of situations like that. We have certainly encouraged it. I think, as I've indicated by our earlier statement, they've come along and are doing very well.

Our launch director though, as far as contingencies on launch day, we constantly train the entire team with some very high-fidelity simulations about how to handle any and all types of things that we might observe during that time. Of course, time is critical on launch day if we want to preserve the launch attempt -- and there are safety risks, once you put fuel in a tank, for just standing down, although that is always our bailout option. If we can't figure it out, we'll not launch that date; but nevertheless there are a number of minor things that if we really have trained well for them, we can safely accommodate and preserve a launch opportunity. And we have invested considerably more resources into training a team to do that and do some of the thinking.

As you well know, there's also a lot of intellectual time that goes into hazard analyses and FEMA cells and updating of procedures and things like that not only to accommodate minor incidents or close calls that we have but also try to just improve them and make them more robust so that we don't have problems like that.

We have also experimented and done a number of so-called process FEMAs where we will go and look at a fairly difficult process where we seem to be putting people at risk of maybe a sprain, you know, because, you know, they're having to handle a piece of equipment that's too heavy or trying to reach too far. And we've used some of our simulation capability to go out and actually redesign the support equipment to make it easier to do those kinds of operations. So all of those things are part of trying to be more proactive, look ahead, and try to decrease our incident mishap and in-flight anomaly rate.

MR. HUBBARD: Thank you.

MR. WALLACE: General Bridges, my understanding is there's this Launch Readiness Review done here two or three days typically before a launch and the formal Flight Readiness Review is typically a couple of weeks earlier. My question is: Where are the most likely sort of stop points? Because we've been told that in the Launch Readiness Review, there's so much that's gone into the preparation up to that point that it's sort of usual that at the Launch Readiness Review there would be something raised that you weren't already working, something new that stops it. I'm not talking about weather or something that's inherently a last-minute thing. Even issues like you mentioned the flow liner cracks and the BSTRA balls. Where are the most likely stop points in this whole process?

GEN. BRIDGES: Well, the stop points are whenever you realize that you can't make it or that you just have to reassess your approach to a particular launch operation. So they can happen at any time from two seconds before the SRBs light to weeks before the launch. Anytime we run into a problem that is new to us or is going to cause us to have to reassess our plans, if we think we can get that problem solved before launch, we may continue with the Flight Readiness Review and give a progress report on how well we are along with solving that problem, with an understanding that typically at the 0-minus-2 review we have to have the work finished or we will have to delay. And I have seen a number of situations like that where we will run down fairly close to launch, usually not past that point, certainly not past the tanking. We don't tank unless we're ready to go fly.

MR. WALLACE: Most of us never do anything that approaches the complexity of launching a Space Shuttle. So all the processes involved are almost overwhelming if you come in from outside, look at all these processes and all these check points and all these cross-looking organizations and processes. I mean, my question is whether you can almost get to a point of dilution with so many processes that there comes to be almost an assumption that it will get caught somewhere. I really don't mean to ask a question that's judgmental. I mean, I just want your thoughts on that.

GEN. BRIDGES: Well, the process depends on having very good people of high integrity that are very passionate about their work and don't pass work unless it's been done correctly. I think we've seen over and over through this program that we do have people like that that are working here. That's what makes every flight safe, and we have obviously over 100 examples of that.

We do miss things from time to time; and, you know, we'd like to make our processes more robust, less likely that some miss, some distraction might cause us to do something or omit something that would be important to us. And I would say we are constantly reviewing and trying to make sure that we have the right person at the right time focused on the job to make sure that we have good information that tells us that a process has been completed, all the data from the process is in family, and it's just a matter of checking off each one of those things, using this work force of very high integrity, very focused people.

Yes, it's certainly quite an accomplishment that we are able to do this safely; and it would be nice if we could find ways to use new technology that would make it less cumbersome, less labor intensive, and less prone to human error. Sometimes those new techniques or tools bring their own complexity, certainly in trying to integrate them into something like the processing operation we have at KSC. For example, if you tried to take our current systems while we're flying and replace them, it is sort of like getting a heart transplant while running a marathon. So we have to be very, very careful about how well we test and those kind of replacement systems; and we have to be very deliberate about any changes we make. But I think in comments that we've gone over today, I believe that we have demonstrated that we have been able to do relatively major changes in a very safe way.

ADM. GEHMAN: General Bridges, did I understand you to say that your current government work force is something like 350 people, civil servants?

GEN. BRIDGES: My current KSC work force is 1850 people.

ADM. GEHMAN: But in the Shuttle program.

GEN. BRIDGES: In the Shuttle program, we have, counting temps, terms, and co-ops, 377 people.

ADM. GEHMAN: Once again, this is not a test. How many of them approximately are in the S&MA world?

GEN. BRIDGES: We charge about 100 of those.

ADM. GEHMAN: About 100 of them.

GEN. BRIDGES: Right.

ADM. GEHMAN: Would you estimate that, by and large, most of those 100 are oversight kinds of people -- I mean they're checkers, they're people who sign off on processes and procedures?

GEN. BRIDGES: These are primarily our safety and quality assurance technicians and their management.

ADM. GEHMAN: My question is: Of those hundred, would you say that most of them are people who are involved in the signing-off of processes and procedures?

GEN. BRIDGES: They're there observing and, yes, stamping and signing off things; but I would like to, if I could, just add to that. Since we've gone to this performance-based contracting approach, all the rest of the people in the organization are involved in some type of insight or oversight activity. That's all we do other than, like I said, the two NASA-led activities that we have. So this is why we felt comfortable in reorganizing our safety and mission assurance organization, is to try to get more synergy between our traditional safety and quality assurance technicians. Now with those engineers that are also out observing the contractor and doing the insight and oversight role, trying to increase the teamwork and the communication among those because, in essence, that entire organization now is doing an S&MA type activity.

ADM. GEHMAN: However that's organized, is the safety and mission assurance person a direct report to you?

GEN. BRIDGES: We have split out our safety and mission assurance so that the people that are involved in stamping and doing things and supervising the contractor, like all the other people in that organization, report to Mr. Wetmore, that 100 people. We have an independent assessment organization with a direct report to me that does the independent assessment of how well they're doing their insight and oversight job, and that's additional people beyond that 100.

ADM. GEHMAN: That's a nice lead-in. What exactly are the duties and responsibilities and size of this independent assessment office?

GEN. BRIDGES: The total office, we call it Safety, Health, and Independent Assessment, is on the order of 65 people. At the time of STS-107, we had a very experienced Senior Executive Service leader of the organization. We also in the organization have other high-grade people, SESs or similar high grades, that, for example, one is our chief safety officer for the center and our safety ombuds. We also have our chief systems engineer in that organization that does engineering oversight of all of our development projects, and we have an organization that does all of the audits and assessments for all types of audits and assessments. And we draw on those resources, whether it be for safety or for an ISO 9001 business system or some other type of program.

ADM. GEHMAN: And they are all government employees? None of this is contract?

GEN. BRIDGES: These are all government employees, and we have tried to be selective in that the grade of the organization is a cut above the average in our work force and all of the people are highly skilled so that they will be sought out for their consulting ability as well as, you know, they occasionally have to render a judgment on whether or not folks are complying with things.

ADM. GEHMAN: I'm going to change subjects on you now. In the SFOC, the Space Flight Operations Contract, how are awards to the contractor determined?

GEN. BRIDGES: I'm not the expert on this. So it's a program function.

ADM. GEHMAN: First of all, it's a program function.

GEN. BRIDGES: Yes, it is. So it's not a function that I do here, although Mr. Wetmore does make an input into the program of how well the contractor has done.

ADM. GEHMAN: Even the processing part of it is a program function, not a center function?

GEN. BRIDGES: That's correct. There's a consolidated award fee process for the Shuttle program. We have one of the inputs into that. That goes together with the other inputs, and a score is recommended to the fee-determining official, who is General Kostelnik at the headquarters.

ADM. GEHMAN: Okay. So the award levels are determined at NASA headquarters, based on inputs from lots of people.

GEN. BRIDGES: Right. The fee-determining official is General Kostelnik, who, of course, is over both the Shuttle and Station programs but who's stationed in Washington.

ADM. GEHMAN: Okay. Is there anybody besides the Shuttle processing manager, Mr. Wetmore, from here, who makes a formal award fee input?

GEN. BRIDGES: We also make an award fee input on logistics, integrated logistics from here; and at one time when we were doing the checkout and launch control system project, we were making an input on that. But that's no longer being done. That project was canceled.

ADM. GEHMAN: I'm going to change subjects again on you. The shift from Shuttle processing from Palmdale to here, the Board has been told that the number of employees who moved with the function was something in the order of 15 to 20 percent of the work force. I haven't got that number pinned down, but does that sound like what you've been informed?

GEN. BRIDGES: I couldn't say.

ADM. GEHMAN: Are you aware of any work centers, KSC Shuttle processing work centers that, due to lack of experienced, mature workers, were in any way under closer supervision or closer scrutiny just because they just didn't have the experience when you started processing Shuttles? Were there any procedures to identify work centers in which you had essentially all new employees?

GEN. BRIDGES: Well, I'm trying to make sure I've got this correct. Now, I know there was some concern over the movement of Boeing personnel from Huntington Beach to Houston where people were given extra supervision. That really didn't have anything to do with me here; and while I was insistent on having those metrics as a member of the Flight Readiness Review Board and certainly followed them to make sure that we had a good skill base for doing that work, I think it was a completely different situation with regard to the OMM. We have a very highly skilled work force here for doing the type of work we do at OMM, and to me it was a matter of just supplementing the work force that we have to take on the extra work when we were going to be processing all four Orbiters here. I don't think that we were underskilled in any particular area nor were we at risk in any area. Had we been, it would have been a schedule issue rather than any type of safety issue.

DR. LOGSDON: You mentioned earlier, a little bit, facilities. We all know that there are discussions, certainly pre-accident, of flying the Shuttle 10 to 20 more years. What kind of facility investments and improvements will you need to be able to do that?

GEN. BRIDGES: Well, we had a conference on that last Wednesday and Thursday in New Orleans where we are looking in depth at the Shuttle Life Extension Program. This is the first year that we've taken a very rigorous look at it, although there have been many studies on this from time to time in the program. I believe the new process that we have kicked off will be very useful in making sure that we really get the highest priority capital improvements in the program to make sure that we can safely get to 2020. So I was very impressed with the process that General Kostelnik inaugurated, and I believe that it will serve us very well.

So what kind of things do we know of here? Frankly, we have some big dollar items that are not very sexy, like a new roof and siding and doors on the VAB. That's a very, very large dollar item. Those type of things did not get much discussion at the conference last week because they're pretty cut and dry. As I mentioned earlier, I think that we're getting pretty good support for those here.

Activities that I spoke about at the conference that I feel like we need more attention to are things that would help us be more predictive and proactive. For example, I would like to see a much stronger fleet leader program in other areas than the engine so that we could predict things like flow liner cracks and not have those be a surprise to us downstream. So as a general category, I would put that at the very top of the list to try to make some investments in those type of test facilities and additional resources to make sure we do that very well.

ADM. GEHMAN: General Bridges, these Orbiters are now 20-plus years old; and as they go through processing, it's possible that you will begin to see symptoms in these Orbiters, as they go through the KSC processing facility, that are similar to what you had in your previous experience in the case of aging military aircraft. The most obvious is corrosion, which is already well inspected for. All of us have had our heads into wing spars and things like that where corrosion is looked for; but there are many, many other signs of aging aircraft. Do you believe that you have the infrastructure in place, that is, the non-destructive test equipment, the non-intrusive kinds of measuring devices, and the time to make an evaluation of whether or not these aircraft are aging?

Someone told me, for example, just informally, that Columbia, for example, had spent, if you added up all the time she sat out on the launch pad, she was out there for over 2 1/2 years altogether -- not continuously, obviously.

And in between every time, she was gone over with a fine-tooth comb. Nevertheless, that's a lot of time to sit out there in the Atlantic Ocean environment. So are you content, or are there some things you're going to have to do in the processing facility in order to make sure these Shuttles are safe to fly for this extended period that's been proposed?

GEN. BRIDGES: Well, I think, to answer your question very directly, that we do need to invest in additional non-destructive evaluation equipment and have state of the art here in order to do a very good job of that; and that was one of the projects put on the list at this select panel last week. I am a little more concerned, though, about the more, you might say, nontraditional things. I think we tend to know how to inspect for corrosion; and, yes, it would be nice to have the latest equipment to do that so we don't have to tear the vehicle down any more than absolutely necessary and so that we can get in some of the difficult environments that we have to get into. But I think this issue of this being a first of a generation of reusable launch vehicles, that the fleet leader program being done more comprehensively would tend to help us spot things that could be very long lead recoveries.

This flow liner thing was -- it took the best in the agency for us to be able to pull that through that in a few months. And it was a spectacular achievement but you can imagine in some cases if we had to go re-manufacture some of the parts we have here, if they're not on the shelf, it could be quite a lengthy downtime. So we would like to get more out in front of that, as well as to avoid some kind of a nasty surprise which would not be just grounding but perhaps would have resulted in some kind of a mishap.

ADM. GEHMAN: Well, the board thanks you very much, General Bridges. I hope that you will pass on to your entire work force the respect and admiration that we have for how hard and how diligently people are working on this tragedy. We spent the morning at the J hangar, looking at debris, and came away quite impressed with the zeal and the professionalism, the energy that's being displayed out there and in the OPF and every other place that we've been. So please pass on our thanks and admiration for the hard work, and I know that you have got 250 or 235 of your people spread all over Louisiana and Texas that are also helping in the debris recovery effort. They're away from home. So we realize how much effort is going into this. So thank you very much.

GEN. BRIDGES: Certainly will pass it along. Thank you.

ADM. GEHMAN: We will take about a four-minute break while we seat Mr. Higgins.

(Recess taken)

ADM. GEHMAN: The next person we're going to hear from is Mr. Bill Higgins from the KSC Safety Division.

First of all, Bill, before we start, I would like for you to affirm to this board that the information you provide today will be accurate and complete, to the best of your current knowledge and belief.

MR. HIGGINS: I so affirm.

ADM. GEHMAN: Would you please tell us who you are and what your job is and how long you've been there.

BILL HIGGINS testified as follows:

MR. HIGGINS: My name is Bill Higgins. I am currently the chief of the Safety and Mission Assurance Division and the Shuttle Processing Directorate at the Kennedy Space Center. I've been at KSC since 1987. I started with NASA in 1983. All of those years have been in various safety and reliability and quality engineering and management positions.

ADM. GEHMAN: So you are the chief of safety in the Shuttle Processing Division?

MR. HIGGINS: Yes, sir.

ADM. GEHMAN: Is there also a KSC chief of safety?

MR. HIGGINS: Yes, sir. In the Safety, Health and Independent Assessment Office, the associate director for Safety and Mission Assurance is in that office; and that is the chief safety officer also.

ADM. GEHMAN: What's your relationship to that person?

MR. HIGGINS: They do an assessment of our performance and --

ADM. GEHMAN: Maybe I'm getting ahead of you here. You probably are going to cover it.

MR. HIGGINS: No, that won't be covered. This is just about us. Their job is to watch and see what we're doing and we take some advice from them and if they find deficiencies or noncompliances, we'll correct those.

ADM. GEHMAN: Before I launch into questions, why don't I go ahead and invite you to make an opening statement and then we'll save our questions.

MR. HIGGINS: I don't really have an opening statement. I was asked to provide a brief overview of safety, and I have a few slides here. If you'd like, I could go over those.

ADM. GEHMAN: Yes.

MR. HIGGINS: Okay. I'm going to go over the safety and mission assurance roles that we have at KSC. This is very brief. The KSC Safety and Mission Assurance functions, we have just a couple of significant deliverables, even though there are quite a few different things that we do, and I'm going to show you briefly what our KSC Shuttle processing S&MA organization is.

The roles that we have, there are two main players in

Safety and Mission Assurance and Shuttle processing at Kennedy Space Center. The first one is United Space Alliance. They are the Space Flight Operations Contractor. This is a performance-based contract, as Mr. Bridges stated. They are responsible for the vehicle processing and they are responsible for quality control. When they step up and talk about the vehicle is ready, they're the ones who state the vehicle is ready.

They are also responsible by contract for all the personnel safety in the USA areas. So for all of the personnel that is, for instance, in the Vehicle Assembly Building or the Orbiter processing facilities, those are USA facilities and they provide the institutional safety responsibility in those areas, including the NASA people that go in there. The NASA people that go in there must follow their rules.

The NASA KSC, we're responsible for final acceptance of designated critical hardware at specific points in the processing and we do that through the Government Mandatory Inspection Points and we are responsible for evaluating the contractor performance of their assurance function. And that's our insight function. We utilize those inspection points in that assessment, but we do other assessments and audits of their programs and processes to make that determination.

On the KSC side, we have three main functions. The first one is the Certificate of Flight Readiness. The requirements for that flight readiness come from NSTS 08117. That is the Space Shuttle requirements and procedures, and also the KPD 8630, which is a Kennedy Space Center document, which describes how KSC Shuttle processing certifies and reports to the LRR and the FRR.

We also do the Shuttle safety and mission assurance award fee, and there are two processes that we use. KDP-P is a Kennedy process. We have a surveillance plan for the Space Flight Operations Contract and then our particular division has its own implementation plan for that and that is what provides the information to the technical management representatives as to how the contractor has performed in this award fee area. We provide an input to Mike Wetmore in terms of ground operations, and to (unintelligible) in terms of logistics, and also to Bill Harris, the safety and mission assurance TMR for the program on our view of the safety and quality of the program here at KSC.

We also have in my organization another function which is the procurement quality. That is governed by the Federal Acquisition Requirements and the NASA FAR supplement. The vast majority of the people that are working in the procurement quality group do Shuttle procurement quality. They go to vendor sites. They manage the DCMA delegations to those sites, in addition to being there themselves.

ADM. GEHMAN: Okay. Let me interrupt you, Bill. I'll admit to being a little confused. Is this a Center function you're talking about here, or is this a program function?

MR. HIGGINS: These functions are all in support of the program.

ADM. GEHMAN: They're in support of the program, but are they part of the program?

MR. HIGGINS: It is a delegated function from the program.

ADM. GEHMAN: From the program manager?

MR. HIGGINS: Yes, sir, there's a letter of delegation to Mr. Wetmore on safety and mission assurance and -- well, actually for his entire ground operations. There is also a delegation from Bill Harris, the TMR for safety and mission assurance, to Mr. Wetmore. That delegation basically flows directly through to me. It's a program delegated function that we're providing.

ADM. GEHMAN: What you got me confused here is this referencing of Kennedy processes rather than program processes.

MR. HIGGINS: The Kennedy Space Center is ISO-certified and our business practices include the use of documented procedures. So in order to keep a consistent process for the development of those products, our particular organization at Kennedy Space Center develops those procedures. They are reviewed and accepted to make sure they meet both the business practices at KSC and the program requirements; and then when they're approved, that is what we execute.

ADM. GEHMAN: Go ahead.

MR. HIGGINS: Okay. Our significant deliverables. I provide a signature not on necessarily the Certificate of Flight Readiness -- there are quite a few endorsements that are required in that. I actually sign two of them -- one for the ground operations. That is for Mike Wetmore. I support his signature to Certificate of Flight Readiness. And I also sign the safety and mission assurance readiness statement for the program. All of the centers sign -- all of the center S&MA people sign that one also. That Certificate of Flight Readiness signature is based upon program requirements, and what we are stating in that particular signature is that we have completed our required activities. There is a long list of activities that we are required to do through our delegation; and if we have completed those, then we can sign that delegation. We do not sign stating that the vehicle is ready to fly. We sign that we have completed our activities. That activity includes the hardware inspection that we have done. If the hardware has not passed all of the inspections or the inspections have not been dispositioned appropriately if they have not passed, we would not be able to sign that certificate.

The other deliverable that we have is an award fee evaluation. That is based upon our evaluation of USA's performance versus the contract requirements, and that is subjective and objective in nature. We have some metrics that we review. We also have objective looks at different programs and different things that USA does for safety and mission assurance and provide that input, like I said, to the program S&MA manager, the ground operations TMR, and also the logistics lead at KSC.

This is the organizational structure. This is my organization. I report to Mike Wetmore, as Mr. Bridges said. We have 107 authorized people in this division. The numbers are going to be a little different than what Mr. Bridges reported, basically because he's talking about the actual charges. We have people that are on military leave and leave without pay and other things, which drops the numbers a little bit; but the people, actually I'm authorized to have 107 people on the rolls. And right now I do have 107 people on the rolls, even though they're not all necessarily at work at their desk every single day.

The Mission Assurance Engineering Branch is headed up by Russ DeLoach. He has the safety and quality engineering functions, and we will get into a little bit about what they do. The Safety and Process Assurance Branch headed up with Ronnie Goodin has 15 people. That is ostensibly the safety specialists, the operational safety people who go out on the floor and monitor the operations of United Space Alliance to make sure that they follow the rules in terms of performing safely.

The Supplier Quality Branch is headed by Terry Smith. That is our procurement quality function. There are 13 people there. Several of those people are located around the United States and closer to the vendor sites, mostly for Shuttle vendors.

The Quality Assurance Branch has 63 people. It's far and away the largest branch. It's headed up by Bob Hammond; and that is the branch of our quality assurance specialists, the folks who go out on the floor, review the work being performed, and stamp the work paper attesting to the work properly being performed.

I have two charts here. Basically they show our program. This is basically our quality program. The system engineers are also in the same directorate we're in. We consider them a partner in terms of what we do in terms of quality. Basically it starts with them. They determine what's important about the systems that they are responsible for. They will modify the OMRSD, which is the Operations and Maintenance Requirements Support Document. If I have that wrong -- we talk in acronyms all the time now. They'll modify work authorization documents; and they provide the purpose, any rationale, and acceptance criteria associated with things that are important about the system which they need assured to be correct in order for us to fly safely.

That information is partnered with the quality engineering folks. We have four quality engineers and one quality engineering technician. Given that information, they are the ones, in conjunction with the systems engineers, they will modify the QPRD, the Quality Processing Requirements Document. That tells the contractor where to put the inspections in their work paper.

They'll determine the surveillance method. Inspection may not be the methodology utilized. They may use a sampling method in auditing. We may use a different type of assessment. In some cases where the risk is low, we may wait for a customer complaint, even though that's a very rare occurrence and as a matter of fact, hardly ever happens on a flight hardware piece of equipment. And they also do risk assessments associated with the decisions that are made in terms of how we're going to do our quality.

That information then, if there is an inspection performed, then that is done by the quality assurance specialists over here in this block. That's the mandatory inspection points. We have 62 quality assurance specialists who actually have stamps, who can stamp the paper and "buy the work" is the terminology. They can accept or reject the hardware and/or the procedure based upon the work authorization document and/or contract requirements. There may be specifications, other measurements and things that are in that work authorization document that the work has to meet. If it meets it, they accept it. If it is not met, they do not accept it; and unless it's accepted, we do not press on. Often when they have work that's not accepted, a problem report is generated on the hardware, the hardware is fixed, the problem report is dispositioned, and then the hardware can be accepted.

Two other things we can do in our quality program associated with the processing is that we do some hardware surveillance. It is done as available. As you might expect, that is done by our quality assurance specialists. However, their main priority is the mandatory inspection points. So they do not have the luxury of being able to meet random activities associated with hardware. They can only go and look at hardware when they are not being utilized for mandatory inspection points. So when they are out, we do not create a surveillance trending type of program, statistically based. It is merely a matter of going out and looking in areas where work is going on, looking for an improper hardware condition, and then they will initiate the resolution.

If they find something wrong, a PR will be generated. It's an additional set of eyes to go see some things not necessarily generated by a mandatory inspection. They look for improper hardware environment. Also it could be something from the hardware or it could be that people are working, for instance, without their certification cards demonstrating their training, that they have been trained to perform certain tasks.

Another thing that we have -- and this has been added in the last couple of years -- is what we call process surveillance. The process surveillance are audits, both scheduled audits that take place on probably, could be on a three-year basis or a one-year basis, depending upon the risks associated with the activity, assessments which are similar to the audits, generally not as broad. We do have some process surveillance that we use in surveillance of the processing of the main engines. And we'll do some -- the PRACA is the Problem Report And Corrective Action system data. We have that, and we review the metrics that the contractor generates to see if there is anything in there that would constitute for us a reason to take a look deeper.

One of the things here we'll see is you see a little note there's an e-mail concern to PHP management. What we have found is that if we're looking at these vast amounts of data that are being developed in PRACA or any other data source that you look at, things look alike pretty much all the time. It's the same types of things and the same types of systems. You know, we're going to see wiring scuffs. We're going to see corrosion, those types of things; and any processes that generally cause us problems, they generally are covered through our scheduled audits. So what we also do is if there's any type of concern that our quality assurance specialist sees on the floor and they don't understand why it's what it is -- or it could come from engineering or actually anybody else -- we will initiate an assessment and that assessment will look at the processes associated with it and the requirements of those processes and see if the contractor's in compliance.

So we don't wait to see trends necessarily. If we see a trend, it could kick off an assessment. But literally, we tell people if you feel that something is awry, we will authorize an assessment to go look at it. Since 2000, when we started this program, we have never told anyone, no, we will not take a look at that. We have told some people that it's already being looked into, but we haven't turned that down at all.

The other thing up here is the procurement quality, and they develop and manage the DCMA delegations. They do audits and risk assessments associated with vendor activities.

ADM. GEHMAN: Why don't you spell out what DCMA is.

MR. HIGGINS: Defense Contract Management Agency. It is the defense quality assurance function that we hire.

DR. LOGSDON: Can I ask you a question about this slide before we leave?

MR. HIGGINS: Yes, sir.

DR. LOGSDON: Over in the corner. Hardware Surveillance. How often do you find an improper hardware condition or improper environment?

MR. HIGGINS: It happens. I would not say it's a routine occurrence, but it does occur. We don't find a lot of improper hardware conditions in the mandatory inspections, and we have 8500 of those in a flow. There is not a large number of discrepancies found. Then when we go off and look on our own and generally -- and all of the critical activities are covered with the mandatory inspection points. These are less critical activities. However, we do find some things. It does happen. We have not found anything I would say is a show-stopper in the hardware surveillance.

DR. LOGSDON: No mission-critical kind of stuff that have showed up in this process.

MR. HIGGINS: We've found some things that have to be fixed. Okay. You know, we fix everything we find. Are there some things? Yeah, there have been some things that could have caused us some problems. Mission critical? Really critical? I wouldn't go that far, but they were important and, you know, we don't treat them trivially.

DR. LOGSDON: So your mandatory inspections are not 100 percent?

MR. HIGGINS: No, sir. There's 8500 points in a Shuttle flow that we will look. There are several hundred thousand actual steps that are worked in the processing.

DR. LOGSDON: So the surveillance is kind of your safety net?

MR. HIGGINS: Yeah. The United Space Alliance has responsibility for quality control, and they inspect considerably more work steps than we do. As a matter of fact, on all of the mandatory inspection points, United Space Alliance has already been there. Often it's done side by side, but it's never done without them. They've either done it first or they're doing it with us. And they're always responsible first. We are, for the mandatory inspection points, another set of eyes for those critical items that are deemed necessary to be looked at.

GEN. DEAL: If I can address that real quick. It's noted on there, it says "as available," which kind of throws out the meaning of "random" perhaps. When we go back to the Shuttle Independent Assessment Team report, they talked about the diving catches that they had to make on some different things and that's probably what that applies to. I guess the bottom line of my question is: Do you have enough people? Would you prefer to have more people so that you can accomplish more hardware surveillance?

MR. HIGGINS: Well, if you can convince Mr. Bridges to provide me with more people, I would be more than happy to accept them. However, basically what happens is that if you look at the flow of a vehicle -- and I'll get to the answer to your question, but I just have to meander a little bit. If you look at the flow of the vehicle, there are a lot of mandatory inspections toward the end of the flow. There are considerably fewer at the beginning of the flow. As a result, the workload of our quality assurance specialists ebbs and flows. There are times when literally our quality assurance specialists are not called upon to do mandatory inspection points. When that is happening, they can do that. There are other times, when it lines up just right in terms of vehicles and flow at the right times, that I don't have enough quality assurance specialists to meet the demand; and literally they shut down work and wait for us. It's a cueing theory problem. So the only way I could do that to the point that I wouldn't hold up work would be to have so many quality assurance specialists I would always have -- except for that rare occasion where everything lines up exactly right, I would always have idle people. They could do hardware surveillance. However, if we look at the concept of mandatory inspection, mandatory inspections are on the critical hardware. It's a risk-based inspection process that determined that that was the appropriate time and place for them to do it.

The other hardware surveillance that we're doing is being done on things that have been deemed to be less critical. They are certainly still important, but they're less critical. So it's a matter of how much resource would you like to put into the activity. We like to put in all that we can, is basically what we do. There are times when we have quite a few people that can do hardware surveillance, and there are times that we don't have enough for mandatory inspections. Does that answer your question?

GEN. DEAL: Sure. And I'll follow up later.

ADM. GEHMAN: Can you go back one more time here?

MR. HIGGINS: Sure.

ADM. GEHMAN: We heard from the chairman of the Shuttle Independent Assessment Team panel, Harry McDonald, who indicated in the assessment that they did, particularly of some main engine failures and things like that, that the PRACA data was not useful. You couldn't go back and trace a problem and you couldn't research into the data. It was hit or miss. It wasn't continuous. If I understand his testimony correctly, there were cases where things that were problems for a couple of years ceased being problems and you weren't able to do an audit to see who said that that's not a problem anymore. It just stopped being a problem. You don't own the PRACA process, but you use it. I would like to know what your experience is.

MR. HIGGINS: Well, my experience with PRACA is that each item, each problem that is found and documented on PRACA is a complete and total story in and of itself. It is dispositioned. It is either fixed or there is a reason why it cannot be fixed or it is basically brought back to print or there is a significant explanation.

ADM. GEHMAN: Or it's waived.

MR. HIGGINS: Yes, it could be waived.

ADM. GEHMAN: But in any case, there should be an audit trail.

MR. HIGGINS: For that particular problem, yes. I don't have any knowledge that any particular problem does not have all of its information for any particular problem. Now, if you were to pile all those problems together, I can't tell you specifically if there is anything that's done to integrate that activity; but I do know that each problem is handled completely. I'm not familiar with the problems that Mr. McDonald saw. That was before I was a part of this process.

MR. WALLACE: If I can follow-up on Admiral Gehman's question. In the PRACA data, are there levels of severity or levels of urgency? Is an in-flight anomaly going to be in the PRACA data along with a whole bunch of other things?

MR. HIGGINS: Well, that's a difficult question to answer because it turns out that there are quite a few different instruments that document problems and, depending upon what you find when you find it and things like that, an in-flight anomaly could result in a PRACA being generated if it was determined that there was a hardware issue that PRACA was appropriate for. Some in-flight anomalies can be dispositioned without PRACA being generated. They work together; however, they're not necessarily one to one for that. But in-flight anomalies, they're handled similarly at the program level such that in-flight anomalies, when they're identified, they are dispositioned by the program with either corrective action or waived, if there's a requirement violation, or in some cases they could be unexplained, but the risk was deemed to be minimal or nonexistent and therefore accepted.

MR. WALLACE: Does your organization, then, have a role in that disposition process?

MR. HIGGINS: We have the role in terms of IFAs or anything else that is deemed to have been originated with ground operations. If that's the case, then we will be involved in the disposition of that IFA, yes, as a participant in the board.

MR. WALLACE: The PRACA data base, is it somehow supposed to systematically feed into the FRR process?

MR. HIGGINS: That's a level of detail I'm not real familiar with. I can tell you what we do with it is that we review the process that generates the PRACA and we do sample records in the PRACA data base to assure that it is being done properly. As far as PRACA data automatically feeding into the Flight Readiness Review or the Launch Readiness Review, not as a data set but as an individual problem that was developed, if it's not dispositioned properly or is not dispositioned at the time of the LRR, it is discussed and could hold up the flight.

MR. WALLACE: Does PRACA, in a sense, become -- or is there another place where we sort of list questions or issues that need to be resolved prior to the next launch?

MR. HIGGINS: I don't want to be speaking as an expert on this because it's not really our function. We do participate in this, but it is the program function. And basically at the LRR and the FRR, what we do go through are all the in-flight anomalies and the closure of those anomalies that happened with the last flight and also for the last flight of this particular vehicle.

MR. WALLACE: So the LRR and the FRR, you go through a closure, you said, of in-flight anomalies. Does that mean, then, that something is identified as an in-flight anomaly will get specifically addressed perhaps more systematically than whatever else might be in the PRACA data?

MR. HIGGINS: I wouldn't say more systematically. I would say they follow a very similar process. It's just a different group of people responsible for that disposition.

DR. OSHEROFF: Well, I would like to get a bit more specific, if I could, because I frankly don't see where some of the kinds of problems that have been appearing are covered by the sorts of inspections and certifications that your people have been doing. An example is the shedding of foam, because it doesn't occur before launch and, in fact, I would guess that you would be hard pressed to find much evidence for anything wrong before launch in the first place. Could you tell me what the history is, as far as you're concerned, regarding foam shedding?

MR. HIGGINS: As far as we're concerned, Mr. Bridges talked about our involvement in the foam. I really can't speak any further on that. Ostensibly, the tank comes to KSC relatively ready. We do have to mate it. There is some foam repair that's done. There is some open work on that when it is turned over to ground processing.

Prior to ground processing, the program handles the tank through their Marshall program element; and they have safety and mission assurance functions with that. That's just not part of our contribution to the program. The Marshall Space Flight Center provides that safety and mission assurance function; and the disposition of the foam shedding, the risk assessment associated with foam shedding, the effect it has to the program is something that's worked between the program element at Marshall and their safety and mission assurance with the program itself. The Kennedy Space Center and our safety and mission assurance is not a player in that. If we identify defective foam, we identify that it needs repair, then it will be repaired; but the overall history of that is the element program at Marshall.

DR. OSHEROFF: So I would conclude that you had rather little to do with the issue of foam shedding in any way?

MR. HIGGINS: Yes, sir. If what we find through our activities is that the foam is meeting the specification as we look at it, then we press on.

DR. OSHEROFF: There was, of course, a launch video taken of STS 107, which showed a large piece of foam coming off; and presumably that was identified by people here at Kennedy Space Center. What happened after that?

MR. HIGGINS: My understanding is that the information is passed on to the Johnson Space Center because it would affect the Orbiter. Then it's up to the Orbiter element in the program to determine the risk associated with that event. The Kennedy Space Center -- and I'm not a part of that video review -- it's my understanding that the Kennedy Space Center has all that video and does the first review and then sends it to the Johnson Space Center for further analysis. I'm not familiar with the level and depth of the analysis that we do at Kennedy Space Center in total.

ADM. GEHMAN: Before we leave this -- eventually you will get off this viewgraph, I suspect. I don't see the words "probabilistic risk assessment" up there anyplace. Am I in the wrong church here or something, or is it just draftsmanship?

MR. HIGGINS: Probabilistic risk assessment has not been used much at the Kennedy Space Center in terms of ground processing. The reason that we have not really gotten involved in that too much is that, from a processing standpoint, I consider every activity to be basically binary. Either it passes or it fails, and 100 percent must pass or we don't fly. So when you get into probabilistic risk assessments and things like that, what's the probability of this thing not working or what's the probability of something failing, well, it is our premise that we have checked everything and every single thing that we have checked is ready to go. If it was not ready to go, we stopped until it was. I think that's the nature of the launch business where you basically can't come back. It has to work that time. There's plenty of redundancy built in. All those critical redundant systems are checked also. They all have to be performing, and it's either all or nothing in our launch decision.

So probabilistic risk assessments are generally not utilized in the ground processing. There have been some attempts to look at probabilistic risk assessment in terms of some activities. For instance, scrub turnaround and what is the probabilistic risk associated with scrubbing a flight, turning it around, and getting it ready for the next flight in the next day or two. There are some risks associated with that. You have to de-tank, tank it back up, and then the risks to the people.

We approached that and there was some question as to once you had that information, what then would you do with it. Obviously you're not going to fly if you're not ready, but you're going to have to turn it around if you're going to have to get it ready again. So it was difficult for to us find the appropriate place to put probabilistic risk assessment into the ground processing. It has been and is being used considerably with the vehicle systems; but the ground processing and the ground hardware, we have not found a significant utilization of it.

MR. HUBBARD: Just a quick follow-up to something you said a minute ago. The tank and presumably all the other hardware that arrives here that is someone else's programmatic responsibility -- and we'll pick the external tanks since that's been the subject of a lot of discussion -- do any of those S&MA inspectors that were part of the fabrication come along with it and look at it here or they ship it to you and you take a visual inspection and say whether it's good to go or not?

MR. HIGGINS: There is work that's done here at the Kennedy Space Center that is not under the auspices of ground operations, associated with other elements; and they have inspections associated with that. In some cases they'll use DCMA to perform inspections. I'm not familiar with them bringing people from Michoud, for instance, to come with the hardware.

MR. HUBBARD: So, in general, the external tank, for example, again, would be shipped here, arrive here, and you start processing it and unless you see something that is obviously out of spec, it just goes through the flow?

MR. HIGGINS: Well, there could be open work.

MR. HUBBARD: Final closeouts.

MR. HIGGINS: Yes. There could be some work that needs to be done on it that wasn't done at its origin, that basically followed it -- you know, there was open paper, there was work to be done, it was decided that a better place to do that work would be at the Kennedy Space Center. So we will perform that work -- and I shouldn't say "we." United Space Alliance would perform that work here at the Kennedy Space Center. But that particular activity, until it's turned over to ground processing for the stacking and that bit of repair that needs to be done, is under the auspices of the program at Marshall.

DR. LOGSDON: If I could ask just a detail. SSMEs and external tanks are not part of SFOC. They're separate contracts, I believe. When they get here, when the engines get here, when the tank gets here, are they integrated by USA people or somebody else?

MR. HIGGINS: Yes, they are integrated as a vehicle by the United Space Alliance and ground operations. That's a major function of ground operations is to prepare those items for integration and launch, and I believe it's a direct contract from Marshall for the tank. It's a direct contract to Lockheed for the tank.

DR. LOGSDON: But it's not Lockheed people that integrate, that make the tank; it's USA people here?

MR. HIGGINS: USA does the mating of all the elements in the stack, yes. That's correct.

ADM. GEHMAN: Why don't you proceed.

MR. HIGGINS: Sure. One last slide is basically the safety function that we have. We have safety engineering, we have an integration function, and we have safety specialists. The engineers manage their safety requirements, and the program does not prescribe safety operational requirements. That's up to the center to do that and we manage the requirements associated with how -- rules associated with processing to keep our facilities safe, people safe, those types of things. We will do risk assessments and reviews on anything that's deemed to increased risk in terms of hazards.

The Kennedy Space Center, the ground operations portion of the program is responsible for ground support equipment; and so we will develop and provide ground support equipment. That equipment has to be analyzed for safety, single failure points, the failure modes and effects analysis. That's done here at Kennedy by United Space Alliance, and we assure that those analyses are done properly and that the risks are properly accepted by the program. We're part of that process.

The S&MA integration basically is responsible for the development of those two major products, the COFR signature and the award fee, among other supporting products. And then the safety specialists are our eyes and ears on the floor associated with watching the operations of the contractor and assuring that they're following the rules. There used to be quite a few more of these people; and they used to be almost black-hat policemen type. We have only five safety specialists. We monitor specific hazardous operations and make sure that they're following the rules during those particularly hazardous operations, and they provide a significant amount of the launch and landing support on the runway, that type of thing for launch and landing.

That's the brief overview that I have for the program itself.

ADM. GEHMAN: Okay. Let me ask a question, going back to something you said earlier. I hate to be dense about this. You've attempted to tell me this twice already. Maybe the third time it will work. I'm still confused about who you work for and what your organization does, because what confused me was the answer to a question that you gave to one of the other board members when you said if General Bridges wanted to give you some more people, you would know how to put them to work. I thought this was a Shuttle program. I thought this was Shuttle program and Shuttle funded, in which case you should have said: "If Mr. Dittemore wants to give me some more people." Or have I got it wrong?

MR. HIGGINS: Well, you've got me on this one. It's really Mr. Bridges gets the work force complement from the agency; and it's divvied up based upon customer requirements. So if Mr. Dittemore requests more people for the Shuttle program, if additional funding is provided, then Mr. Bridges can hire more people and he can send them over to us.

ADM. GEHMAN: Or the other way around, I assume.

MR. HIGGINS: Or the other way around, yes, sir.

ADM. GEHMAN: Yes.

MR. HIGGINS: I'm not sure how it worked the other way where Mr. Bridges would decide I need more people, then just ostensibly charge Mr. Dittemore for them, you know, adding to that. I don't know how that would work. But Mr. Bridges basically is, I believe, the official associated with the overall head count at the Kennedy Space Center. There are customer requirements and requests that come in that can do that, but I believe that we're not under specifically a full cost accounting type of accounting for all of your people. So I'm not exactly sure the entire mechanism for getting people. But it's not so simple as, Mr. Dittemore, if you have a few extra dollars, we can get it. It's a complicated process, the civil service to do that. Because

I'm not an expert on personnel but it does seem complicated to increase the head count associated with a Center with the projects and programs that are going on. It's just not a simple correlation.

ADM. GEHMAN: All right. Let me thank you for that. Let me try this again then. Who is your direct reporting senior?

MR. HIGGINS: I report to Mike Wetmore, the director of Shuttle processing.

ADM. GEHMAN: And he reports to Mr. Dittemore.

MR. HIGGINS: He reports to -- his supervisor is Mr. Bridges. He is delegated technical management responsibility for the Shuttle program.

ADM. GEHMAN: Technical. Right.

MR. HIGGINS: From the Shuttle program and Mr. Dittemore.

ADM. GEHMAN: All right. Thank you.

GEN. DEAL: I've got a few for you. You touched a little bit on the metrics that you do. Could you kind of give me an idea of what kind of metrics you review and then, more importantly, what levels are exposed to those metrics, all the way from the technicians up to management in Washington, D.C.?

MR. HIGGINS: Well, that's a broad question. I'll do the best I can. We review the metrics associated with institutional safety type of things first. Those come in routinely, how many injuries they're having and things like that. Some of the other metrics that are reviewed have to do with problem reports. Those are normalized to see if we're getting an increase in number of problem reports, PRACA generated for a particular flow. We'll look at work force maximum work time deviations where people work longer than 12 hours a day, 16 hours a day, longer than so many hours per week, per month. Those types of metrics are reviewed to see if we're stressing the work force. And we work those with the contractor in terms of if we see anything that appears to be a problem or could cause us some problems in the future, we'll talk to them about what they're doing about it.

United Space Alliance is quite a forward-leaning safety activity. They watch those metrics at a much lower level. They get right down into the units, the working units, and have generally quicker information than we do; and so we see it at a higher level. When we talk to them about it, they're generally already dealing with it at the lower levels where the specific supervisor and group of people is having some difficulties.

Some of the major metrics like lost time injuries and things are shared with headquarters. There's a report put out by the Safety and Health Independent Assessment Office that has a compilation of those and other metrics across the center that are sent to headquarters. Our review has to do with looking at them to see if we have any problems that we need to work with them prior to -- well, during a flow, if we see any problems, whether or not we need to initiate any assessments on our own. I'm not familiar with discussions or any other activity that would take place at another level. For instance, with the metrics that are sent to headquarters, I'm not familiar with the discussions that they have up there based on those metrics.

GEN. DEAL: Do you have a level of comfort that a technician, for example, working on the external tank would know about a safety concern on an SRM? I mean, is it that level of cross-tail?

MR. HIGGINS: I personally can't be certain of that. I'm not that familiar with that level of communication on the floor. My general knowledge of what goes on out there is that they do have knowledge of what they are responsible for. The contractor does go to a considerable effort to communicate information, and some of the people do move around somewhat. That kind of specific problem associated with that, if it was relatively major, we all know about it and the technicians would have it. I would say that, in general, though, that the information is available to the technician. How it's specifically handed to them, I think it varies, depending on the severity of the problem; and if it's a severe problem, we'd all know about it. And if it was minor, it might just be available.

GEN. DEAL: One more follow-up on the GMIPs. It's kind of history of how we got to where we are. You talked about hundreds of thousands of steps, but the number of GMIPs has been decreased by about a third, down to 8500. Do you know of any examples of when we have increased the GMIPs and, if we did have more QASs, would we have more GMIPs?

MR. HIGGINS: Well, we have increased inspection points on wiring, for instance. We have recognized that certain aspects of wiring, the only way you can check it is by putting eyes to it. That has increased. If we had more quality assurance inspectors, I suppose that we would be more liberal in our look at what types of things would be inspected. I think I would characterize it differently, however, as that I would like to see more in terms of fine-tuning of the inspections that are done. I think you can go out to any quality assurance specialist that works in our division and they would legitimately tell you that they look at things that don't mean very much and they watch people do work that they're not invited to watch that they think is important. And I think every single one of them could give you an instance of that. I don't believe that we have, you know, thousands and thousands of instances like that. I think there are several. And I think if we could spend the time and energy honing in on those that need to be done and eliminating those that don't add much value, I think there will be a change in the number of inspection points. And I don't really believe it would be a significant jump in either direction.

The process that we used to come up with the 8500 was pretty rigorous and risk-based, based upon the criticality of the hardware and our ability to check it further in the flow. So they were basically limited to that, as close as we could get to the very end, the very last point, to make sure that what we were stamping off was good hardware, not some intermediate point for the NASA MIPs. So I think some fine-tuning would help. And if we did fine-tune it, we might need more quality assurance specialists. If we fine-tuned it, we might need a few less. I would guess we would probably need a few more, as opposed to the other way around.

MR. HUBBARD: I would like to talk a little bit about the review process, just pick on the Flight Readiness Review as an example but there are always a lot of others. I would like to have you talk a little bit about how minority viewpoints get surfaced, both on the government side as well as on the contractor side, and how are they disposed.

MR. HIGGINS: Well, the minority viewpoints are put on the table. I have not seen them suppressed. All of the information goes on. It is discussed. The right people review this -- the system engineers, the design centers. It's a pretty thorough process of a review.

When you get to the FRR, the expert people who do that have all had hours and hours and hours of discussions; and the FRR is generally, from my perspective as almost a spectator, it's basically a summary of all of that activity. The minority viewpoints that were discussed are generally, as far as I can tell, are put out for everybody to see and that they are told in that summary what was decided as a result of that opinion. Generally, what I have seen is that there is a significant amount of analysis, and there's some work involved in putting away every single concern.

I think there are cases -- the BSTRA balls were one -- where someone, after all was over, said, "I'm still not comfortable. We can fly, but I'm not comfortable. I would prefer to see every single BSTRA ball." Okay. We heard that at the FRR. There was an awful lot of analysis on the table associated with what to do about the characteristic of the cracked BSTRA balls, the probability of BSTRA balls being cracked, what would be the failure modes. All that information was out there. So I don't see any suppression. I don't see any suppression, and I see a lot of conversation and discussion going on.

MR. HUBBARD: Now, how much insight do you have into the tiers of reviews that go on, leading up to the FRR? As someone who has signed a Certification of Flight Readiness, I know it's a big deal and there's a ton of reviews that lead up to that point. Within the contractor side of the house in which there are thousands of people and dozens of reviews getting up to the point where everybody decides that we can go fly, in those USA reviews or other contractor reviews, do you have somebody there; or are those done through the performance-based process and just brought forward to you as the government oversight?

MR. HIGGINS: I will try to answer that through the LRR process, which is the Kennedy Launch Readiness Review prior to the Flight Readiness Review that's done by the program. For the Launch Readiness Review, all of the different activities that need to sign that endorsement provide their status of their activities that led them to be able to sign; or they have to stand up and say why they can't sign if there's something that's wrong that has not been taken care of.

That being said, we are all pretty much focused on taking care of our activities to make sure that we can sign that COFR. We also do some insight into other people's signature of that COFR from an S&MA standpoint. For instance, the operations people have a pre-LRR review of metrics with USA; and we participate in that. We go to make sure that they do it and see how it's done. The engineering group will have a pre-Launch Readiness Review associated with their surveillance that they've done, the in-depth observations; and the in-depth observations that they were scheduled to perform and they didn't perform, they must provide rationale as to why they didn't perform and it and then why it's okay. You know, what did they do instead? It's not like, well, we missed it; forget about it. It's well, we missed it and so what we did is that we went back and reviewed all the test results, we checked the inspection record -- they have to go back and do something other than watch the activity. So we make sure that they have gone through that process. So we participate in some of the other groups that have to sign also.

We also participate in United Space Alliance's, their pre-readiness review activities for safety and mission assurance both for their Kennedy part and then we participate in the United Space Alliance safety quality and mission assurance. They call it SQ&MA. They have a program-wide review for USA, and we participate in that to get insight into whatever we can see for the entire program.

MR. HUBBARD: So at the lower-tier reviews, it is more of a -- spot-checking is not quite the right word – it's selected participation as opposed to an across-the-board function?

MR. HIGGINS: Yeah, it's selected participation. You know, we concentrate on meeting our responsibilities first, and obviously we spend the vast majority of our efforts making sure that we have met our responsibilities. But we do spend time and energy participating with others who have to sign also to assure that the product they're providing is reasonable. Yes, we do support them.

MR. WALLACE: I'd like to shift to your role, your input into the SFOC award fee, the contract award fee. We have been told there are these various safety thresholds that, if you go below, you won't get an award fee, or if you go below a lower one, you won't get any fee at all. It seems it presents a dilemma in terms of you want full safety reporting, IFAs or whatever else. Are metrics like IFAs something that are a part of that award fee determination?

MR. HIGGINS: I can't speak for the final scoring of the award fee associated with safety or quality. I have not participated in that other than provide input. That is done by the program in Houston and in Washington. I provide our objective and subjective input to that. It's possible that we could provide input on an in-flight anomaly that we thought USA handled improperly that would be a negative award fee hit. We could also provide input to in-flight anomalies that they handled well that we said would be a positive award fee input.

MR. WALLACE: There's never a perfect way to write a contract, but do you see a dilemma there, where there's some perhaps incentive to under-report safety issues?

MR. HIGGINS: This has been something that's been discussed and worked over many, many, many years; and if I can digress for just a second, if you go back quite a few years, you would have a culture at the Kennedy Space Center, if you were to ask the question, "Who's responsible for safety," they would point their finger over to the guy with the green hat that said "Safety" on it and say that's the person responsible for safety. In those days when they reported things, then you would have that kind of culture where people actually under-reported because they weren't responsible for safety, and so it was just a different culture associated with this. Today if you were to ask somebody, "Who's responsible for safety," the answer would be, "I am," from every single person out there. I think you would be hard pressed to find somebody who would give you an answer other than that.

Under that culture, reporting is rewarded to a certain aspect. There are rewards given to people who report things that turn into significant fixes or significant improvements, and never do we punish anyone for reporting anymore. That change in culture of "I'm responsible for safety" has permeated itself throughout the entire work force and the management structure and how we deal with reporting.

While we can deal with individual events and talk about whether or not that particular event was preventible, should it have been preventible, did you do a good job before the fact and after the fact -- those types of things can be dealt with on a positive and negative basis, but we do not treat the reporting of those activities as anything but positive. As a matter of fact, Mr. Bridges, well, at the quarterly that he mentions, if the numbers of your close calls is going up, Mr. Bridges notes that as a good thing. We want a reporting culture, and we encourage a reporting culture.

MR. WALLACE: We've been told also that, as opposed to award provisions, penalty provisions, severe penalties for loss of vehicle, that those penalties are dependent upon sort of a fault finding. Is there a different standard at work there?

MR. HIGGINS: I'm familiar with the clause in the contract that has that. I'm not familiar with the philosophy that is utilized to come up with that. I believe that there is some -- if there is an incentive out there to cut costs, I think the logic has to go in hand with that that there must be some incentive to remain safe. Given that as a quick base is that those types of penalty clauses were put in probably to achieve that balance in some form. You have to balance the incentive for saving money with the balance for being safe.

ADM. GEHMAN: Okay. I'll ask the last couple of questions, and then we're going to have to move on here. In a number of reviews, published reviews of NASA events -- for example, the Rogers Commission that looked at the Challenger explosion and this Harry McDonald study, the SIAT that we mentioned before -- there are editorial comments in there about a series of events that are attempting to send messages -- remember the famous O-ring seal leaks -- they had been leaking for many, many flights before the Challenger disaster -- and that the system either doesn't recognize or can't hear those messages. You have to stand back from the shop floor. Just like you said, there's 100,000 processes and 8500 check points; and you said it's a binary problem. If every one of those is done right, the thing will fly right. Not necessarily. That's what these reports tell us. So my question is: Where in the organization should we look for that group of people who are standing a mile back from the Shuttle and are not looking at it through a 10-power magnifying glass to find whether or not foam-shedding is a message that's being sent or something else, nuts and bolts falling off? I don't know what it is; but whether or not there are messages being sent to us, where is that organization and where is that place in the food chain that we should be looking for that?

MR. HIGGINS: That's a very good question. I'm not sure I can answer that specifically because that would be program and agency functions that are above me.

ADM. GEHMAN: That's a fair answer.

MR. HIGGINS: I can tell you my impression of where those types of things are is that there is activity that I can recognize that performs some of those functions in Shuttle integration that takes place in Houston. There is some launch integration that takes operation at KSC. Flight integration that takes place at JSC that steps back a little bit from each individual element and looks at the whole, so to speak. There is also the independent assessment function that is performed by Code Q through center activities that is supposed to take a step back and take a look at some of those things and provide information.

ADM. GEHMAN: Thank you. That's a fair answer.

Another question. Take the BSTRA ball example as a case in point. Is your organization manned with sufficient people and money to independently review an engineering solution or an engineering analysis or, let's say, a disposition or a waiver which the program wants to grant? Are you manned with sufficient people and sufficient money to get outside experts to do a risk assessment in order that you can go to some of these councils? What I'm saying is when you go to one of these review boards, the engineers will come up with, you know, all this much data and say, "Okay, we're going to waive that problem because we've certified that this is okay." Do you also come in with 18 inches of studies and analysis and say, "Not so fast"?

MR. HIGGINS: No, that is not the responsibility that we have. Not to say that that responsibility doesn't exist in the program. It does. It's just not mine. That belongs to the assurance functions that are associated with the element and the element program. For instance, the BSTRA balls would be the Orbiter and the Orbiter safety and mission assurance function is at Johnson Space Center.

ADM. GEHMAN: Actually I kind of misled you a little bit on that issue. I was referring to something that is in the Shuttle processing universe. You're right, I should restrict my question to something that's in the Shuttle processing universe.

Let me give you another hypothetical then -- and I know how dangerous hypotheticals are. One of the questions I asked earlier to Mr. Bridges about aging aircraft. Aging aircraft has safety ramifications, and the effects of aging aircraft are very hard to detect. They're very subtle. So let's restrict ourselves to the universe of the processing facility. Are you managed and equipped to come in with independent studies to suggest whether or not all of these safety ramifications are being addressed or not?

MR. HIGGINS: The answer to that is probably yes and no. If you're looking for a large-scale, massive effort, immediately the answer is no. If the question is could I get that, the answer is yes. I do believe I could go both to the program, to the agency, through the Center. We have other organizations at the Center that have that. I do not normally have a funding line to go off and go purchase that type of activity. I would have to go request that and get it separately. I don't have any support service contractors that I could just add a task onto and say, okay, provide some experts and let's go off and do something. It could be done; it's just not routinely done for us.

ADM. GEHMAN: Okay. Now, I'm going to go down one more level and ask the same question. My understanding of how the system works is that there are certain things, there are certain kinds of repairs which are called standard repairs, and then there are others which require an engineering process -- and I don't remember what the name of the engineering process is. Does your team up here have sufficient manpower and ability to conduct independent analysis to second-guess or to challenge the engineering department from calling something a standard repair?

MR. HIGGINS: I think in some areas, yes, we have. It's not so much the staffing as so much the expertise. If you look at the organizational chart and you're looking at the engineering numbers, they're pretty small. So the engineering numbers being what they are, there are areas that our people have experience and knowledge in and can challenge quite reasonably well. There are other areas that we're just not the experts. With the six, eight engineers that I have, I just can't possibly have that many areas of expertise. Where we have a lot of expertise in, for instance, lifting devices and we have significant expertise in lightning and we participate quite heavily in activities associated with that. When it comes to some of the other areas, we're not going to be as readily available to provide analysis. We would have to go off and get some funding and get some outside experts to come help us. We could use our Safety, Health and Independent Assessment organization to help us procure that activity. We might even request them to perform it, and they would go get it. That's another possibility. It can be done; it's just not something that I have a significant amount of in-house expertise on.

ADM. GEHMAN: Thank you very much for your patience with us this afternoon, Mr. Higgins. I know we've asked you a whole lot of what seems like pretty basic questions, but it's very helpful for us to get to the bottom of this. I certainly was struck by some of your introductory viewgraphs there where you indicated how important you and your people take this mission and how key safety and mission assurance is to the safety of the program. I certainly salute that and recognize it.

I will also tell you that as a group, every time we have gone to the Shuttle processing facility, the logistics center, anyplace, we've all been impressed by how safety seems to be on everybody's mind. So thank you very much. Thank you for your help this afternoon.

We're going to go right on to the next witness, Mr. Al Casey, if he's here, and jump right in.

General Casey, thank you very much for helping us this afternoon. We appreciate it very much. As I did with the other witnesses, I will just ask you, before we begin, to affirm that the information you provide to the Board today will be accurate and complete, to the best of your current knowledge and belief.

GEN. CASEY: I do so affirm.

ADM. GEHMAN: All right. Thank you very much. Would you tell us a little about your background and your area of expertise, please.

ALOYSIUS CASEY testified as follows:

GEN. CASEY: Yes. I had served 34 years in the Air Force, worked in several aircraft development programs; but of interest to your work here, I had three tours in the development of Minuteman 2, Minuteman 3, and MX or Peacekeeper missile. My last tour was as commander of the Space Division in the Air Force. I have 15 years consulting since then and have done a lot of work on a lot of different programs, looking at system engineering relative to both aircraft and missile systems.

One other thing, in my consulting work I was also on the board as an outside director and chairman of the board of NTS, a national testing organization for test specifications, mostly in aerospace hardware. I'm no longer with that. I'm an independent consultant. Today I have retired from there.

I have put my thoughts -- are we ready --

ADM. GEHMAN: Go ahead.

GEN. CASEY: I have put my thoughts into a short series, eight total, slides. It turns out that the – I'd like to point out the upper left-hand corner, it shows an X. I had there a little cartoon of an MX, just to make sure that everybody knew that I was not masquerading as an expert in manned space systems. My expertise is all in expendable launch vehicles and ICBMs.

To talk about Shuttle reliability, it is demonstrated at .984 -- that is, two failures in something a little bit over 100 flights. .984 is a factor of 2 better than most of our unmanned launch vehicles. On average, they've been about .95, or five failures for a hundred. That's pretty good. That Shuttle record is really pretty good, considering that it is at risk both in the ascent and in the re-entry, which we don't have to worry about on an ELV, an expendable launch vehicle.

I believe that very high reliability is achieved by two things: redundancy and margins. Now, if everything is perfect in the assembly and tests, you still depend upon redundancy and margins for things that can happen in flight. And there are things that can happen relative to either the environment being more stressful than you thought it was going to be or the hardware not quite being up to the capability you thought was in it.

In the case of the Shuttle, the redundancy has already been built in where it's practical, I believe. The margins have already been built in and designed in, and they are verified in qualification tests. I'd like to talk a little bit more about that in the later viewgraphs.

I did say that .984 is pretty good; but, in fact, I don't think it's good enough for optional human flight operations.

Now, if you talk about what do you do about margins, the redesign of subsystems I really don't think is very practical for the Shuttle fleet. Extensive analysis has already been done on vulnerabilities, and changes have been made and continue to be made where they're affordable. It's impossible, in my mind, for a system as large and as complex as is the Shuttle to identify with any certainty the next most probable failure mode. So if you go around just trying to redesign the subsystems from today's baseline, you may very well spend a lot of money on things that are not really the next most probable failure cause.

I believe that redesign with greater margin is only practical for the whole system for the long term where, in fact, if you were trying to replace the Shuttle, you would probably look at having criteria that would say you would have improved safety, reliability, and affordability and drive that in an organized system engineering concept for a replacement system.

Of course, there are other things that affect the way that a Shuttle will fly -- the assembly, the tests, and the operational controls. Relative to assembly, I believe that NASA has repeatedly demonstrated effective assembly techniques, despite the fact that they have a very difficult job with a fairly complex system, both flight system and ground system. And it's pretty hard to improve on the record they have there. You've heard a lot about that today. There's a lot of detail in that and yet we have had, as best I can tell, over 125 flights and I don't know that any failures can be attributable to the actual handling and processing.

However, now the next point I point out that I believe the *Challenger* failure was a case where there was operation beyond the qualification of the seal. In other words, the margin was negative in the environment that that particular rocket was subjected to at the time.

ADM. GEHMAN: Excuse me for interrupting, General Casey. While we're on that point, I want to make sure I understand. From my understanding, I would agree with you. In hindsight, the margin was negative. The question is: Can you determine that ahead of time?

GEN. CASEY: Well, I believe so. I believe that the qualification of the rocket motor was never subjected to the extended low-temperature condition; and, in fact, the only way you know that you have a margin is to stress the thing beyond what it's going to see in flight. That has to do with vibration, acoustics, temperatures, pressures, whatever you're going to see. I don't think you learn anything about margins from repeated flight. Where you learn about margins is in the qualification testing of the hardware.

ADM. GEHMAN: Essentially what you're saying is that occasionally you have to test to failure to find out what the margin is?

GEN. CASEY: No, I'm not saying you have to test to failure. I am saying you have to do qualification testing, which is stressing the article beyond the environment it's going to see in flight. Now, it may not be to failure; but I also believe that it's possible and, in fact, perhaps been demonstrated not only in the Shuttle but probably in some of our other systems where we operated the system where the margin was driven negative by the conditions that we operated in, as opposed to the margin you thought was there based on your testing because you violated the environment for which it was qualified.

In this particular case right now, you may find that the recurrence of a *Columbia*-type failure can be avoided by acceptance testing. I think you're pursuing that quite a bit in your discussions. What I show in the sub-bullet there is I'm thinking in terms about acceptance testing may preclude the debris coming down on the vehicle.

Let's go to the next one. Relative to this cause of failure – and again, I'm not going to act like I'm some expert in this particular failure, because I am not -- but I would make these observations. High-speed impacts of material on the Shuttle wings are beyond the qualification envelope of the Orbiter. The known debris from the tank hitting the left wing is incontrovertible. Regardless of the specific sequence and the details of the failure events, it seems to me that the remedy is to preclude debris from impacting critical systems during ascent or anytime they have to operate.

I believe that this was, doubtless, an original design requirement for the whole system, that you do not have debris impact down the vehicle systems in any kind of -- I should cut it there, that you do not impact down the vehicle's subsystems. However, I think this is a design requirement that was not achieved, demonstrably not achieved.

Now, what can you do to preclude debris impacts? One might consider looking at the amount of insulation that's on the tank. After all, it may be that the potential for having debris is reduced if you reduce the thickness of the insulation. I don't know whether that's true or not, but I would suggest that there ought to be lots of data now so you can rather precisely decide how thick that insulator must be for its functional use on the tank. And it may be that -- and this happens sometimes. It happened to us on some ELVs, expendable launch vehicles, where we had too much insulation on something and it caused another problem. In fact, we had a failure to separate in one case from the payload and the last upper stage just because it was over-insulated on the cable that was supposed to separate. So I think we ought to look carefully at that. And maybe there's nothing there. I don't know. NASA has an expert group of people to look at this sort of thing.

Clearly, I think that testing has to be developed to ensure that the integrity of the foam insulator and those pieces which are bonded on have, indeed, the integrity to stay on in flight if they're going to be in place at the launch. And, of course, that applies to any other debris that might come off the forward sections of the system.

I think it's absolutely critical that we retain the margins. A concerted effort needs to be made to operate within design margins. Again, I'm talking about margins that are verified in qual testing for each and every one of the subsystems. A series of successful flights does not verify a margin. You may be skating on the very edge, and you may come up to that flight where either the environment or the particular hardware causes you to go negative.

I made the point about rigorous qual testing. Special efforts should be made to preclude waivers or deviations in production or assembly or pre-flight checkout or any other kind of method that's used for accepting the things you've been talking about here today, that they do not, in fact, reduce the margin; and that's very difficult to decide. It requires expert system engineering judgment to look at that particular point, that how we buy this thing off does not, in fact, reduce our margin.

As you have pointed out here today, aging or repeated use may also erode the margin, unbeknownst to the operators of the system. Aging and surveillance programs have been used successfully in aircraft and in ICBMs to not only protect the margins in a given flight but also to predict the service life of the vehicle. It's important in the ICBMs because we build a rocket and we may want to use it 18 years later and want to have the same reliability or a good reliability. It's also important in this system and doubly important here because of the fact you do cyclically heating sequences both in ascent and descent in the repeated use items. I believe it's absolutely essential that a comprehensive system engineering effort is made to not only know what the margins are but be sure that we protect them in all ensuing operations.

The last observation is the next chart. In my view, it's important that we return to flight soon. Long delays incur loss of people and skills as well as the morale of the whole team; and all of the above may well reduce the reliability of the future flights, which is exactly what we're all interested in, increasing the reliability.

All reasonable steps to preclude debris impact is, in my judgment, the best approach to returning to flight. If we do all of that, I still believe that in the short-term -- I'm talking about lacking a full redesign as we talked about earlier -- protecting the reliability, in other words, trying to project that the reliability is better than .984 is very hard to guarantee. And it's my observation, therefore, that the crew size ought to be looked at as being a minimum and you should not use the Shuttle where an expendable launch vehicle or robotic system can do the job.

Those are the thoughts I had. I'm perfectly willing to answer questions if I can be of help.

ADM. GEHMAN: Thanks very much. Your comments are very helpful because in some of the readings we've all done as part of our review of some of these programs, that subject of successful flights don't re-establish margins has come back again and again. The other issue that's come up again and again is this question I asked before -- that is, as in the *Challenger* case where the leaking O-ring seals were trying to send us a message because they had been leaking many times before the *Challenger* disaster but yet they were sending messages but nobody was hearing it, the trick is to find those. And successful flights should not be used as evidence. They weren't evidence that the O-rings were working right, and they should not be used to indicate everything is okay here.

Our challenge is to receive those messages and do something about them. That's the tricky part, and I agree with you completely. Your presentation has made some of those things crystal clear.

GEN. CASEY: Sir, if I may on that point, I would say there really are two cases. There's one where you have the indicators and you have to act on them. That's true across the board. The other one is where, in fact, you're losing the margin and you don't have any indicators. Those are the really tough ones; but that's why I believe very strongly that it's very important that you keep a running system engineering accounting of what you think your margins are, because you can violate them, as I pointed out, either just by operating outside of your planned environment or by something squeezing through the acceptance testing, which doesn't give you the data that you're looking for.

ADM. GEHMAN: So when you use the term to qualify the system -- I think I understand what you mean by that --

if you take it in the case of the ET, for example, the external tank, if we were in agreement right now, we would agree that in its present situation that the ET is an unqualified system because it's shedding foam continuously. It wasn't designed to shed foam. We didn't design this thing to have the Shuttle Orbiter to be impacted by foam. Therefore it's currently not qualified in the sense that we're using in this room.

GEN. CASEY: Exactly. That's right. I believe that in your group that's looking at margins unless they, in fact, know that they're operating within the margins, there's no way, in my mind, that you can say I'm operating within margins if I have an unknown mass impacting the aerodynamic surface and it has unknown damage.

ADM. GEHMAN: We've heard that explained to us in other words; and I'll use those other words here to explain, just to see if you agree with this. That is, that what we should do is we should change the operative question on the table here. The present question is that you've got to prove to me that something is unsafe before I'll change it. What we need to do is we should require the system to prove it is safe. Particularly if we have something which appears to be exhibiting anomalies, the impetus should be to prove it's safe. The burden shouldn't be on me to prove what's not safe. The burden should be on the system to prove it is safe -- in other words, to qualify it.

GEN. CASEY: Yes, I would agree with that. Again, you point out these indicators you get. I think that obviously we have to give a lot of credence to any indicators you get; but I am equally as worried about those things which, in fact, are so subtle you haven't seen them yet but, in fact, the margin isn't there and you can lose it.

GEN. DEAL: General Casey, I'd like to springboard from something you said a while ago about the aging and surveillance programs. You mentioned about the expendable vehicles that we've had -- your Gemini, your Mercury, your Apollo, you've worked the Minuteman and MX. This aircraft or spacecraft was on its 28th flight, yet it was more than two decades old. So we've kind of entered this arena of an aging spacecraft in a research-and-development environment. What you didn't say in your biography is that you've got experience in the early days of the B1, which has been flying more than 20 years later, and the A10. So it would be interesting to know if there's any principles that you might apply to the Shuttle and specifically what type of aging and surveillance programs do you think NASA should pursue.

GEN. CASEY: Well, I agree with what Roy Bridges said, that it's quite a different thing for the aircraft than it is for the rockets where you don't have any ability to observe in repeated flight. More applicable, in my mind, is what the ICBM world does; and, in fact, they do detailed aging and surveillance on each and every piece and part. They don't say we want the Minuteman to go until year 2010; what they do is they look at all the detailed parts and see if, in fact, they expect the reliability of the entire system to be the same at that time, based on the test data and analysis that's done.

Now, NASA, it sounds like I heard Roy Bridges mention a lead-the-fleet kind of thing. That's what I have in mind, something where you take the oldest pieces you have, or subsystems, and you put them through the environments that they have seen in some kind of accelerated way you can, to get ahead of the curve, as I think Roy pointed out earlier, so that, in fact, you have some projection of whether or not you're losing margin or whether or not you have some reasonable idea of what the service life really is. I think it's a complex system of tests and analysis.

MR. WALLACE: General Casey, I'm from the world of civil aviation where, I guess, you might call that optional human flight, wherein we --

GEN. CASEY: You could fly those airplanes without people on them, too, if you like.

MR. WALLACE: Right, but it doesn't get the passenger from A to B. In 2000, we operated 11 million flights, 32,000 a day, without a single fatality. And operating on this level of reliability, we would lose 640 of those airplanes every day. I'm asking you to maybe expand a little on this. This is a big question that this board has been sort of gingerly walking around and you plunged right into it, about who should fly. So I see your last two lines there sort of give the short answer to your question. Could you go on perhaps and discuss what you think would be an acceptable risk for various types of operations, even assuming a next-generation spacecraft? Because it sounds to me like, from what you've said, that the order of magnitude of safety of the Shuttle is not going to change, given even with incremental system improvements.

GEN. CASEY: Well, I believe that new technology probably will allow us to go very close to 1.0 on reliability, but I think that's going to take a new system to do that, in my view. What I meant by the statement was that if, in fact, there's a mission where you can do the same thing with an ELV, don't use the Shuttle. I know there are certain missions that NASA has, very important missions, that require the Shuttle, there's nothing else that can do it, and I think that those obviously have to be done. I don't think you're saddled with .984. Maybe we won't have it that bad. What I'm trying to tell you is I don't think you can guarantee that it won't be .985 or .99 rather than 1.0 for the next ensuing hundred flights. That's what I'm trying to point out there.

MR. WALLACE: If I could just ask another question on return to flight, because you focused on fixing the falling foam issue. Do you have thoughts on other return to flight? I know NASA's working on things like on-Orbiter/on-Station inspection-and-repair capability.

GEN. CASEY: I believe that anything else you can do to enhance the safety of the mission -- I didn't try to explore all of those. There are a lot of things you can do. I was focusing my thoughts on how do you avoid catastrophic failures. I believe anything else you can do is good.

ADM. GEHMAN: Let me interrupt a second here. I thought you did address that. Maybe I misunderstood. What Mr. Wallace was talking about is not margin but redundancy. It seemed to me that these are two different things. In the case of redundancy, you have kind of given up on the system. In other words, you've said, okay, when this thing fails, at least I've got a backup -- whereas in the case of margin, you don't want the thing to fail in the first place.

GEN. CASEY: Well, there are certain things in this world, both in our more simple ELVs and clearly in the manned systems, where redundancy is used automatically. Like if a computer fails, we just automatically have another one to crank up; but you can't do that with a rocket motor nozzle or other things that are single-string failures. So that's the distinction I was drawing. Where redundancy can be done, I think NASA has already done it already on this vehicle.

DR. OSHEROFF: Well, actually both of the failures you talked about, of course, are failures which had not been anticipated that we were working outside the margin. What do you think should be changed about the way NASA has been assessing safety in the Shuttle program which will, if not guarantee, certainly greatly increase the probability that, in fact, issues like this will be detected before another Shuttle is lost?

GEN. CASEY: I don't claim to have explored the details of the program to know that. I'm saying from the top down you ought to demand that we know what the margin is; and that's a very complex set of things, as you know, because margin is expressed in different terms for virtually every subsystem in the whole lash-up. I think -- I don't know, maybe you've asked -- have you asked NASA what is the margin that they have across the board or by subsystem? I think that that's something that they ought to know, if they don't; and I think you want to work to protect that. That's my point.

ADM. GEHMAN: In your previous experiences in the other programs like Minuteman and things like that, did you have the issue of unknown unknowns? How do you go after unknown unknowns?

GEN. CASEY: Well, there are two things I don't have much use for in the system. One is the word "robust," and the other is "unknown unknowns." To me, they're both so vague, I don't get anything out of them. I believe that you need to use a very strict rule of qualification, and NASA probably has done so. I assume they have done so for the Shuttle. You know, 6 dB in all of these environments is what we have used as a specification for spacecraft; and that's a very rigorous way of looking at acoustic environments or vibration environments and all the other things that these systems have to be able to operate through. It gets more complex if you're talking about rocket motors and things like that. You have a hard time really stressing to a margin of 25 percent above what it's going to see in its actual function. But you can always in every one of these systems test them with more stress than they're going to have in flight in virtually every case. In some cases you cannot do the combined environments that you would like to do, but, in fact, you do the best you can. To the extent you make estimates of those, you can get an overall assessment of what the margin is of the vehicle; and you try to protect that. That's my view of the way to operate.

DR. LOGSDON: You've got reliability out to three figures there: .984. How real do you think that number is, since it's two failures out of 113 flights? Statistically is that good enough to give you a three-digit reliability number?

GEN. CASEY: No, my calculation of reliability is 2 over 125. I mean, all I did was just divide the failures by the number of flights.

DR. LOGSDON: I don't think there have been 125. 113.

GEN. CASEY: 113. Well, the answer to your question is I don't believe the reliability is static. In fact, when you go around and you make some of these changes NASA has, they made significant changes relative to the seal after the seal failed.

DR. LOGSDON: That was the *Challenger*.

GEN. CASEY: And I think the new seal has changed that reliability a bit. Now, how good it is? My point about not knowing what you can get to is that it's very difficult to know that you're at 1.0.

DR. LOGSDON: I understand that, but I'm not certain that .984 is a good number either.

GEN. CASEY: No. I'm not either.

DR. LOGSDON: But you're basing a lot of recommendations on that. I mean, I look at your latest bullet. It says if it's this, don't do -- you know, crew size at a minimum. I gather "crew size at a minimum" and "optional human flight" on your first slide mean about the same thing.

GEN. CASEY: No, I don't think so. If you don't need to use the Shuttle at all, then it's not crew size -- it's zero crew size.

DR. LOGSDON: But you're not saying don't fly more humans than you have to?

GEN. CASEY: That's correct. I am saying that.

DR. LOGSDON: You are saying that.

GEN. CASEY: Yes, I am saying that. For whatever the mission requires.

MR. HUBBARD: I would like to ask you to draw on your collective aircraft, expendable launch vehicle experience and what you know about the Shuttle as a system and ask would you characterize the Shuttle system as experimental, developmental, or operational?

GEN. CASEY: Well, I agree with what was said here earlier today. I think it is all of those or both experimental and operational because they keep changing certain things; but, in fact, the bulk of it is operational, in my mind.

MR. HUBBARD: I'm sorry, what?

GEN. CASEY: The bulk of the system is operational. I think the changes are small in terms of the total system.

MR. HUBBARD: Okay. So now let's compare this to the ELV world where they've got thousands of launches by comparison with 100-plus here, yet the success rate there, fleet-wide, is .96 or something like that. What kinds of ongoing system engineering are employed in the ELV world to try to push that number up closer to 1.0?

GEN. CASEY: Well, I think the recent systems have put a lot more emphasis on qualification and margin; but that won't be demonstrated until we get some history on them. In the past we've had a very large diversity among the various ELVs in the U.S. inventory; and most of them descended from ICBMs, which were greatly modified, sometimes not fully qualified in the changes. And they also have the difficulty in the launch vehicle world of ELVs where few of them are the same -- that is, you don't fly the same thing. We did not have that problem in the ICBMs. We flew the same over and over and over until we got very repeatable results, sometimes not as good as you'd want it.

MR. HUBBARD: Last follow-on in this thread of what can you do to improve and what does it mean to have a comprehensive system engineering program. I think you may have answered this partially already, but can you give us a few more definitions or thoughts about what comprehensive system engineering approach means to you?

GEN. CASEY: Well, I do not claim to have looked at the Shuttle system across the board or the NASA approach. I think the system is so large it has some of the aspects we heard here today where there's so many different delegations that it's hard to say that one system engineering group is, in fact, looking at the margin on this flight for all the things. That's the difficulty, in my mind. It's such a large system and so diverse in where the work is done that it's hard to pull this together. I would like to have, if I was managing this program, some confidence that there was a central system engineering group that had a good handle on what the margins are. Perhaps it exists. I don't know. I don't claim to know one way or the other, but I think your board should know.

MR. HUBBARD: Is your impression drawn from the hand-offs that occur between these various elements, the coordination between the elements?

GEN. CASEY: I can't criticize that. I have not looked at it.

ADM. GEHMAN: But in your experience as a program manager of these complex ICBM systems, you had the same or similar problems. I mean, even though it was an unmanned system in the Minuteman program and things like that, the nation expected more than just that the thing launched. It also had to hit what it was aiming at. So it is a complex system. So did you use any particular techniques to make sure that the system was integrated, the program was integrated?

GEN. CASEY: Oh, yes. In fact, in both Minuteman and the MX or, as we called it, Peacekeeper, the operational name, we were the integrating contractor. We had system engineering contractors supporting us; but we actually wrote the specifications, individually wrote the contracts for each of the rocket motors, the guidance system, and each of the parts. We were responsible for the specification at the level of the missile itself and, in fact, enforced the qualification test of each and every part before it ever met the other parts in the assembly process. So, yes, we were, in fact, very much involved, making sure that we had qualified all the hardware to levels that are significantly above what it was expected to see in flight or in operational ground operation.

ADM. GEHMAN: That's very interesting, and we could go on all evening about that because there are elements in the Shuttle program which have demonstrated their reliability and there are certain qualification tests which have been backed off on due to success. So what you're suggesting is each component has to be qualified.

GEN. CASEY: Yes, sir. In fact, just one more thought on that whole area. I hear a lot from my own cohorts in the ELV world about "in family." In family is also a nice thing to watch. You can tell whether hardware is beginning to drift out of the way it was produced before; but again, it tells you nothing about margin because you might be in family right in the center and you might be very close to the edge if you're operating outside of what you qualified it to.

ADM. GEHMAN: General Casey, thank you very much for agreeing to help us work on this problem, which we haven't got quite solved here. Your insights strike a very good accordance with what we're feeling here, and you've paraphrased a couple of obscure concepts for us very nicely. We thank you very much. Thanks for your help.

GEN. CASEY: Thank you.

ADM. GEHMAN: Okay. This public hearing is closed for today.

(Hearing concluded at 4:18 p.m.)

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March 26, 2003
Cape Canaveral, Florida

Columbia Accident Investigation Board Public Hearing
Wednesday, March 26, 2003

9:00 a.m.
Radisson Hotel
8701 Astronaut Boulevard
Cape Canaveral, Florida

Board Members Present:
Admiral Hal Gehman
Rear Admiral Stephen Turcotte
Major General Kenneth Hess
Brigadier General Duane Deal
Mr. G. Scott Hubbard
Dr. Douglas Osheroff
Dr. John Logsdon

Witnesses Testifying:
Mr. Michael Rudolphi
Mr. Steve Altemus
Dr. Gregory Kovacs
Mr. G. Mark Tanner

ADM. GEHMAN: All right. Good morning. This, I think, the fifth or sixth hearing of the public hearing of the Columbia Accident Investigation Board is in order. Today we're going to cover the subject of what we can learn, what we have learned from debris collection and analysis, which is one of the important avenues of our investigation and one of avenues that we have a lot of hope for. We're going to hear from both the debris collectors and the debris analysts today.

The first panel consists of Steve Altemus, who is one of the Shuttle test directors and is in the debris collection management system, and Michael Rudolphi, deputy director of the Stennis Center, who is also part of the debris collection team.

Gentlemen, before we begin this morning, let me first ask you to affirm that the information you are providing the Board today will be accurate and complete, to the best of your current knowledge and belief.

THE WITNESSES: Yes, sir.

ADM. GEHMAN: All right. Whichever one of you wants to start, would you please introduce yourself and tell us both your NASA job and your job in the debris collection role.

MIKE RUDOLPHI and STEVE ALTEMUS testified as follows:

MR. RUDOLPHI: Okay. I'll go first. My name's Mike Rudolphi, and I'm the deputy Center Director at the John C. Stennis Space Center in Southwest Mississippi. I am, along with the rotational assignments of Dave King from the Marshall Space Flight Center and Allen Flint from the Johnson Space Center, are what we call the NASA Oversight Group at the Lufkin Command Center for the debris collection in East Texas.

A little bit on my job history. I took over the job at the Stennis Space Center as a deputy director in December of this last year. Prior to that, I was the project manager on the reusable solid rocket motor project for the Marshall Space Flight Center, which was responsible for the design, manufacture, and production of the solid rocket motor for the Space Shuttle, with Thiokol as the prime contractor manufacturing in Northern Utah. For the solid rocket motor project, I was the project manager there for about three years.

Prior to that, I was the project manager on the Solid Rocket Booster project at the Marshall Space Flight Center, with similar responsibilities for booster hardware on the Space Shuttle; and I was in that job for about a year. Prior to that, I was the chief engineer on that project. I think that goes back far enough.

ADM. GEHMAN: Thank you. Let me introduce Steve, and then we'll get started with the presentation.

MR. ALTEMUS: Okay. Thank you for having us here today. My name's Steve Altemus, and currently I'm serving as the reconstruction director here at KSC for the *Columbia* reconstruction effort. I have served for the past five years as the Shuttle test director, responsible for managing and integrating the launch countdowns and executing them down through the critical terminal count phase.

Prior to that, I did serve as a landing recovery director in the Launch and Landing Division, responsible for integrating the landing recovery efforts here at Kennedy, and also as the NASA convoy commander responsible for receiving the vehicle and directing the convoy at the Shuttle landing facility upon touchdown.

ADM. GEHMAN: Thank you very much. Would it be incorrect for me to summarize that, Mr. Rudolphi, you're in charge of the field collection efforts of all the thousands of people out in the field and, Steve, you're in charge of the reconstruction in the hangar?

MR. RUDOLPHI: Yes, sir, that would be fair.

MR. ALTEMUS: That would be correct.

ADM. GEHMAN: All right. The floor is yours.

MR. RUDOLPHI: When I was asked to update the Board on the recovery efforts, I elected to use some slides that we had used roughly ten days, two weeks ago, when we updated the local community on the status of the recovery effort to kind of bring them up to speed on what we were doing and how the efforts were going. So I selected a few of those slides, and that's what I'll use here this morning.

In terms of background, I think we all understand the first bullet on that page. Shortly thereafter, President Bush issued those emergency declarations, one for Louisiana and one for the state of Texas. NASA was defined as being the lead agency for the investigation. FEMA is the lead Federal agency responsible for recovery operations.

It was, indeed, a multi-agency response; and some of these numbers are going to be fairly significant in terms of the amount of people involved and the number of different organizations involved. It says more than 92 Federal, state, and local agencies, volunteers, and private groups responded. That number probably will never be known, the exact number of agencies and individual organizations that responded; but the response was overwhelming.

The NASA, the FEMA, the EPA, the Texas and United States Forest Service, DOD, Navy, Coast Guard are still on Board; and they are doing a preponderance of the recovery of the debris. We're working approximately 5700 people, and that number fluctuates on a day-to-day basis. I checked those numbers this morning; and we're working about 5,400 people today on the response and recovery.

I put this next slide up just to give you kind of a logo spread of all those individual organizations involved; and it is, indeed, overwhelming and has been a tremendous effort on all those organizations' part to make this successful. They have all worked together in support of our effort and the FEMA effort in actually collecting the debris.

This is a slide of the debris field. The blue dots on the page represent either a reported sighting of debris or an actual place where we have picked up debris. There's a good bit of information here that I would like to share with you. The first is the green arrow. It shows that as debris is picked up, it is assembled at Barksdale and there it is packaged and shipped to the Cape.

In this box right here, that's the Longview staging area for the Forest Service workers that come in. As I have stated earlier, we're working somewhere in the neighborhood of 5,000 forest workers; and they, by their job requirements, rotate in and out on about a three-week cycle. So we are continually moving people in and moving them out. At Longview there is a runway with the capability of handling the charter jets or the Forest Service jets that they use to move those people in. So we use that as a staging area, and from that position the workers are bussed to the various ground location sites.

We have a work camp at Corsicana, we have one at Palestine, we have one at Nacogdoches, and we have one at Hemphill. Those are the ground search areas. We have two air search areas, one at Palestine and then one works out of the air field at Lufkin. And then we have our headquarters also at Lufkin. Then our water search is down here by the Coast Guard and Navy. It works down in the lake in this region right here.

We also have a procedure in place that if there are items that are of significant importance that would need immediate attention by the Johnson Space Flight Center, we have a method where we can ship those directly to the Johnson Space Center.

The debris field is about 250 miles long, 4 or 5 miles wide; and we have focused our ground search and air search on that corridor. We also have a little bit of runover in the state of Louisiana, and I'll show you that as we progress on.

This is a ground search and air search grid. We've broken it into two-minute by two-minute boxes. That's roughly 800 and a few acres. It was set up in this manner so we can methodically walk through the search and make sure that we cover all the area. Up here at this region up here, this is across the debris zone. It's a 4-mile stretch.

Our intent is to ground search the 2-mile zone and then use air search on the outlying areas around that. That's all driven by the items that we find. Obviously if we move into an area where there is more need to do more ground search, then we let the debris that we find drive us to that. In working here with the guys at the Cape, we coordinate that effort and see if we need to move into a greater area of search or some area of special focus.

Our ground crews. As I said, each one of these is about 8 acres. The helicopter crews, which is our air search, can cover about eight of those boxes in a day. Our ground crews can cover about two boxes in one day or roughly 10,000 acres.

ADM. GEHMAN: What do the color codes mean?

MR. RUDOLPHI: That's the method of us tracking the completion of those search zones. The green means that the search has been completed, the yellow means it's in process, the red means it is working, and the white still to be activated and turned on.

ADM. GEHMAN: So the green means completed. Is that right?

MR. RUDOLPHI: The green means that it is completed. However, I will tell you that it gets a little fuzzy because some of it will be searched by air and then we will turn around and maybe in some areas we would search that by ground also. But there is method in it and it is well defined on our accomplishment charts.

I put this chart up just to give everyone an appreciation for where our workers have come from. We've had firefighters from virtually ever state of the union participate in this activity. As I told you a while ago, we're working about 5700 people on a daily basis. When you add all those up, you know you're going to end up with more than 5700 people; and that's because we're going through a rotational program. We are trying to capture everybody that's participated, and the interest of the ground crews and their participation has been quite remarkable.

This is just a picture of the folks in the field. As you can see, it's a pretty intense activity. They literally go shoulder to shoulder and walk through the brambles and the woods, looking for debris. Each crew is composed of roughly 20 firefighters accompanied by three or four Environmental Protection Agency specialists who do the actual identification, marking, and tagging of the debris and then pick it up. They also will have a NASA person with those guys to help them in early identification.

ADM. GEHMAN: Is this a good time to talk about the imperative of spring and with foliage? I mean, this is pretty indicative of what will happen when this vegetation is all in full bloom and leafed out.

MR. RUDOLPHI: That's a good point. At this stage of this photograph, the foliage was still dormant. As the spring comes on, the undergrowth begins to leave out and the canopy begins to cover. Obviously, first of all, the first area you'll lose search capability will be in from the helicopter where we're at tree-top level. When that gets leafed out, obviously you're not going to be able to see that. Then on the ground as the brambles and the briars begin to leaf out, it's going to damage our ability to see and to identify any material on the ground.

The Forest Service thinks that we've probably got another four to six weeks before that becomes real serious. As you can tell if you have been there -- and I have been there in the last two or three weeks -- it has already begun to leaf out and will impair our ability to find as much as we would like to find.

ADM. GEHMAN: From a management point of view, there is a time element in what you're doing.

MR. RUDOLPHI: I would characterize it there is a time element to do the thoroughness that we would like to do. Obviously we can keep walking around in the woods after it's greened up and we can keep looking; we just won't be as successful.

ADM. GEHMAN: Okay.

MR. RUDOLPHI: As I talked about, we've also got air search going; and this is a kind of a portfolio of the various aircraft that we have either used or plan to use. Primarily the most successful devices that we've had is the helicopters where they go in at tree-top level and clear those regions along the band of the debris field. We do have a DC3 that is working, some equipment that's helped us work out in the western part to see if we can identify material that might have come off early. It's to be decided yet if that's going to give us the success that we would like to have.

Here's the boat operations. The method of identifying targets is with sonar and other underwater identifying devices, and what we do is we mark the target and then send a diver down to look at that. We've identified some 300-plus targets in Lake Nacogdoches and have just about completed that, and so far we've not found anything in Lake Nacogdoches. I forget the name of the lake on the other end right now, but the large lake at the Louisiana-Texas border has the same processes in place. We've identified several, maybe 1500 or so targets out there and they are diving on those and so far they have not brought us a lot of success, but we're going to keep going until we finish.

This is another way to describe the impact on the state of Texas. All those counties that have some color in them were impacted by the emergency. You can look over here. There's a total of 169 counties were impacted. The number that have cleared has gone up in the last day or two. I've now got new numbers on those, if you want to write them down. The number of cleared counties is now 143 and with 10 pending, leaving a total of 26 left to go. The core counties are the ones in yellow. Those will be the ones that will be the last ones to go.

One of the jobs that we're doing that's important to us and to the FEMA is to make sure that these counties, as we walk away from them, that we have cleared all the potential debris sightings, picked up anything that had been reported, and make sure that those folks understand that they have a way in which, if they find something in the future, they have a way in which they can report it and someone can pick it up.

Louisiana is the same way. We've cleared 31 counties in Louisiana; we've got eight left to go. Again, you can see that it's a fairly significant effort of getting around and making sure that we have cleared all those counties. The debris zone is up here, these two or three counties; and the rest of them were either sightings or potential sightings that someone had called in. So we've got a lot of work to do to clear those up, and we're moving right through that.

In terms of status, we're about two thirds of the way done on the ground, same way with the air, and the water is about 65 percent also. So we're about two thirds of the way done, with the intent in four to six weeks we'll wrap up the field search efforts. Obviously that will be impacted by what we find. If there's a need to go and search broader areas and to look at more sites, we'll do that; but our intent is to press on, not worry too much about the conditions -- for example, the green-up. We're going to search the areas as we have got laid out, and we will be complete when we get the actual searching finished.

That's all I've got in terms of information. I'd be glad to entertain any questions.

ADM. GEHMAN: Thank you very much. Why don't we go to Mr. Altemus, and then we'll come back and ask our questions.

MR. ALTEMUS: Okay. If we could have the reconstruction slides.

ADM. GEHMAN: While they're coming up, let me go back to Mr. Rudolphi a second. Can you talk about Barksdale? Do you intend to close out of your operation at Barksdale sooner or later?

MR. RUDOLPHI: We're using the Barksdale facility right now for assembly and packaging of items for the transportation to KSC. I would anticipate that that operation, since it is established and we've got crews that know what they're doing there and they know how to do their job and understand the need, I suspect we'll keep that place operational through the entire ground search.

ADM. GEHMAN: All right. What can you say about the value or what can you say about predictive search areas? In other words, has there been much effort and have you been on the receiving end of direction to look here, we think this thing is here, and has that been useful and to what degree do you employ predictive measures?

MR. RUDOLPHI: Through the NTSB and their capability, we have done quite a bit of predictive work. The best of my knowledge, I believe there are eight sites outside of the state of Texas, out to the west. Those areas, some of them we have searched without any luck. There are still a few more that we want to take a look at; and as I talked about a little bit ago, we've got the DC3 with its capabilities. We will use that and try to fine-tune that and see if there is something there. Those targets are in terrain that's very difficult to get into, but we are interested and we are continuing to explore the predictive measures which we have worked with the radar folks in the NTSB. Unfortunately, they've just not been as successful as we would like.

ADM. GEHMAN: Okay. Thank you very much.

Okay. Mr. Altemus.

MR. ALTEMUS: Looks like we have the *Columbia* reconstruction slides up. First I wanted to talk to you in terms today of where we've come to date with the reconstruction effort, what efforts we're working on here in the near term, and then maybe where we're looking ahead in the longer term with the reconstruction effort.

Mike, can you hand me the pointer?

As Mike had alluded to, there's four collection sites that are feeding Barksdale; and this is essentially the debris pipeline as it gets to KSC. Items that are collected in Texas are fed to Barksdale, in Shreveport, Louisiana, and then shipped off to KSC, Kennedy Space Center. The primary reconstruction facilities we're using here on Kennedy Space Center are threefold. The mid-field park site up in the upper left-hand corner there is our decontamination site we set up for any items that have been contaminated with the onboard propellants. We can decontaminate them there before we bring them into the main hangar. We also have the auxiliary storage of the clamshell hangar, which is located about a mile and a half from the Shuttle landing facility, and then our main facility, which is the SLF hangar, where we contain most of the debris.

Here you see at the mid-field site some technicians working in supplied air to decontaminate some maneuvering system parts, reaction control jets that have come from back in the field with a little oxidizer or fuel in them. We want to get those cleaned up. We do that at this facility.

Then within the clamshell or auxiliary hangar, we have about 8 to 10 thousand pounds of debris that we store there. Those are items that are not necessarily related to the investigation. They're primarily our tanks, our Orbiter maneuvering system fuel tanks, the helium tanks, Kevlar-wound tanks, nitrogen tanks. Also some of the payload bay door items which are graphite epoxy composite. Those fibers present a hazard to us. We have those encapsulated, and we tend to move those to the clamshell building, out of the way. And I guess engine parts also, the Shuttle main engine parts that we have retrieved, we move down there. So again there are 8 to 10 thousand pounds of debris located in that facility.

Then here in the main hangar, lower right-hand side, is where we're doing the two-dimensional reconstruction of the outer mold line of the Orbiter.

I wanted to talk to you a little bit about the grid layout before we go into the process. Basically we're doing a two-dimensional reconstruction of the Orbiter structure and thermal protection system. We worked with the NTSB and the folks with Boeing in Seattle, the air safety folks, to lay out about a 40,000-square-foot grid, 110 percent scale of the Orbiter. What you do to visualize this is actually take the Orbiter, flip it upside down, and then open it up, if you will, and what you see here is the underside of the Orbiter with the nose facing forward.

We then generated three wing planforms, those on the left and right side. You have a lower surface tile or thermal protection layer. Then in the middle in the hangar on either side, you'll have a lower surface structure. Then towards the aft of the hangar, you'll have the upper surface structure of the wing.

That allows us to get the lower surface structure laid and then eventually migrate the tiles or thermal protection from the upper wing planforms on top of the structure. For space reasons, we've had to locate the vertical stabilizer up in the front. So that's a little bit counterintuitive; and then we also show control surfaces, elevons, and body flap as we had space available. We also have a section here for mid-fuselage tiles so that we could actually lay those out also and see any flow patterns we might have on those.

The next slide here is talking about the process. Basically from Barksdale we're at a receiving process of about a rate of about two trucks per week. We receive a tractor trailer truckload, a flat bed, on Thursday and Fridays. We have received about 15 trucks to date, averaging on the order of about 4,000 pounds a truck. That varies between trucks.

We'll go ahead and offload the trucks in the impound area, up at the top of the page, from forklifts. We'll go ahead and do toxic vapor checks of those items to verify there's no contaminants that are hazardous to the personnel working on them before we move them inside to what we call the uncrate area.

In the uncrate area, we'll take the shipping material and the packing material off of the part, expose the part so that folks can look at it, identify if it's hazardous or not, friable, having manmade vitreous fibers on it. If it is, we'll encapsulate it with plastic wrap and move it down the line to our quality area.

In our quality area, we'll generally create a data pack for each item, which is made up of photos. Here you see a quality technician taking photos of a piece of debris. We'll generate a parts tag for each item and also a bar code. The parts tag will be affixed to the part, and the bar code will allow us to track that item throughout the facility and other storage locations for the life of the reconstruction effort. Once the data pack has been generated, we'll put it into an engineering staging area where engineering will begin the identification process of the parts.

We split that identification process into two areas, one on the east side of the hangar and one on the west side. Primarily on the east side of the hangar over here we're going to work 70 percent of our process, which is to analyze the structure and the thermal protection system as it comes through. Also the fast-track items that have been identified, critical to the investigation, will come through and get priority handling in this area. We'll try to identify where that piece of debris belongs in the system, whether it belongs in the grid and we'll get that out to the grid, or whether it needs to go to storage.

Storage contains all our subsystem components. We're not doing the reconstruction of the subsystems at this time, just mainly the structure and thermal protection of the outer mold line of the vehicle. We did set up a warehouse logistics storage kind of numbering system for our shelves so that we can accurately retrieve any part that's put into storage and precisely locate it for further investigation if required.

This is our grid in the hangar as of Monday. You could see it's actually an aerial view we take a couple of times a week to see how the grid is being populated. It is the exterior of the Orbiter from the underside view, and what's interesting about this photograph is that the parts are relatively small. We don't have many very large parts. I'd say the largest part we have is about the size of a desk.

ADM. GEHMAN: Why don't you just take a second and orient the audience to what we're looking at here.

MR. ALTEMUS: Okay. If you recall the layout, we're actually looking from the north side of the hangar. This is the vertical stabilizer here, at the front of the vehicle. Here you see the nose landing gear. This is the front end of the Orbiter, and then this would be the whole underside of the fuselage. On this side we have the right wing tiles, the right wing under surface, and the right wing upper surface. Then the left wing would be down this side over here.

ADM. GEHMAN: The cluster of people at the left over there are all clustered around the famous left landing gear door.

MR. ALTEMUS: That's correct. Our infamous Part 283 which shows the outflow out of the wheel well across the underside fuselage.

ADM. GEHMAN: Thank you.

MR. ALTEMUS: Primarily our area of concern, as you know, is the left wing; and we're putting a lot of emphasis in populating that left wing and quickly getting those parts accurately placed in the grid.

Just some statistics for you. It is a 40,000-square-foot grid. We've collected, so far, 54,000 pounds of debris. That represents about 24 percent of the Orbiter dry weight, which the dry weight was 223,000 pounds. We've received in over 45,000 parts; and 1,400 of those have been accurately placed on the grid.

Just to talk a little bit about the level of effort. This is a multi-agency effort including the contractors, NASA, and the NTSB, who are resident with us down in the hangar. It's primarily managed and facilitated by the USA ground operations folks. They supply the technicians from launch processing as well as the quality inspectors, the handlers, the logistics function, the safety folks.

The debris identification experts come from a wide variety of areas. They're both NASA and NASA KSC and NASA Johnson Space Center. They're NASA USA and Boeing individuals, systems experts. And in certain cases where we need outside vendors or outside expertise, we'll bring them in. One example of that is we had the Michelin folks down to look at the tires that we retrieved.

The operation runs approximately six days a week, two shifts a day. That's a 16-hour day; and we have approximately 150 people working on those shifts, resident at the hangar each day.

Now, the main thrust of the reconstruction effort at this point has been to receive the parts in, identify the parts, and get them placed to the grid. Also, one of the other arms of this reconstruction effort is to go back and feed Mike and his folks in the recovery effort. So we have several tools that we use to feed the recovery effort to help prioritize their search.

What I have here is an electronic mapping tool which is an accurate representation of the kind of debris that is on the floor on the grid. It actually orients the engineers that are working on the debris as to where in particular that piece of debris belongs on the Orbiter. Electronic mapping tool, I guess, is what we refer to it as. It's only to positively identify parts that are put on the grid. It was a tool that we used for waterproofing the tiles during Orbiter processing, and we've adapted it for this reconstruction effort. The color coding on the underside there. Blue is tile and structure, green is just structure, and then there's some brown over here on the left wing which are individual tiles.

We have a debris-plotting capability with this tool that we've enhanced its capability where we can actually select a part off the vehicle anywhere on the vehicle, that piece of debris, and then it will automatically bring up a plot of where it was found in the debris field, as well as where it is located in East Texas or Louisiana. We actually feed this information back to the recovery folks in the hope to prioritize the search grid patterns that they have laid out for each day for significant items. We've used a similar technique in particular to identify the location of the OEX recorder, which was a recent find. We actually plotted the four corners, if you will, of where the avionics bay contents had fallen; and these guys went back and did a research of these grid areas in East Texas and located that black box.

Like I said, our main focus has been to identify parts and place them to the grid. In the near term here, we have just begun the factual documentation associated with each part of the debris. We'll generate a factual report identifying what the critical fracture surfaces are and where the burn marks are and where there's some melting of molten aluminum, that kind of thing, document that in a report with drawings and photographs, as well as generating a sampling analysis wish list, if you will, for each part.

We can perform some of this analysis locally in the hangar, and what you see here is an engineer performing a stereomicroscopic examination of a piece of debris, which results in almost a 3-D picture of the fracture surface. This information will be included in the factual report of that piece of debris.

Once we take a sample of a part, we have here on this slide an example of metallic contaminant on the inside of an RCC panel where we've taken a sample and sent it to one of our three resident material science laboratories here on Kennedy Space Center. We can sample for metallic contaminants or inorganic contaminants. What you see on the bottom is an inorganic contaminant that was located on a tile. So we have some serious capability here with our three laboratories as the first line of failure analysis; and if we need to, we could also send them off site as these labs fill up or as we need other opinions.

Basically, the point of sampling is to identify what the contaminant is, where did it come from, and actually when did it occur so that we can take this data and look for trends in the debris and also support a scenario, failure scenarios. We've taken about 75 to 100 samples of the debris to date; and they have been primarily on the reinforced carbon-carbon leading edge panels, the leading edge components, tile, and also the uplock roller we took a number of samples on.

We'll eventually roll these factual reports and the subsequent sample analysis into subsystem reports that speak to a whole subsystem, say, a wing leading edge or a structures report, a tile report, and then eventually have that report rolled up into a reconstruction report which speaks specifically to just what the debris is telling us, which is independent of any of the scenarios that are out there.

One of the longer-term tools or concepts that we're looking into is, as we migrate forward with the factual sheets and get as much as we can out of the two-dimensional reconstruction, we may be driven to perform on a component level some three-dimensional physical reconstruction. We're already thinking about what kinds of jigs and fixtures we might need to recreate the leading edge of the left wing as an area of high interest. So we're starting to develop that tooling and think about how that might be accomplished. As we populate the grid with debris, we'll see what other major components might require a 3-D physical reconstruction.

One of the other tools that we're working with that we're hoping will bear some fruit for us is a virtual 3-D reconstruction effort. We're actually using 3-D laser scanning technologies to create a virtual model of the Orbiter debris that's been collected. We place the debris with known Orbiter coordinates on a 3-D CAD model. This tool can also serve to actually help mate fracture surfaces together and essentially put the puzzle together for us. So we actually select a piece of debris, run it through an algorithm that matches up the curvature and fracture surfaces with other areas on the vehicle that have already been populated. Then within the tool, we're developing the capability to recognize patterns of melted metal across several components so that we can get a map, if you will, of the slag or the molten metal across several parts, say, on the wing leading edge.

This is a technology that was in place within the Shuttle Program. It was a digital Shuttle project which we were using to identify or actually collect as much as-built and as-designed information on the vehicles. As you know, the vehicles were constructed with just two-dimensional drawings and it was never a really good CAD model. So this effort was putting a good CAD model together for the Orbiter. It's a collaborative effort with Ames Research Center, Johnson Space Center, and Kennedy Space Center. So we're hoping it'll provide us some insight here.

I have a demonstration of the 3-D virtual reconstruction that I can play for you. If you could bring up that little demo for me.

What you see here is a model of the Orbiter. In red are the pieces of debris that's on the left-hand wing. What they'll do is they'll create basically a cloud point of light associated with the piece of debris. They'll fill in the surface where you can rotate that part around. It's a 3-D part. They'll place it up to the vehicle. Like I said, in red you can see the pieces on the left-hand leading edge. That's where we're focusing right now, the reinforced carbon-carbon leading edge panels. Actually we've scanned in about 140 parts to date, and this is just a demonstration of a leading-edge piece that we've shown. Hopefully, this will provide us some insight down the road.

Basically, that's where we've come to this point. In the near term, we're going to be getting those factual reports and descriptions of debris and, in the long term, migrate towards that three-dimensional virtual reconstruction. That's all I have.

ADM. GEHMAN: Thank you very much.

MR. HUBBARD: I'd like to start off with a couple of questions for Steve. First of all, you showed, on one of the slides, 45,000 parts had come into the hangar. Now, I assume by that you don't mean part as in identifiable Orbiter part but rather a piece. Is that correct?

MR. ALTEMUS: That's correct. It's a debris item. We've received over 45,000 debris items. They may be as large as this table or as small as a quarter. Each part that comes in, though, receives the same attention with respect to its data pack. It will get an individual part number and actually get an engineering disposition put on it, and that is recorded in our *Columbia* reconstruction data base so that we could actually track that part throughout the facility.

We do have storage and bins, if you will, for the variations of how identified that part or that piece of debris becomes. No part is truly an unknown piece of Shuttle debris. It's first classified as whether it's Shuttle or Orbiter, non-Orbiter, or payload. You run it through that filter. You then look at what system it's related to. If you cannot identify the system, you'll run it through a material screen which says what material is it. Is it unknown tile, is it unknown tubing, unknown electrical? So we have categorized all the debris to some extent in some fashion.

MR. HUBBARD: Now, of those 45,000 pieces, about 1,400 are laid out on the floor there. Can you explain to us why there's only a few percent of what's coming in is laid out on the floor?

MR. ALTEMUS: Actually the two-dimensional reconstruction is strictly the inner mold line structure underside of the vehicle as well as the thermal production system outer mold line of the vehicle. So those are the only components that actually migrate to the grid, with the exception of a few key pieces like elevon actuators or landing gear that kind of give you a physical or a reference point in the grid. The other system components, all the subsystem components like a fuel cell or an auxiliary power unit will go on storage on the shelf. They've still gone through the screening, the initial triage from engineering to identify to some extent what that part is; it's just they're not necessarily relevant to the investigation at this time.

MR. HUBBARD: Now, can you take us just quickly through how you would identify a part that's critical to this investigation, like a leading edge subsystem, one of these reinforced carbon-carbon items? How do you know to put it on Panel 6, for example?

MR. ALTEMUS: As an item comes in from the field, initially there's a group of engineers out at the collection sites who are giving an initial identification of what they think that part is. That part's sent to Barksdale and comes to Kennedy Space Center where we have our team of engineers look at that part and we have reference in the facility, reference to a Shuttle drawing system, where they can actually pull up drawings, try to match the features of a part, whether it had a physical part number etched into the part, whether it had an alteration made to it where you have what we call an MR stamp put on the part where it had some modification done to the part. You may see, for example, a doubler on a piece of structure that was a modification that you can accurately place where that part was. Also there's some sampling techniques where you can actually clean a tile, maybe try to raise a part number off of that.

Specifically with tile, for example, you can go to the thickness maps that we have and actually measure the thickness to within a hundred thousandths of an inch and get a general location of where that tile would be zonal on the Orbiter. So there's a lot of indicators on how to identify that particular part.

With respect to the leading-edge panels, the RCC panels, we set up a work station right by the left-hand wing where we can actually take these pieces and put the fracture surfaces together. We have the folks who are knowledgeable about RCC panels who can mate those together and understand the differences between left and right side, RCC Panel 9 and 10, the subtle differences. So any clues that we can glean from the part will help us identify it and place it on the grid.

MR. HUBBARD: Have these initial identifications been stable, or have they changed over the last several weeks?

MR. ALTEMUS: Actually that's a good question because initially as we place parts on the grid, we know their -- we take a swag at where it might go on the grid. We know it's a piece of importance, and then that's our initial triage of the part. We will then go back and do an iterative process with the engineering folks who revisit the parts on the grid on a regular basis, audit the parts on the grid, and determine if we actually have them in their correct place.

We've actually had parts migrate from the left wing leading edge, which we thought were critical. They've migrated over to the right side. We've had parts from the right side migrate over to the left side, just as we get a better understanding of these parts and where they may fit in the whole puzzle.

MR. HUBBARD: Thank you.

DR. LOGSDON: Steve, the Board's investigation is focusing on the left wing and where it attaches to the fuselage. Do you have more, less, about the same of that, compared to the total population of the grid?

MR. ALTEMUS: Our emphasis has been in the recovery effort, through our significant recovered items list, to emphasize parts on the left wing to get those searched for and sent back to the Kennedy Space Center on a fast-track process. If you look at the grid, you can actually walk through and see that on the left wing lower surface there is not a whole lot of structure there. There's not very much structure on the upper wing of the surface of the left side.

On the right side of the vehicle, when you look at the right wing, you can actually see there's quite a bit more lower surface structure there. I'm not ready to explain why that is, but at this point it just appears that that seems to be the case. So as far as the mid-fuselage goes, there's some critical interface pieces that we've received that we put in context with the left wing. There's maybe a dozen or so interface pieces between the mid-fuselage and the left wing that we've identified, and that seems to be typical on both the left and the right sides.

DR. LOGSDON: You have a priority list of things you'd like to find. Could you talk about that a bit?

MR. ALTEMUS: Well, obviously the telemetry has pointed us towards an anomaly with the left wing. So the left wing items, specifically the RCC panels, the RCC fittings, the upper and lower fittings that attach the RCC panels, the wing box, the intermediate wing components, the wheel well -- those are all items of interest we'd like to get our hands on and put to the model so that we can extract any clues to the investigation.

DR. LOGSDON: What about the piece that came off on Day 2 of the flight? Maybe this is more for Mr. Rudolphi. What's the status of looking for some of the early debris?

MR. RUDOLPHI: Well, relative to the earlier question that was asked about the radar contacts and the narrowing-down of zones, we are continuing to try to find parts or pieces that have some kind of indication either by radar or -- and primarily it is by radar -- or I guess we have some cases where we've had some sightings. We've continued to look for those pieces in the Western Utah and Nevada region and as far west as California, but so far that's just not been productive.

DR. LOGSDON: Just nothing.

MR. RUDOLPHI: Nothing. Right. We have not found to date a piece outside of the state of Texas and Louisiana.

GEN. DEAL: I've got a question for Mike the and one for Steve, as well. Mike, in the first week of the Board, Admiral Gehman took us all out to view the debris field; and at that point in time most of the debris was centered around roads and populated areas. There was a lot of talk during that time about perhaps offering incentives or encouragement, everything from bounties to certificates to even a Scout merit badge. Are we still proceeding towards any of those, or is that a dead issue? Do we think the well's kind of run dry?

MR. RUDOLPHI: Let me answer the question this way. Early sightings were in the inhabited areas because that's where the people were that saw. We have had very good response from the local communities and individuals in identifying and turning in reports and helping us find and pick up things that they have identified. As a matter of fact, one of the metrics that we are tracking in the field is return calls. Obviously we're making a lot of progress. We still get a few return calls, and over the last few weeks they have gone significantly down, meaning that we have picked up the debris.

The idea of incentivizing folks was not necessary. I think we got cooperation from the locals without having to do that. So where we are today, I would say that we have responded to the community in those areas, picking up the debris as they have identified it, and we're now just in the throes of going through systematically, of walking the areas or searching the areas and looking where we didn't have reports.

GEN. DEAL: Do we have any plans to thank those that have come forward?

MR. RUDOLPHI: Since I've got the mike, I would like to use this as a forum to just talk about that just a second. The response from the community and from the organizations has been overwhelming. I think all of us who have been there and have participated in this activity, there's just no way to express to the local communities the interest, the sharing of their community and their resources with us as we go through this effort.

We do, indeed, have plans to come back and somehow try to thank the folks. I don't know whether we'll ever be able to adequately do that or not. One of the activities we are undertaking is we have got the space flight awareness organization in the Space Shuttle Program on site; and they are there every day, working with the workers and local communities, personally thanking them for the work that they're doing and for their participation. Again, it's absolutely overwhelming, the support that we've got; and we cannot thank those people enough, both the professional organizations, in terms of the logos that you have seen, and the individuals that have been involved in this.

GEN. DEAL: We applaud them, as well.

Steve, a question for you. We've been on the floor with you, scratching our heads, trying to figure what some of those parts are, particularly as it went through the gyrations on that left main wheel. Are there any lessons that you have in the back of your mind that have come out either for future Shuttle missions or future design systems where, you know, God willing, we'll never have another accident like this, but to help to us identify parts better or mark parts better so that if we ever do need to either disassemble or have another accident investigation, we can identify what those parts are earlier?

MR. ALTEMUS: I know a lot of the teams associated with identifying parts have been thinking of those exact things as they go through and struggle to identify the parts. There may be some techniques to etch part numbers into some of the structure, into some of the subcomponents, because the heating that this debris has gone through has really taken off all of the stampings; and specifically with the tile, we've lost all the markings on the tiles. So we're really focused on the thicknesses. So there may be some findings that come out of this that find a better way to mark the tile or etch part numbers more frequently and into the structure themselves so they're not lost in this kind of environment.

MR. WALLACE: I guess for Mr. Rudolphi. You've recovered 24 percent approximately by weight. Have there been any calculations as to what percent by weight of the Orbiter likely made it to earth?

MR. RUDOLPHI: Well, Steve and I were talking about that right before we came in; and we talked that with the community at large in terms of what we might expect. I think it would be reasonable to expect somewhere between 35 and 50 percent, and that's our guess. We may be surprised, but we would think that would be reasonable.

MR. WALLACE: Of what returned to earth. So then that would suggest that you've recovered probably well over half of what returned to earth, by weight?

MR. RUDOLPHI: That's a different question. I would hope that we find a large percentage of what returned to earth.

MR. WALLACE: Sort of a follow-on question to that. Can you characterize just sort of generally the pace of the recovery in terms of by weight or whatever other measure you want to use? Is the faucet starting to slow to a trickle?

MR. RUDOLPHI: With the ground folks, we're covering about 12,000 acres a day and we're turning in somewhere between 6 or 7 hundred pieces or bags of material and that has been that way now for 2 1/2, 3 weeks. So we're kind of at a plateau right now on what we're finding and recovering.

MR. WALLACE: Most of the debris field here, the known debris field, of course, doesn't go much west of Dallas-Fort Worth; but, of course, there are those critical early debris-shedding pieces. Is that all a single, coordinated effort?

MR. RUDOLPHI: Yes. The entire effort in the state of Texas is coordinated out of Lufkin with our search --

MR. WALLACE: I guess I was thinking about some of those things in Nevada.

MR. RUDOLPHI: That is also, in large part, coordinated out of Lufkin also. So it's all one focused effort.

MR. WALLACE: Where the change of seasons may present a challenge with foliage, are you optimistic that, in those areas where snow is an issue, the change of seasons will work in your favor?

MR. RUDOLPHI: I would say it this way. We're always hopeful that we'll find something out west. West of Texas. We've not been productive in finding it.

ADM. GEHMAN: Let me follow up on that question, as long as we're talking about that. I mean, I know that you both realize the weight that the Board places on finding the first things that came off the Orbiter -- which, of course, are probably in the Dallas-Fort Worth area or west of there. And the search briefing that you gave us today with those grids and 25-person teams, five people a part, doing 2 acres a day kind of a thing, as I understand it, that's the plan for the kind of a center of the debris field where there's debris everywhere. But tell me about the plan for searching the less fruitful, more difficult area where the debris is much more scattered, essentially west of Dallas-Fort Worth. Is there a plan for searching out there?

MR. RUDOLPHI: In the areas west of Fort Worth to the West Coast, we have got, I would say, several different approaches to how we identify areas where we would like to search. We have actual what I would call reported sightings and we have got radar indications, which we have the NTSB working with us full-time on a regular basis, working a myriad of radar contacts, doing the assessment of those, and that is pointing to areas where we would have potential targets.

We have searched some of those ground targets on foot with crews similar to what we have at the main debris field. We have searched some of those areas with the light aircraft, and I'm trying to think of the name of that. Civil Air Patrol. We have got plans to use the DC3 to help us identify targets. We are using all those means available to us to try to identify and find that part or those parts that are the farthest west we can go. I will tell you we do have a plan, we do have a method, it's just not very productive.

ADM. GEHMAN: Well, I agree. I mean, there's not much out there; but it is key, we think.

MR. HUBBARD: A question for Steve. We had on a previous hearing an expert on re-entry debris; and he made the statement that aluminum, at least in the spare sense, just aluminum skin, aluminum structure, hardly ever makes it to the ground from a space re-entry perspective. Has this observation held up on the floor? I mean, are you finding much aluminum by itself?

MR. ALTEMUS: I believe it is supported by the debris on the floor in that, at least from what we see now, is we see that a lot of the parts have come through on an aluminum molten rain cloud, if you will, where they have aluminum splatter over much of the debris. We see that molten aluminum on almost everything that we have back. It may also speak to why we don't have upper wing surface on the left wing in that that's very thin aluminum that's not as protected. So, yes, I think you're exactly right in that we expect a great deal of the aluminum not to have made it to the ground.

MR. HUBBARD: I'll follow that up just a little bit. If you were to stand back, you know, 50 feet or 100 feet and look out at the pattern that's emerging, are there any holes in the grid, any places that you would think you might see a piece and there's just nothing there?

MR. ALTEMUS: Well, there's actually a lot of holes in the grid at this point.

MR. HUBBARD: Looking for major patterns, yeah.

MR. ALTEMUS: There's only 24 percent of the Orbiter; but what did strike me as odd, first of all, is the size of the pieces, how small they actually are, and also there's very little left wing lower surface structure and very little left wing upper surface structure. When you walk the grid, that's what you can notice by what's not present as opposed to what is there.

MR. HUBBARD: Okay. Thank you.

ADM. GEHMAN: This is a question for Mr. Altemus. I have described in general terms the challenge that this Board has, which is to overlay essentially six independent investigations in order to find a match that describes what happened. In rough terms, they are an aerodynamic reconstruction, a thermodynamic reconstruction, a time line based on the telemetry, a photographic and videographic reconstruction, a documentation reconstruction of everyone who touched or repaired the Orbiter in its turnaround, and the sixth one is the debris. So can you say whether or not you have a plan to develop where you are in developing your theory of what happened, based on what the debris is telling you? If it's too early, just say that; but can you tell me in your own sense where you are in the debris talking to us?

MR. ALTEMUS: Actually we're currently in the phase -- you know, we did the initial phase of ID'ing the parts and getting them to the grid. We've recently ramped up the process of sitting down with each piece of debris, each critical piece of debris, say, specifically left wing pieces, and thoroughly documenting factually what we're seeing there and generating that sampling wish list. Those factual sheets will be rolled up into subsequent reports here, and we think that we can generate that stand-alone sense of what the debris is telling us in a time frame of 60 days or so from the time that we terminate the recovery effort. After we get the debris back, in about 60 days or so we're thinking that we can have that report generated.

ADM. GEHMAN: Thank you.

DR. OSHEROFF: I have a few questions. I don't know who's most competent to answer this. What fraction of the Orbiter was actually made of aluminum, if, in fact, we've recovered rather little of that?

MR. ALTEMUS: I don't have that data handy for you, but we can go off and get that.

MR. RUDOLPHI: I wouldn't have an idea. I would be glad to run it down, but I don't know.

DR. OSHEROFF: It would be interesting because if, in fact, most of that burned up, that would probably put some sort of limit on how much you can expect to recover.

The second question. Did I hear correctly that the OEX recorder was found in an area which had actually been searched before? Mike, I guess.

MR. RUDOLPHI: My answer to that is I'm not sure. We are searching some areas twice by virtue of the fact that we searched the first area in our initial effort to find crew remains and we did not focus on hardware. So it may very well have been that situation. I was not on the field -- I was not on the ground there when they found that the other day. So I can't answer that specifically, but it is reasonable that that would be a response, that it could have been searched and is now being more thoroughly searched for hardware. That could be the case. I can run that down exactly if it had been.

I do not know that we are searching places twice. I know of no places we're searching places twice because we went back and found something by some other means. That's the only reason I know that we would be ground-searching something twice. We have searched places twice where we have found good targets with the air search and decided we need to move in there and do ground search. So those statements could be true.

MR. ALTEMUS: Along the lines with that, a little more insight into that is we have been working the process back and forth between the reconstruction and recovery folks and have actually helped with those prioritizations of researching certain areas that have critical pieces in it. In particular, when we plotted out the corners of the avionics bay contents, it was in an area that was initially searched; and we went back and searched that again in a little more detail. And it may have been along the lines of initially searching for remains, as Mike says, and came back and did a hardware search of that area and uncovered the OEX recorder.

DR. OSHEROFF: Is the expectation, then, that if you've done a ground search, that you've probably recovered virtually everything that one could expect, maybe larger than --

MR. RUDOLPHI: I'll say that the size and the type of debris that the crews are bringing in are everything from the size of, say, a nickel to larger. They are bringing in everything that they can, everything imaginable that you believe they can see. So I'm confident that after we have walked the area down, we will have found anything of any size. Now, there's always a chance you'll miss something; but I believe debris of any size will have been picked up.

As I showed on the debris field search effort, that is down the 4-mile corridor down the middle; and on the outside of that is done with air searches. If we find something by air search that we would like to push into and we think is important to push into ground search, then we'll do that also. Our plan is, I believe, that after these guys have walked those areas down, they're going to have found anything of any magnitude. Yes, sir.

DR. OSHEROFF: Thank you very much.

ADM. GEHMAN: All right. By way of closing, I would like to ask Mr. Rudolphi whether or not there is a seasonal aspect to your search. What I mean is we talked about foliage, but is it planting season, plowing season, hunting season, fishing season, or is there anything that we ought to advise the public here that's related to the seasonal activity in that area?

MR. RUDOLPHI: We've already taken some actions along that line. We have put out a notice in anticipation of the farmers going to work, advising them what to do if they come upon stuff in their activities. Of course, East Texas is a highly intense timber-growing area. We've also advised those folks, should they come upon it, how to do that and what to do with it. So I think we have taken those steps, as you have alluded to, that we need to alert these folks.

Springtime, there will be a lot more activity. As you get out into the western region, it is a more agricultural area. Haying and farming activity. So we anticipate that we'll have more, possibly more call-ins as folks go out and walk their land a little bit more and become more familiar with their property. So I think we've done the prudent thing in allowing folks to -- or giving them the information that they need so they can make those contacts, should they find something.

ADM. GEHMAN: Well, thank you very much both of you gentlemen. On behalf of the Board, I want to echo the comments that have been made earlier about the remarkable efforts of 4 or 5 thousand people a day, sometimes more than 5,000 people a day, that have been searching diligently for the debris is a wonderful testament to the American spirit.

The debris is enormously important to this Board. We continue to learn things from the debris, almost on a weekly basis. The last two weeks have been good weeks, as a matter of fact, between the main landing gear door uplink roller and the OEX recorder; and these discoveries are only found by just plain old hard work. The Board is enormously grateful to you, Mike and Steve, and also to the thousands of people that you represent here today.

I'm glad we had an opportunity to put all the names of all the agencies and organizations up there because I know it goes all the way from local private citizens to local community associations to Forest Service and sheriffs and fire departments and the National Guard and includes everybody. We are very much aware of it, and we are very much grateful to it. We haven't solved this yet and we don't know that tomorrow or next week an important discovery will be made out there in the debris collection area or in the reconstruction area by putting two of these things together. That discovery is still out there waiting for us, and so we're banking on it. So please keep up the good work. I know you do this on a regular basis, but you can certainly express on our behalf our gratitude and our admiration for all the good work.

We're currently going through a period of relatively good weather. I know that in the last couple of weeks that this was going on, it was not quite so nice out there. So it's quite remarkable.

So thank you very much. You're excused.

We'll take just about a five-minute break while we seat the next group. So please don't go too far.

(Recess taken)

ADM. GEHMAN: All right. We're ready to resume. Thank you very much. The second session this morning will be about debris analysis and debris reconstruction. We're pleased to have two folks help us through that today, Dr. Greg Kovacs and Mr. Mark Tanner.

As is our process here, before we begin I'll just ask you to affirm that the information you're going to provide to the Board today will be accurate and complete, to the best of your current knowledge and beliefs.

THE WITNESSES: Yes, sir.

ADM. GEHMAN: All right. Would you please introduce yourselves and give us a short summary of what you do, both for the accident investigation and in your daytime jobs.

GREG KOVACS and MARK TANNER testified as follows:

DR. KOVACS: My name is Greg Kovacs. I'm a professor at Stanford University in the School of Engineering and also work in the astrobionics program at NASA Ames, developing medical monitors for humans and space flight hardware for biological experiments.

ADM. GEHMAN: And as part of the accident investigation?

DR. KOVACS: I'm serving as the investigation scientist for Group 3. I'm involved in debris analysis and things of that nature.

MR. TANNER: My name's Mark Tanner. I'm a senior consulting engineer with Mechanical and Materials Engineering. My career has primarily been failure investigations, accident reconstruction; and what I'm doing with the Board is, as being a failure analyst, looking at the debris and coming up with plans to try and focus in on the origin area.

ADM. GEHMAN: Thank you very much. The floor is yours. Please proceed.

DR. KOVACS: Thank you, sir.

So what we wanted to do today was give you an update on what we're doing, explain what we're doing, but also, since this has been our first public opportunity, just to extend our sympathies to the families of the astronauts and the NASA community. There's not a single day when we go in there that we don't think about that, and that's a big driving force that motivates us.

So what the CAIB KSC team is which you are looking at here, Mark and myself, we are supporting the Board in determining the cause or causes of the disaster, working toward an understanding of the causative events, using analytical techniques. So taking the debris and not just looking at it but looking at the chemistry, looking at the heating patterns and so on. Thinking about scenarios, based on the debris, and fusing that with some telemetry information and other things that we're getting. Sequences of events that may have taken place and then trying to test those scenarios, looking at the debris. We walk out there most of every day, looking at the debris. And summing up and archiving the findings. As we come up with factual findings and opinions, we sum those up for the Board; and then hopefully we'll be able to suggest some preventive measures for the future, based on what we've learned.

You saw this or a slide like this from Steve Altemus. The grid, you can see, is fairly sparsely populated; and so we're very interested in look at parts that aren't on the grid also. You may not know, but along the sides of the building there are what are referred to as bread racks, which contain a lot of pieces that have not yet been positively identified. One of the things we've done recently was ask for some additional support in identifying pieces in critical areas that were sitting off on the side lines, and the support was excellent we received from NASA. Leading edge components of the left and right wings were what we requested help with, and those areas have been populated on the grid much more densely than the average, as a result of that. So we are able to get assistance in filling in what we think are critical areas.

It's very important to note that many of these pieces, debris pieces, are tentatively identified. I want to show you a slide where you can see the orange tags. The orange tags on these pieces mean they're not in their final locations. They're not confirmed. So a lot of the things that you hear, certainly if you walk out on the floor, about this piece being important versus that piece, it's important to bear in mind that some of those relationships may need to be revised.

For example, some of the pieces that were on the left-hand wing reconstruction, after an audit that we all agreed should be done, several of those pieces moved to the right wing. Some of those pieces moved off the grid. We don't know where they're going to be; but the analysis, the identification of these pieces is painstaking. It requires experts. It requires one-to-one blueprints printed out, where the parts are actually laid on the blueprints and argued over for a period of time. And especially with these small fragments, it's difficult. So just so you know, there's an awful lot of energy being put into positively identifying these things. The ones without orange tags are the ones where we all feel very confident in their locations.

So there are three levels, though they're overlapping, to this analysis. One is the large-scale, which is look at the physical debris, its condition, its relationship to other pieces, and try to understand what story it might be trying to tell you. Coming in a little closer, which is Mark's area of expertise -- and he'll talk about this -- is the microscopic and metallurgical. What can you see when you zoom in with a microscope? What can the metals tell you by their characteristics? What's happened to that metal thermally, chemically, and so on? Then the last category is chemical analysis. There's chemistry going on when you have heat and you have gases. This is not happening in a vacuum when these piece are getting hotter, and they got hot. So there's some chemical analyses that may tell us what these components experienced and maybe even the time sequence of what they experienced.

So on the large scale, we have a lot of questions that we're asking what can we learn about temperatures and forces experienced by each debris piece. So we're looking for condition, color, orientation, fractures, and other clues. How do they relate to their initial conditions? We've spent a lot of time crawling through intact Orbiters, taking photographs, asking people for blueprints and what the materials look like so we get a sense for what the baseline should be. So we looked at flown hardware and non-flown hardware, but mostly flown hardware.

A key question when we're looking at these pieces is: with the damage that we see, was it caused by something that happened on ascent, on descent, breakup, or ground intact? A lot of these pieces, you look at them and there's pine needles embedded in them. So clearly that did not happen in space, and we're taking great care to understand the relationships of those issues.

Then how do the pieces relate together physically. There's a lot of jigsaw-puzzling going on out there. We will, as you heard, soon have tables for the tiles where the tile pieces, often which are smaller than a tile and not positively identified, can be put together to look for flow patterns, patterns of damage, and orientation. I think a lot of those orange tags are going to start to go away when we get to that point, but it is like putting together a multi-thousand-piece 3-D jigsaw puzzle on a 2-D surface.

Also a very important aspect of this physical analysis is comparing pieces on left to right. If you're saying, well, something happened here on the left wing, well, what happened on the right wing. If we have a comparable piece, we'll definitely take a look at it. So we spend a lot of time walking both halves of the vehicle, making comparisons. That's how we spend a lot of our days.

What you see here is an example of one section of the vehicle that you've heard a lot about, and it made sense for us to show our perspective on this. Here is a frame of video from the enhanced ascent video showing this little white spot in the red circle, which is something impacting what looks like the leading edge of the left wing. This was provided by Scott Hubbard. This is the enhanced stuff -- and I don't know the history, but I'm sure Scott will -- from the downrange camera.

What you see here is a section of the Shuttle locater, which is a very handy document that we have electronically where we can look up particular sections of the vehicle and diagram out parts. So this is the leading edge of the wing people talk about, and these sections here that you see are the reinforced carbon-carbon or carbon composite, which is the high-temperature-bearing leading edge of the wing where I'm going to show you the physical debris that we have.

So this is a closeout photo of the actual Orbiter prior to flight. This is STS-107, and these pieces here are the leading edge reinforced carbon-carbon pieces. The area that you see in that video frame of the impact is somewhere around here. I don't think we can be much more specific than that, and the landing gear door and the piece that people seem to spend a lot of time looking at all come from this region. So what you see physically on the floor is that.

So there's very little. That's the first thing I always find quite striking, very little of it there; but it's getting filled in here very quickly because of this added effort. The added effort is this section here. This was not there until maybe a week ago, and these are the bread racks where all these pieces are. It's hard to get a perspective, but there's maybe 200 pieces of RCC, this reinforced carbon-carbon, the leading edge, that are not yet on the grid. They're right here because they're so small that you can't get the curvature, you can't get a serial number off of them, you can't just look at it and say, oh, that's Panel 5. So that's where we place them. The emphasis on this table here is where the puzzle gets put together; and what you see there, this white thing, is a 1-to-1 full-scale blueprint where these pieces are laid very carefully to try to understand where they are. So what you're looking at there is the leading edge of the left wing now.

It's been confusing to some people. This is the bottom of the wing. You see here some of the landing gear. There's the tire, and what you're seeing here is the bottom edge of the leading edge and here's the top and it's something that looks sort of like an arch that's been split. And we're looking inside it and you see some of these parts have some interesting features I'd like to show you next.

ADM. GEHMAN: Can you go back one, just to make sure our orientation is right? Based on the debris reconstruction layout that we saw in the last panel, just off of the viewgraph to the left up there would be the tile. That would be the bottom.

DR. KOVACS: Yes, sir.

ADM. GEHMAN: So what we should be looking at here is kind of the inside of the wing, the structure of the wing, if there were any.

MR. KOVACS: Yes, sir. This is the structural section. Some of these structural pieces do have tile fragments or tiles on them, but up here -- and I'll show you in a later slide -- is the tile region of the wing.

ADM. GEHMAN: But the point of my question is that it is blank. There's nothing there.

DR. KOVACS: It is blank, sir. Yes.

ADM. GEHMAN: Which is all the aluminum structure. The struts, the spars, and all that kind of stuff is not there.

DR. KOVACS: Not there. That's correct.

So here is an example of what we've been doing. You need to get calibrated first. So here is an intact RCC panel. That's what they look like on the vehicle. Now, it's important to note that *Discovery*, *Endeavour*, and *Atlantis* don't have the same structure in the leading edge at the detail level that *Columbia* did. So the construction is somewhat different. So we're very aware of that when we do these inspections.

You can see here a flown piece of RCC. It's discolored. They start out darker. This is okay. This is normal. What we've done is inspect the surface of this at a distance and a close-up, and then we look at the pieces that we're actually trying to match. Here's a piece from the left wing. You see the process. We're trying to fit the pieces together. This one's obvious.

This is not normal. What's inside here is sprayed-on material, and people have called it various things. Slag. Slag is probably not the right term, but it's oxidized metal and metal components, inorganic. That's what our analyses are showing. I'll come back to the analyses later, but this is a very important thing is to fit these pieces together and we're now marking the intersection, the fracture line so it's very obvious without people picking them up and trying to thrust them together.

Here's a typical piece where we get interested. I want to show you how this relates to the recovery efforts where we look on the maps. Here's a piece that has not been finally identified, and I marked that on the slide because a lot of people may jump to conclusions. It's not quite that yet, but here we see some significant erosion. It's eroded through very many layers of the composite material, and you see a gradient. So out here at the edge what you can't see too well looks fairly normal. So this piece probably saw a lot of abuse for a long time, relative to some of the other pieces.

There's two here that can be mated together. That's the kind of identification we're doing. But in relationship to the ground efforts, recovery efforts, we say, okay, where do these pieces come from. Mark put this together using data from Jon Cipolletti, a person involved in the recovery effort. You see that right there. You see on this chart is left wing reinforced carbon-carbon and right wing. So clearly there's some pattern here.

Interestingly, that very eroded piece is right up here in the very most westerly pieces found. So that's the kind of thing that we're doing to understand the relationship of, state of a debris piece and its location and then perhaps the time sequence. So those are the kind of things we see on a daily basis.

Another thing that I mentioned we do a lot of is comparing. Our interpretations are not meaningful unless we're also looking at the debris on the opposite side of the vehicle. So you can see here the left wing and the right wing. It also gives me a chance to point out some of the pieces like these used to be over here but after the audit got moved to the right-hand side. But we really need to, on a daily basis, walk both sides. And we do that.

The tile area, Admiral, that you referred to earlier is shown here for the left wing. And I don't know if you can see this very well in the audience, but there's an awful lot of orange tags. Those indicate tiles that are not necessarily in their final positions. So we look at those. A lot of them are in the right distance along the wing but their actual exact locations are not clear. And the thing that we need to point out here is that these tubs are much larger than the tiles. So if you took the tubs away, you'd say, boy, we don't have very many tiles on the grid. And when we get those tile tables in there, we'll be able to put the tiles in, we'll be able to take pictures of tiles that are on the fuselage pieces and put those pictures in place and actually make a mosaic and be able to identify an awful lot more tiles at that point. I think the take-home message here is the tile areas are quite tentative. However, there's a lot of information there.

Here's a slide. I went around and shot four tiles that we have been looking at and oriented them in the direction of flow, as best I could. So the flow is from this corner off the screen. So those tiles, while they don't have their serial numbers on them anymore, the erosion or the coloration patterns will tell you something about the flow; and this is just an example of four of the kinds of things we see. None of these things are unique.

This tile, except for these chips, which may have happened on ground impact, looks pretty good. That's what a tile looks like.

This tile here has a pattern where you see it's nice and white; and if you look closely, you see what looked like a glassy glaze, a clear, glassy glaze. We looked at tiles from some previous flights where known impacts were documented, and they look like that. They're white. They have a glassy, beady coating inside. Some of them have what people describe as worm holes where turbulence, they say, may have eroded these holes deeper.

These two tiles -- and this is off STS-107. So this is also off STS-107. You see they're darkened and in the analyses done to date -- so I say this in a preliminary way -- to date, the darkened material is not soot, as some people have referred to it, but it's burned aluminum, largely. So the black is the metal that's been reacted with oxygen and maybe some nitrogen embedded in there.

This piece, there's a semicircular gouge and it went all the way through the bottom of the tile and it's likely that, if you think about the impact angle here, this tile adjacent to it must have been gone at that point. There doesn't seem any other way to get that kind of impact.

This one, this tile's been eroded all the way through. If you look at these lines in the flow pattern, whatever happened to that tile, it happened over a long enough time that these patterns could get set in. Just as a point of reference, for these tiles to melt, you need to get them to 3500 degrees Fahrenheit. So that gives you a little clue also of what was going on at this point.

I'm going to turn it over to Mark, who's going to talk about what we're doing at the microscopic and metallurgical levels.

MR. TANNER: Greg kind of gave us a large-scale overview. We're also now focusing on what I call the close-up, microscopic view. This is where we actually go in and look more carefully at the individual part.

What we're trying to see first non-destructively is, as he said, he talked a lot about deposits, what are we seeing in places that aren't supposed to be there, what are we seeing on the RCC panel that's not there. We see a lot of splatter. We see a lot of different types of splatter. We see deposits that we can't identify, and we don't know where they come from. We see fractures. We see an old fracture. We see new fractures. We see some erosion.

Again, these are all the little details that we're focusing in on, trying to identify and hopefully tell us the story to bring back to an origin area. With the deposits, a lot of times there's a flow pattern, a splatter pattern that will tell us potentially a direction. We see the fittings that are made out of different alloys melting or having parts of it melted; and in cases, again, sometimes we can determine things. We'll have some examples in a minute on that.

What we're trying to do from that is to get an idea from the flow patterns, from the splatter patterns, and from the deposits, you know, potentially where was the breach, where was the origin that this whole thing started from -- or potentially origins. We're not sure. Again, we're going to let the data tell us the story; but with the identification of that, I hope it will point us back.

We also can look at other things, too. The metallurgy is going to be important. As mentioned already, aluminum is the primary component; and we see very little aluminum. Fortunately, the fittings that were used in the RCC are made out of materials that have higher melting points. So in those cases we do have some of those. We can look at the microstructure, look at the hardness and, again, because of the temperature that it's seen, it's going to change. Hopefully, we'll be able to eventually create almost a heat map of where we think the hottest area on the wing was and where does that point to or potentially another location.

Most importantly, we're going to be comparing what we see on the left wing to the right wing. Very early on, when some of the parts weren't identified, it seemed to be showing a pretty good pattern that all the deposits that we were seeing were on the left wing; but when they went and did their audit, all of a sudden we found one panel that had pretty heavy deposits on the right wing. Does that give us an indication of re-entry things that we would see on re-entry? So we're very carefully looking close up at all the comparisons of all the parts to try to identify what will it tell us in the story.

What I would like to do is give examples in the next few slides. We're going to start with the RCC. Our focus has been on the leading edge. We saw a picture of it, but in this case I wanted to show some of the alloys we're talking about. What we see here in the purple is the reinforced carbon-carbon; and then what we have in the red, this is an A2D6 material. Then we have some more fittings that are Inconel 718. Those all have been to melting point. So what we see again points to temperature gradients.

Then we have what's actually called the carrier panels. These are some of the tiles and different density tiles. That's attached to the aluminum wall.

Here's an example of a spar fitting. This connects an RCC panel, the top to the bottom. What we can see from this when we look at it is we have an area that's been fractured. The RCC is gone. Again, we have another area where it's been fractured; but what's more important about this one is we can actually focus in. This is pretty much intact. We have a little region, if we focus up here, where we had melting. Well, Inconel 718 melts at approximately 2460 degrees. Plus, it also shows us a pattern of the melt. The melt's being pushed over, basically out of the screen towards us. So that will give us an idea of the direction of flow.

Another example where we looking at the alloys, I think Admiral Gehman mentioned the uplock. On the landing gear door, we have four uplocks that help hold the door into place. Well, we can see here we have one in the forward and we have three along the side. We're not sure yet which uplock this is, but this is the only uplock that we have from the left landing gear door. And if we look at it more carefully, one, you can see we have a splatter pattern. That has been analyzed. Right now, basically it's a 2000 Series aluminum. The vessel has 2024 aluminum in it. So it's likely that is part of the aluminum splatter, but you have a small amount on this side and you have a large amount on this side. That's again going to tell us a direction of flow. But if we focus in a little more and we look down at the edge, we can see that we have a localized area that is melted.

Now, this is made out of titanium. So now we're talking approximately 3,000 degrees. So when we starting looking at what type of damage we see in melting, just from our visual observations so far without any destructive analysis, we can start, hopefully, zeroing in on the hottest points. When we finally come to a conclusion, our story has to jibe with why this wasn't melting on the landing gear door when our focus right now is on the left wing.

ADM. GEHMAN: Do either of you know where on the ground this was found?

MR. TANNER: I don't.

DR. KOVACS: Not offhand.

ADM. GEHMAN: Do we know?

MR. HUBBARD: That's in the data base.

ADM. GEHMAN: We'll find out.

MR. TANNER: The next two slides, I just want to show some of the types of damage that we're finding on the RCC, the reinforced carbon-carbon. If you look at the top-left corner, you basically have what I call an impact damage. Typically, if you see a beebee hit your window, it hits on one side and makes a little ding that pops out a plug on the other side. Well, that's what we see here. This one's a pretty fresh fracture. You see the silicon carbide layer, which is the light gray, and then the carbon matrix underneath.

Here we have a panel that's been fractured. So now we're looking at the thickness; and again, we have our silicon carbide layer on the outside and the nice carbon matrix in the middle. But as you look at these fractures and you start looking at the things they're telling, we look around the fractures, these three are all from the same piece, but eventually we found one area of the fracture where now we don't see the standard pattern. We see actually where we see some heat. This thing has actually had a little erosion and some oxidation occurring. So that's telling us, again, an important piece of information as we try to put all of these pieces back together. We'll eventually be able to map out a fracture map and heat patterns again of where the penetration occurred.

Here's just a little hole that we found on one of the panels. It's fairly oxidized. It's seen a lot of heat. The interesting thing about this hole is this is on the external surface. I don't have a picture to show you right now; but on the internal surface, it looks like a screw with a washer had hit it because there was actually a buildup of slag or the molten metal around it and at some point in time that bolt left and, when it did, it allowed the heat to create some damage there in that one location area. Again, we don't know where this panel comes from. It's another important piece of information as we're trying to let the debris tell us a story.

When we're looking at the RCC panels, again, there's other damage that we can see. In this case we have a fracture that's been coated. Well, for that to be coated, it needs to have broken early on to allow the coating to occur. Now, was it real early or was this part of cloud they talked about, post-breakup, where you potentially had a lot of the molten metal? Again, we'll be trying to determine that.

Here's another example of some heavy erosion still on another RCC panel. In this case we have a crack there. Is this important or not? Well, we'll be investigating more carefully, again, and trying to put it all together.

The RCC panel that Greg showed a few minutes ago, the one that was heavily eroded, here's an example on the side. In this picture I don't know if it shows the color very good, but we've got erosion going through multiple layers again and kind of gives a direction of flow. We also have some deposits that are there. We'll want to identify those deposits. They'll potentially allow to us backtrack to where, again, the flow is coming from.

Then this last one on the panels we see is a crack. This is on the external surface, and there's some erosion in that. So we have to determine was this something that was occurring after the breakup and was on re-entry or was this potentially something early on. The erosion pattern right now would indicate it was probably later on; but, again, we are going to be focusing on our actual analysis and fractographic analysis looking at things like this.

What this picture is is basically a close-up of the RCC panel from the right wing where we -- I think earlier Greg showed you the picture with all the deposits and slag that were on the panel. Well, you can see this one is pretty clear. You can almost see the crisscross pattern of the carbon matrix and then with the silicon carbide layer over it. The reason I wanted to show you that is this is a nice, clean panel; but we found roughly 16 or 17 different types of deposits. So we're going to go through just a couple of slides showing you some right now.

We don't have a clean panel anymore. We have an area where apparently we have some sort of metal splatter. We will be identifying what is that metal. Again, we talked earlier about the visual, looking at the flow of the pattern. Now we're trying to find out what is the metal splatter.

Here we have a nice, almost peacock-colored one. What is that? What alloy created that?

Again, another heavy deposit. A little rainbow effect.

In the bottom right, we have something almost like a volcanic rock; and this one, the picture doesn't do a good job. It's real glossy. We'll have to analyze that. Is this part of a tile that was molten that came into the panel?

Then when we even continue to get more microscopic, one of the things we noticed when we took this close-up photograph is this material's probably part of the insulation. We're not sure until it's identified, but we notice that there were some little blue spheres. Is that normal? Is that something that was produced by melting? If it was, can we identify what melted and get an idea of the temperament? So we're trying to take all the data and help us focus on the temperatures to create a temperature map.

And when we step over, we've identified like over 16 different deposits but there will be additional deposits we'll be wanting to analyze. We've got some areas of chemical analysis we want to do, and Greg's going to talk about that.

DR. KOVACS: So this, again, back to the big picture, is how do you use the small-scale chemistry to understand what happened over an entire wing or entire vehicle. So the starting point is what elements are present in surface deposits, and we've done some good work so far with the NASA lab. They've done the good work. There's a lot of people supporting us in this, and we want to thank them. We looked at a lot of the deposits, and you can say these elements are consistent with the pieces of metal that were in the leading edge. So things burned away in the leading edge and deposited somewhere.

One of the questions, though, that is more higher level is: Are these deposited materials just melted Shuttle components, or have they reacted with the atmosphere? So when you're talking about these low earth orbit operations that the Shuttle takes part in, they are not in a total vacuum. There are a lot of gases there. They're, of course, at much lower pressures and concentrations than on the earth, but they're there and when things get hot, they can react.

So the high-level goal is can we say what altitude, what temperature did these things react. So we have oxygen and nitrogen; but as you go up in altitude, they don't exist in the normal forms we are familiar with -- two oxygen atoms together, two nitrogen atoms together -- they're dissociated, and they're highly reactive. So they get hot in an atmosphere where there's mono-atomic single-atom oxygen. Things react very quickly and they react very differently than they do with the atmospheric normal type of oxygen, the two-atom oxygen.

So this is not the best chart in the world, but it shows the relationship of the gases in the atmosphere as you go up in altitude. The two lines I've added to it, this blue line here indicates the typical altitude. This is in kilometers that the Shuttle operates at. It's just a rough cut for the time being.

Down here we have what is defined as atmospheric interface or atmospheric entry. We start to enter and start to become aerodynamic versus being in space. What we're doing is looking at these things. Here's the mono-atomic oxygen. The single atoms, the very, very highly reactive oxygen. Its ratio relative to these other gases as you decrease in altitude changes. So when we're getting down here, the dominant gas piece is nitrogen, ordinary nitrogen. So what we're asking is: Can you tell what altitude these things were hot at by the chemistry? So we're looking at the chemistry of the reactants, not the metals per se, what reacted with those metals.

The other thing you can ask is if you have a wing and there's a hole in it, hypothetically, and stuff is coming in from an atmosphere that is rarefied and there's not a lot of reactant species there, if it's hot, you would think that these reactants get used up at some points during their path down the wing. So we may find when we do the analysis that, for example, the oxygen is concentrated near where an entry point occurred and, further down this path of flowing hot gas, there's less oxygen. So the metal deposits there may be less oxidized.

So what we're trying to do is construct a map based on the chemistry and ask that question: Where, if there was breach, was the breach, based on that chemistry? So the way we're doing that is sampling multiple points on each debris piece.

What you can see here is four sampling points on one piece. This happens to be on the right wing, but we have to do right versus left if these analyses will mean anything. We've taken those and said what elements are there, from the bottom of the sample to the top. That's an important point. If you peel off a little, tiny piece of the sprayed-on material, it's like tree rings; there's a history there. The bottom of the piece is where the first deposit was. The top of the piece is the last deposit or the last remaining piece. So we're interested in that gradient also, when the first little bit of that stuff hit the wing, what was going on. 'Cause that will tell us much closer to the causal events, we think.

So to sum up, we're supporting the Board in trying to determine the cause of the disaster but with a focus on debris that we're very carefully listening to the information coming from the telemetry and, for example, the OEX box when that becomes available. We're doing this analysis on these three levels -- large-scale, microscopic, metallurgical and chemical -- and we're trying to fuse them into a comprehensive overview. Anyway, as I said, we're using a lot of other input like debris recovery locations and sensor imagery data; and that's how we're trying to get an overall picture.

Thank you. That's all we have.

ADM. GEHMAN: Thank you very much. I'll ask the first question here, of which I've got a whole page full. Let's start kind of at the macro level and work our way down to the micro level. Would you make just a subjective evaluation based on your many, many walks from the left wing to the right wing and the right wing back to the left wing, just what the left wing debris looks like that's different from the right wing? Not through a microscope, just from a person standing there, looking at it.

DR. KOVACS: Well, I could say that as we walk the two wings, on the left-hand side there are many more pieces that are coated with deposited material in a region near these RCC Panels 7, 8, 9. We don't have any of 6 at the moment. You see a lot more deposits. You see deposits that are different in character along the wing -- for example, white versus darker and in a gradient, not a mixture, not a patchwork but actually one panel is much lighter than the other one, suggesting maybe one saw more heat than the other. And that's about it.

If you look at the right wing and you look at those panels, they're pretty clean. There's maybe one that has deposit on it, but otherwise they're pretty clean.

ADM. GEHMAN: From the picture and from my recollection, would you agree that on the right wing there are actually pieces of aluminum, whereas on the left wing there's essentially no aluminum?

DR. KOVACS: Well, in general, I would agree, sir. I think on the left wing there are some aluminum pieces. For example, on the wing glove there is honeycomb left, which the forward Panels 1 and 2 and the glove don't look like they saw the same amount of heat as the back panel; and further back there are some pieces of honeycomb left. But in general the honeycomb is gone. This is the aluminum. It literally does look like honeycomb cells. It's mostly nothing, and it's something that might very well burn up quickly.

ADM. GEHMAN: A question for Mr. Tanner. It's my understanding that the qualities of the different materials that the Orbiter was made out of, since they all have different melting points, that possibly we can determine that the metal, the titanium, the CRES, the Inconel, the aluminum, of course, were all witnesses and they all fail at different temperatures. I call them witnesses because they were there and they saw what happened. Is there a possibility that the temperature differences have a chance of telling us something?

MR. TANNER: Oh, that's definitely my hope. As we find more of the spar fittings and as they are getting put on the grid and audited and their location, the sampling plan will be eventually to look at that; and hopefully that will give us the information that we need to try to say this part of the leading edge saw the hottest heat. Then as we work our way down, it was cooler. But one of the things we have to do very carefully is look at the right wing, too, because early on when we were working, we saw all of the deposits primarily on the left wing. Then all of a sudden one got moved over and we've seen some molten metal over there, too. So we have to calibrate ourselves, but I do believe that will tell us a strong story. We don't have a lot of the spar fittings on the left wing.

MR. HUBBARD: Two questions. One, is it still true today that we don't have any pieces from Panel 6 of this RCC material?

DR. KOVACS: There are certainly no major pieces as of yesterday that are placed on the grid. Pieces on the shelf? There may be some that are being worked up for Panel 6.

MR. TANNER: As I think Greg mentioned earlier, there are a couple of hundred small pieces of the RCC panel; and this could be like putting together a solid gray puzzle. What complicates this is that in some cases the fractures that would mate normally very well have been eroded away. So it is going to be a time-consuming process as they put that together. Plus, in some cases where one broke off, there's actually a perfect match and one side had a deposit mark and the other side didn't but they match perfectly and you don't see a pattern across the two. So it's going to be a complicated puzzle, but there are a lot of pieces there and if they're left wing, they're going to be able to tell us a lot of information eventually.

MR. HUBBARD: Can you just summarize for us, either of you, what rules of thumb you carry around in your head to begin to evaluate what happened pre-breakup and what happened post-breakup? Are there any patterns that are emerging?

MR. TANNER: I guess probably what I'm looking at right now is the RCC. It seems to show much more of the heat. The heat patterns that we're seeing from the right side RCC to the left side, I see more erosion. I see more degradation of the layers. When you actually start getting into the carbon matrix, it appears to be on some of the pieces I'm looking on the left wing. So when I'm looking at the RCC, for example, sometimes we have worked with the NASA guys and said, "Hey, can you find this piece for me 'cause I think it's an important one?" And they'll go and spend the time and effort to try to get it and find it and, sure enough, it appears on the left wing. So we're seeing some of that as a rule of thumb. Sometimes the splatter patterns can do it, but you can be fooled by that, as far as the deposits, like we saw on that one Panel 8 on the right wing.

DR. KOVACS: I'd say that depending on what you're looking at, there could be more than rules of thumb. I think I have rules for every finger. For example, with tiles, you have to ask yourself did this tile fall attached to a piece of aluminum, as some of them did, in which case its ballistics would have been very different from a tile falling by itself or attached to something heavy. The elevons, for example, are big, heavy objects that could have reentered at much higher speed than a tile that just fell off.

So you look at the tile and you ask how they fell and you look at the erosion and ask how long it would have had to sit at 3500 degrees or higher to get that kind of pattern. So we try and replay in our minds some hypothetical scenarios for what these things went through; but there are many different rules of thumb we would apply, depending on the types of component we're looking at.

MR. WALLACE: We're looking at this investigation from debris and thermal flow and aerodynamic flow and sensor data. Are you optimistic -- we saw a demonstration earlier today of the 3-D computer reconstruction. What's your sort of outlook on how you think that will work out in terms of even being able to add in things that you learn from your microscopic and metallurgical, chemical? Are some of the things you learn there going to be able to fit into that display?

DR. KOVACS: Yes, I think that the 3-D reconstruction, if it's on the computer, that is, if it's done with enough density that we can actually place our sampling points on it and try to visualize flows, I think it's going to be very useful. We do a lot of crawling underneath things, holding things up and trying to visualize where they were on the Orbiter. With a tool like that, you can do it all day long without damaging components; and we're very careful about handling the components too much because some of them are friable, fragile, and we don't want to damage them. I think that's going to be a very, very powerful tool for visualizing things.

MR. TANNER: Specifically on the leading edge, there we're going to find more of the parts. When we start getting to the main structure, so much of that is twisted and turned. So when they go in and do the laser analysis, somebody's going to have to decide how to straighten that out. At least the RCC is keeping its shape; and so that, I think, is going to give us a lot of information.

MR. WALLACE: What is the plan on that? The pieces we saw were like RCC where they weren't twisted so you could put them back; but if you have something that was straight that was turned into a pretzel, what's the plan on that, if you know?

DR. KOVACS: I don't know, but what we've seen so far is very preliminary. I don't know how this will get scaled up when it gets big time, when they start scanning in large pieces. They've been scanning some smaller pieces and some intermediate size, but there are a couple -- for example, one piece of fuselage that's bent at 90 degrees. That's a good question how to either straighten that out or segment it.

MR. WALLACE: Straighten it out physically or --

DR. KOVACS: Straighten it out software-wise.

MR. WALLACE: Also another question. Could you tell a little bit about do you have a feedback process to the people who were just up before you -- in other words, your wish list to the debris collection part?

DR. KOVACS: Well, certainly as we look at these relationships between debris pieces and maps, you might say, gee, could you go back and take another look at this area where we found a highly eroded piece of RCC that might be interesting. That capability for us is just coming on line, and it's not realtime. So I think we will have more feedback.

MR. TANNER: For example, one of things we thought might be very helpful, once they've got all the RCC that they, like I say, kind of cherry-pick it, they look at the thickness and look at the shape and know where it goes, and now we're getting to the smaller pieces -- one thing that might help them zero in is seeing where those pieces fell. So we're telling them that, you know, if this fell near Panel 8 and 9, then you maybe want to start there when you try to mate up the pieces first. So we're working with them, and they've been very helpful as soon as we start getting access to the data.

MR. WALLACE: Thank you.

GEN. HESS: I was just wondering. If we back up again to the macro view, there's been a lot of concentration on the left wing, the leading edge, and the bottom surfaces, and comments about metal not being there. First, has there been any of the blanket that's been recovered; and are we seeing any signs of heating along the main body line, the vertical tail, and areas like that that are adjacent to the left side?

MR. TANNER: Are you talking about the top surface?

GEN. HESS: Well, the top surface and the vertical tail piece. Are you seeing any transition of heat down that way?

DR. KOVACS: There seem to be bits of it around. I don't know whether any of it has really been put out on the grid except for a few little pieces.

MR. TANNER: Yeah. And most of them, if they have, they haven't been positively identified. Again, it has to do with where the identification markings were and if that was on the piece they had.

GEN. HESS: But in the area of the vertical tail and stuff like that, it also has the tiles that project to the leading edge of that. Are you showing signs of heat in that particular area at all?

DR. KOVACS: The vertical stabilizer, I wish I could show that slide again, is empty space. There are almost no pieces of that region of the Orbiter, and it may that be that section hasn't been searched yet. That's one of the things we keep in mind is we say, well, we don't have much of something. We can't yet go realtime and click on the grid and say have they searched where those few pieces came from all the way. So we don't know. Definitely if you look at the vertical stabilizer, that's where there's a lot of room to walk around.

GEN. HESS: With the level of analysis that you've done right now, do you have an estimate where you think the max heating of any piece may have been?

MR. TANNER: The max heat of any particular piece?

GEN. HESS: Yes, the highest heat.

MR. TANNER: Well, it's over 3,000 because we've seen some of the RCC that have been heated up to a point where -- or, I'm sorry, the tiles would be 3500 plus. So some of those have been melted. So that would be the highest.

DR. KOVACS: That's based on observation and knowing the melting points. When we get into the chemistry, we may have more precise max heat numbers.

DR. OSHEROFF: I was just struck, when I was looking at the layout on the floor in the hangar, that you have almost a complete right-wing landing gear door but there's not much other lower skin of the right wing or anything else. Can you speculate as to why that piece is in such good shape?

MR. TANNER: I guess when you do look at the right wing, what you'll see is I think we see a lot more of the skin on the right wing, aluminum skin, both the top and the bottom, compared to the left wing, in some cases almost an order of magnitude difference in the skin. So based on that information, I would say that side saw less heat even on the re-entry and breakup. So therefore the aluminum, which is the main structure of that, survived, where it seems like the heat was hitting our left wing area and so therefore the door that we're seeing on the left wing, we've basically found but one little small piece.

DR. OSHEROFF: So presumably I guess it was protected by the thermal panels and didn't break up until much later or something like that.

Certainly the 3-D reconstruction, computer reconstruction, seems like a really wonderful resource to have. Can you suggest other resources that you feel would be useful in this very daunting task that you have?

DR. KOVACS: Well, I think, first of all, we're very grateful for the resources that we do have. I think a tool that would be very helpful would be some way to, in real-time, ask about a part when you're standing over it, what other parts were found near it, where was it found. You can imagine a lot of things like a wireless tablet PC where you call up into the data base, but that's easier said than done. But some tool where it doesn't take a half a day to figure out where the part was for us. I know others have work stations that are used, but I think coordinating that sort of thing into a unified format that we can use on the floor while walking around would be very useful.

DR. OSHEROFF: Thank you.

ADM. GEHMAN: What can you tell me about your ability to determine what I will call here "preexisting conditions" -- that is, pre-accident conditions? For example, could you determine if a piece of RCC had been struck by an external object prior to heating or could you determine if a tile -- well, obviously I'm getting back to the left wing being struck by something -- or corrosion, for example. Could you have determined if there was corrosion present pre-accident? Can you just describe have you seen anything like that, are you on the lookout for it, and what your ability to determine what I would call a preexisting fault, a preexisting condition?

DR. KOVACS: From a hypothetical standpoint, you could imagine -- and I don't think we're there yet but you could imagine looking at a crack or other impact mark that saw heat first or later. So I guess what I'm trying to say is if there's a crack there and then it got hot, it would look very different from a crack that occurred when this piece impacted the ground and split in two.

I think there are some clues there. We don't have enough pieces out there and I don't think we're quite ready to say anything like that that we could say with surety; but on the metal side, corrosion implies chemistry. There may be some ways to dig down into the metal. Certainly at a microscopic scale if there's corrosion, cracking or anything like that, it's definitely a possibility; but the chemical analysis is just beginning. But that's a very useful question to guide a chemical analysis.

MR. TANNER: I think also we'll be looking at the tiles. We have been visually looking at them, but we'll look at them much more carefully. I think early on we took one tile and had a sample removed because we thought it might be potentially embedded foam, for example. So we're going to be looking for any potential damage, not just the metal, the RCC, but also the tiles.

ADM. GEHMAN: Does your button over there make your slide presentation go backwards?

DR. KOVACS: Yes, sir.

ADM. GEHMAN: Could you go back to the uplink roller?

DR. KOVACS: Yes. We have to scroll through a few slides here, but we can get there.

ADM. GEHMAN: There we go. That's a good one. Looking at the top corner of that, which is in the lower left-hand corner here, the corresponding other ear over there or whatever you call it on the other side doesn't show any of the -- no, on the back side.

Here. Go back to the one where you were before. This part here is the other ear or the other flange.

DR. KOVACS: Okay.

ADM. GEHMAN: In other words, this flange has been eaten away but the corresponding one on the other side shows no -- of this torching, if at all.

DR. KOVACS: Yes, sir.

ADM. GEHMAN: Now, how do you attribute that?

DR. KOVACS: Certainly something impinged on this.

ADM. GEHMAN: Well, they're only like two inches apart.

MR. TANNER: I think one of the complicating things we're trying to figure out right now is, when we look at the debris, it's just almost like after a tornado. Sometimes you'll say: How did this thing survive? When we're looking at this debris, we see a lot of damage from the post-breakup to the heating damage. Every once in a while we'll see something that doesn't make sense. Now, potentially there may have been a directed flow where, again, potentially some of these panels may have broken and come in and then acted like a shield -- redirected some flow. But right now it's just speculation. It could have been a very directed, pointed flow to do that.

ADM. GEHMAN: A very directed pointed flow. But also if you assume that the door was closed and the uplock roller was in its locked position, then the hook, the latch is in between.

MR. TANNER: Yes. You're exactly right.

ADM. GEHMAN: In other words, whatever the latch looks like, it's in between the two ears. So the idea being, then -- I assume -- that this is like a signpost here in that it kind of tells you the directionality of the heat flow. Of course, now we've got to figure out which of the four this was.

MR. TANNER: Yes.

ADM. GEHMAN: But this is made out of titanium, as I understand. So whatever heat flow was doing that damage, it was not a trivial matter.

MR. TANNER: No.

ADM. GEHMAN: That's why I asked do we know where on the ground this was found, because that will be illustrative. If you assume that this heat damage was done to one ear but not the other because the latch was latched, then I guess we can assume that the door was closed. That might be a stretch because it could have been pulled out of the door. But if you go back to the next one -- can you make it go back one more time?

The fact that the bottom -- the fact that this is the part -- as I understand it, this is the part which fits inside the door. So that's all bright and shiny, all 360 degrees all the way around. So I assume this was in the door for most -- whatever this assault was that sprayed metal here, ate this away, it looks to me like this part was protected-- see, here it is right down here. It's in the door.

DR. KOVACS: Yes.

ADM. GEHMAN: The instruments which all registered heat are all up in here. These things are all on the bottom. But if we can determine which of these we're talking about, we begin to get some directionality of this flow. What I'm saying, though, is that this could not have been caused by radiation heating or something that was 4 feet away. This thing here was impacted upon, a direct heat flow of some significant magnitude.

MR. TANNER: As a matter of fact, there's also a point, if we look at all four sides, which you can't see it from this photograph, you can see another hot spot that started on the uplink but it hadn't started melting it yet, by just the heat tinning. So it was another area of impingement which again could give us an idea of the flow.

DR. OSHEROFF: Pursuant to the explanation that, in fact, there was a latch that was in place that protected the flange on the other side, what was that latch made of? Is it aluminum, or was it titanium?

DR. KOVACS: I believe they're steel.

MR. TANNER: I'm not sure on that.

ADM. GEHMAN: You're absolutely right; we'll have to find that out.

MR. HUBBARD: One of the things that we're touching on here, of course, and was mentioned a little bit earlier is that we have to pull the threads from a whole number of different lines of investigation together, the aerodynamics, the aerothermodynamics, and so forth. Are you seeing from your place there on the debris floor a connection? That is to say, are you getting people who are doing the calculations to see what it would take to make such a directed plume flow to come and observe these materials? Are the analysts and the hardware people talking to each other?

DR. KOVACS: To some degree. I think more would always be better. I think one of the questions that we've asked and is still pending is vehicle orientation over time. When we're interpreting these flow patterns, we don't really know that the flow is coming from forward to aft. So that's not just the analysis guys but the general question that we have. Yes, we've had a few people come out who have been doing the modeling. It's been very productive. I would like to encourage more of that if it's possible.

MR. HUBBARD: In this particular case, what hope do we have of learning which one of these four positions it actually occupied?

MR. TANNER: I know they've been investigating some of the closeout photos, trying to look where there was an orientation; but at this point in time, they're leaning towards one but they're not feeling too confident about it. So I would rather not say.

MR. HUBBARD: What's the method of identification?

MR. TANNER: It's just trying to look at the way the pin fits in. There's actually a little slot up at the top. There's actually a little play up here in this hole. So they're trying to look at the orientation of that pin.

MR. HUBBARD: So it's minute differences in what are essentially four identical pieces of hardware.

MR. TANNER: Exactly.

GEN. DEAL: I would like to go back to one of the bullets you had on one of your earlier slides since you two have probably had more hands-on time with the pieces than any of us have. You said you were going to suggest preventive measures for the future. As you've been going through all of this, our previous analyst would like to have seen every piece stamped with some type of identifier, you know, like you do in mass-produced aircraft. But besides that, is there anything that you have that are surprises that you've run across regarding Shuttle construction? It could be anything from, when they designed it 30-plus years ago, what the heck were they thinking, or maybe something incredibly astute that was ahead of their time. Anything that stood out in your minds so far?

DR. KOVACS: Well, certainly it's a design that reflects the era in which it was done; but it's a state-of-the-art design, certainly. The one thing that has been a topic of some discussion was the OEX was really a vestigial device from the early flights, and there is no real black-box recorder as a standard piece of equipment. That's one thing that I think would be invaluable, to have sensors that are routed -- connections routed differently than the main sensors so if you have a sensor cable that is severed or burned through, you don't lose that. And a box that has its own power -- I understand the OEX box did rely on external power -- maybe with some more robust recording capability. That, I think, would be a retrofit if there was an intent to do so. That's, to me, the most striking thing.

GEN. DEAL: Nothing regarding structure itself?

MR. TANNER: Well, I think one of the things, the subsequent structures to the *Columbia*, as far as the spar fittings where they attached the RCC, they went to a titanium. So therefore you've got an alloy that can handle the higher temperatures. So that was something that was a plus, but they don't have that on the *Columbia*.

MR. WALLACE: If I could follow up on General Deal's question to Dr. Kovacs, are you suggesting a crash-worthy flight recorder be incorporated; or are you suggesting that that additional data be telemetried down to earth?

MR. TANNER: I was actually suggesting, not hoping that we would ever have to deal with it, but a black box of the FAA type, commercial aircraft type be incorporated. You know, we crawled around in there and looked at the connection points for the sensors, the way they're routed to the OEX recorder, and many of the same cable routes are shared.

MR. WALLACE: I hear two different issues, the one being the shared cable routes and the other being how you get the data back to earth.

DR. KOVACS: Right. I was thinking of something that was robust and hardened so that it would be definitely recoverable regardless of telemetry.

ADM. TURCOTTE: You've described a couple of things as you work your way around the debris analysis in the hangar and you've described some challenges that you have with orientation and handling the debris. In a perfect world, would you have any recommendations for perhaps reorienting the way that we're looking at the debris in the hangar now?

DR. KOVACS: Well, let me start, then. I think there's been a lot of energy put into thinking about that and there are proposals that range from taking every piece of debris and laying it out and moving every single piece to a more unified layout to moving pieces together that seem to be related. Each of the proposed plans has its pros and cons. My personal opinion is if you wait until the parts tell you they need to be together, you can see that emerge; but there is no ideal layout in a hangar that is smaller than the surface area of the vehicle.

So that's one of the fundamental things. The first thing you can say is give me 60,000 square feet instead of 40,000 square feet, so not use that hangar. Then you can lay it out any way you want with lots of space. What ended up seeming to me to be the right limiter was the fact that we were moving puzzle pieces and deciding which ones could be non-ideal, because we're limited by the floor space. But certainly the tool that we use the most is walking around and staring at parts and trying to visualize relationships and maybe even putting little flags down so we can see things. And we do a lot of that. I think the 3-D reconstruction, both software and physical, will help a lot, though -- certainly the software one which will come along soon, we hope.

ADM. TURCOTTE: Thank you.

ADM. GEHMAN: Another area of useful comparisons -- I hope it's useful comparison -- where we have matching sets between left and right is tires. Would you tell us how many tires we have, how many on which side, and have the tires told you anything?

DR. KOVACS: We've spent a lot of time looking at tires, sir, and we have a tire that looks like it's left inboard and we have a tire that looks like right inboard. The nose gear tires are both there; and they're not really the focus at the moment, at least not for us. We have one more tire that's come in that's pretty much intact that I think we were told one thing then told another. So I'm not sure which it is but it's, by elimination, an outboard tire, right or left. And we've been looking at those a lot and their relative condition.

ADM. GEHMAN: And?

DR. KOVACS: Well, the outboard tires, the supposed outboard tires, seem to be in much better shape, comparable to the nose gear tires.

ADM. GEHMAN: I was referring to any left, right. Can you make any left wing, right wing comparison?

DR. KOVACS: The left inboard tire seems to be completely blown apart into two pieces or at least separated into two pieces. Whether it was blown apart or not, I couldn't say, as found -- and bearing in mind that it impacted the ground at some fairly high velocity. Its section is inside out. We spent a lot of time with a Boeing person, picking up and rotating those pieces to be absolutely sure that they were of the same tire. We're all convinced of that. There's some sections that look like they experienced more heat than the other tires that we have. So there are those differences.

DR. OSHEROFF: Looking at the picture that you have up there, which one of the two tires that we're looking at is the inboard tire?

DR. KOVACS: The one you see the rim of here is the outboard tire. So we're looking out to in. So that would be the inboard tire there.

DR. OSHEROFF: Are we looking from the bottom or the top? In other words, is the inboard tire above or below the outboard tire?

DR. KOVACS: I'm going to take that back, I'm sorry. I think this is inboard here. I'm not dead sure of the orientation of the drawing with respect to the screen but there are inboard -- because this vent --

DR. OSHEROFF: This is vertical. Then the question is which one is this front --

DR. KOVACS: So this would be the inboard tire. This vent here is on the inboard side.

MR. WALLACE: But the door hinges on the outboard edge, correct?

DR. KOVACS: Right.

ADM. GEHMAN: Let me see if I've got any other questions here. I guess I have the last question. Again, it's for Mark. I gather that in the area of kind of what we call microscopic analysis that we're really just getting started. Could you tell me is that correct, are we just getting started and how aggressive is that program and what kind of time lines are we looking at here?

MR. TANNER: That's a good question. At this point in time, I think you heard Steve mention earlier that they've taken like 70 samples. We're supposed to be getting -- as a matter of fact, this afternoon -- a report, I think, on the majority of those presented to us. Those are what I'd call the less non-destructive because we've been able to take a little piece off that we had multiple deposits on. At this point in time, I haven't seen a plan yet of the metallurgical analysis to start try to focus on what we're going to do.

Their methodology is making a fact sheet for everything and then making a wish list and then combining that to go forward. It's a very methodical, very logical process, but it's also a little more time-consuming process than sometimes you might see in industry. So it's hard for me to put a time line on when that whole process would take place, but I do think the deposit analysis we should be able to ramp up, especially after getting the results today. We wanted to review the results to kind of make some slight corrections or more emphasis in some areas and then go forward with some analysis Greg was talking about earlier, especially on the oxygen content.

I do know they're supposed to be cross-sections through some of the metal splatter and trying to see what we can see. I'm not sure if that's going to be presented today or not. So it's a little bit slower, but it's also that they want to be correct and accurate.

DR. OSHEROFF: Which parts? I mean, parts are still coming in. If you had a wish list of the most important parts that you would like to see, what would that be?

MR. TANNER: We'll both take a crack at that one.

DR. KOVACS: We each have our wish list, but I would say as much of the reinforced carbon-carbon pieces as possible because of the focus on the leading edges. As I said and Mark said, we have a lot of pieces; it's just a matter of puzzling them together. That would be my first priority.

MR. TANNER: I think the second would be the left landing gear door area. At this point we really just have that uplock, and we've got an interesting pattern of some heat there. It would be nice if we could find some more pieces to help us figure out how that flow was introduced into that wheel well. So that be would my second wish list.

ADM. GEHMAN: The Board, of course, we have in the past put a lot of weight on finding pieces which were shed early. Do you also attach a lot of weight to that? Do you find that there's probably significance in pieces that were shed early? For example, we do have these two, a tile and a fragment of a tile that were found west of Fort Worth. Then, of course, there's this very large Debris No. 6 and Debris No. 14 from the video, which we haven't found yet, all of which are even west of Texas. Can you give me an appreciation for the importance you attach to those pieces?

DR. KOVACS: Certainly it could be very interesting to see early debris, westerly debris; and we spent a lot of time trying to figure out which piece is the most westerly. I think an important question though is where, if there was a breach, where those pieces ended up. Because if the breach was in the RCC, it's not clear to me, anyway, that it wouldn't have been blown inward into the wing. So I think there's a pretty good probability that we have some pieces from such an event, if that's what happened that it were lodged in the wing and then, when it finally came apart, were released. So I'm not so sure that we don't have some of that early information already; but it would be wonderful, of course, to have a piece that was shed in Nevada, for example.

ADM. GEHMAN: Have you seen any evidence in the leading edge of the left wing, of just -- this would have to be, of course, a gross evaluation because you have very little of the leading edge of the left wing -- but have you seen any sign whatsoever of heating, either slag or dark deposits or anything else of a heating pattern which seems to dissipate? In other words, does it seem to be more intense in one place and then get lighter someplace else or are we way too early to talk about that?

MR. TANNER: Well, I think it may be a bit too early. There is a trend that's starting to occur right now, and that's around Panel 8. We're seeing a lot heavier deposit, very significantly heavier deposit that's been thrown up on the upper side of the RCC; but as you get away from there, the deposits are still there but not quite as much.

MR. HUBBARD: Two questions. What's the status of finding any of the carrier panel structure? That's been called various things -- carrier panel, closeout panels, et cetera -- the piece of structure and tile that goes between the RCC and the body.

DR. KOVACS: We have several of them. I wish I could quickly get to a picture. There are several pieces there. They seem to be more, at least for the moment, in the forward panels. Of course, what we don't find would be quite interesting; and I think as the search is closed out, those are very easy-to-identify pieces. And we'll have, hopefully, some telling information in what we don't find. Those that we find aren't particularly enlightening.

MR. TANNER: Indeed, there are some tiles that go on the carrier panels that they think they have located; but again, those famous orange tags. They're not quite convinced yet. Some of those are in the region of interest and show some heating.

MR. HUBBARD: One point to be sure that didn't get missed here. We sort of zipped by it, which is if you had an initiating event that caused something to leave, you know, like the thing that was seen on the second day of flight, that would be in this westernmost region; but anything that happened after that, if there started to be damage in the wing, consumed itself from the inside out, so to speak. What I think I heard you say is one plausible hypothesis is that those things got carried inside the wing and actually could be part of what we see on the ground there.

DR. KOVACS: I would say that's something that I have been contemplating a lot because if you think about the static pressure loads, certainly at the beginning there's a lot of force. So if a piece is sort of flapping around and it's on the underside and so you think about the angle of attack, it's easy to believe that a piece would be folded and broken up and end up wedged inside there and be driven back as thins are melting. If it's RCC, it may well survive that and be sort of stuck there. And we've seen some interesting things like pieces of what look like RCC slammed into a tile. So there's some hope that we'll find some pieces from early on.

MR. HUBBARD: That would imply, too, that if it's not all exiting from the outside into the environment but there's a significant fraction going inside, that that would imply a lot of this whatever we want to call it, sprayed metal or slag, toward the end of the RCC panels, around Panel 22 or something. Is that a trend that's emerging, or is it too early to say?

DR. KOVACS: There's not much down there.

MR. TANNER: A little early to say right now. There is some stuff down there, but I would hate to speculate for sure.

MR. HUBBARD: At least it's a testable idea.

MR. TANNER: Absolutely.

ADM. GEHMAN: We have released information to the press sometime ago that there appeared to be heat flow patterns around the left main landing gear door that appear to be heat flow patterns coming out of the door rather than going into the door. You've seen those?

MR. TANNER: Yes.

ADM. GEHMAN: You agree? At least visually you agree with that?

MR. TANNER: Definitely appears to be exiting the door and there are some tiles that have some interesting deposits on them that would also indicate they're in that vicinity, the metal's exiting the door and getting onto the tiles.

DR. KOVACS: One thing to add to that, though, is I think it behooves us to be dead sure about vehicle orientation when we're looking at those flow patterns. You say, well, it's out because it's perpendicular to, you know, fore to aft -- well, it may be that the vehicle was in a funny orientation.

ADM. GEHMAN: I understand. Lastly, going back to our first panel about debris collection, I suppose that you all would vote in the camp that you need a lot more debris and you need for them to keep picking things up.

DR. KOVACS: Yes, sir.

ADM. GEHMAN: Well, thank you very much to both Mr. Kovacs and Mr. Tanner and the hundreds and hundreds of people that are working so diligently to find the answer to this, the riddle that started this terrible tragedy. We, the panel, have a certain amount of weight that we're giving to the debris reconstruction and analysis; and as time goes on, that weight increases. So we are counting on you and your people to help us with this.

I know from our personal interaction with the people on the floor out there how hard they're working and how careful and diligent they are. I think that someplace out there is probably a couple of our answers that we need. We just have to keep working at it until we find it.

So please pass on to all of the folks that are working so hard and so seriously our admiration and our gratitude for what they do on a day-in-and-day-out basis for which they don't get a whole lot of publicity. It's just plain tedious work and it's got to be done right and it's got to be done carefully and real smart people are working on it and we realize that and we want to give them our thanks. Thank you very much.

For the members of the press, I think we have our press conference at 1:00 o'clock right here. So please don't attack us as we leave the stage. We will answer all your questions later this afternoon. Thank you very much.

(Hearing concluded at 11:36 a.m.)

April 7, 2003
Houston, Texas

Columbia Accident Investigation Board Public Hearing
Monday, April 7, 2003

1:00 p.m.
Hilton Hotel
3000 NASA Road 1
Houston, Texas

Board Members Present:
Admiral Hal Gehman
Major General Ken Hess
Brigadier General Duane Deal
Dr. Sally Ride
Dr. John Logsdon
Mr. G. Scott Hubbard
Mr. Steven Wallace

Witnesses Testifying:
Col. James Halsell, Jr.
Mr. Robert Castle, Jr.
Mr. J. Scott Sparks
Mr. Lee Foster

ADM. GEHMAN: Good afternoon, ladies and gentlemen. This public hearing of the Columbia Accident Investigation Board is in session. We're privileged to have with us today two experts to help us see our way through some of the issues that we have to deal with, and we're going to deal with the treatment of anomalies and waivers and certifications and all that sort of stuff today. We have a panel of two -- I don't know if you'd call them experts or not; we'll see at the end of the day whether they're experts or not -- but to help guide us through the first part of this process. The first is Colonel James Halsell, who is an astronaut and has a couple of duties, one of which is, I presume, to command a mission here in the future, I trust; and Robert Castle, who is from the Mission Operations Directorate.

Gentlemen, before we begin, let me ask you to first to affirm that the information you provide the Board today will be accurate and complete, to the best of your current knowledge and belief.

THE WITNESSES: I do affirm. JAMES HALSELL and ROBERT CASTLE, JR. testified as follows:

ADM. GEHMAN: Would either one of you start and introduce yourselves and tell us a little bit about your background but also tell us what your duties are today.

COL. HALSELL: Okay. I'll start first, sir. It's my privilege to be here to have the opportunity to work toward what certainly anybody at NASA considers to be one of the most important things we'll ever do in our career -- that is, to find out what happened, to fix it, and get back to flying safely.

I have a background in the Air Force. I'm an active duty Colonel in the Air Force. My background in aviation was fighter aviation, followed by test aviation, and then an assignment to NASA for the last 13 years, since 1990 as an astronaut. I had the privilege of flying five missions; and at the conclusion of my fifth mission, I was asked to take on, as a career-broadening experience, a management job down at the Kennedy Space Center as a launch integration manager, working directly for the program manager, Mr. Ron Dittemore. I did that from the summer of 2000 until January of this year, when I was relieved of that job in order to take my next assignment, which was to command STS-120, which will be a mission to the International Space Station, taking up Node 2, one of the hardware components that will complete the American initial phase of the construction of the Station.

If you'd like, at this point in time I can talk to you --

ADM. GEHMAN: Before we do, let me ask. Do you also have a role in the return-to-flight process?

COL. HALSELL: Yes, sir. I received word just two weeks ago that I would be requested to head up a return-to-flight planning team. We would be doing a staff planning function, reporting directly to the deputy Associate Administrator for Station and Shuttle. That's Retired General Michael Kostelnik. Our job is to be his interface to the Shuttle Program and, in fact, throughout the NASA system working this issue, to come forward with recommendations and options in response to the Columbia Accident Investigation Board's findings and recommendations. So the way it should work is that once your investigation board wraps up with a report, and hopefully even in the interim before that final phase, we'll have the opportunity to map out a response to your investigation board's findings and recommendations. I'm sure that we'll come down to a set of options that we'll offer up to our leadership and our management and they will make some of the tough choices that have to be made with regard to what has to be done to fly safely again, what needs to be done in the long term to make the system even safer.

ADM. GEHMAN: Let's let Mr. Castle introduce himself, and then you can start.

MR. CASTLE: Okay. I'm very honored to be here and take part in this, in the return-to-flight effort for the *Columbia*. A little bit about myself. I'm a full-time career civil servant. I've been working for NASA for 25 years now. I started working one of the mission control sections as a communications officer, did that for about ten years, and then was a mid-level manager for about a year and then was selected for the flight director office in 1988. So I've spent right at 15 years as a NASA flight director, running missions in Mission Control.

I have recently left that job to become the Missions Operations Directorate chief engineer and currently working on things like orbital space plane and some upgrades in the control center as well as contributing a lot of work on the International Space Station. I should also say I was a Shuttle flight director for virtually all of that time. The last two years or so, I've switched over and become mainly a flight director on the International Space Station. That started around the middle of the year 2000 was when I did that much more than I did Shuttle flights. So that's my current role to date.

ADM. GEHMAN: Thank you very much. Colonel Halsell, if you have a statement or perhaps a presentation, we're ready to listen.

COL. HALSELL: Yes, sir. I did come prepared with a presentation package. Certainly I would expect -- and feel free, as I'm sure you will, to ask me questions as we go along in this somewhat lengthy package.

It's my understanding that I've been asked here today to give you any information that I might provide with the preflight process. In the Shuttle Program we call it the Flight Preparation Process, FPP for short. So if I use that acronym, that will be what I'm talking about. And that is the all-encompassing phrase, if you will, for everything that we do to get ready to go fly safely, including a subpart of that is the Certification of Flight Readiness and all the reviews and boards that we go through for that.

Before I launch off into the details, it might be helpful if we just review the basics. The basics are basically this. The way the Shuttle Program is set up -- and I believe correctly and appropriately so -- is we have a set of requirements. It is huge, long list of requirements. It's broken down by the projects and the elements and all the contributing manufacturers, but the Space Shuttle Program is responsible to be the keeper of the list of requirements. It tells us how we're going to build a component, how we're going to use it. It tells us how we train the crews. It tells us how we prepare the vehicles. Everything we do answers back to a requirement; and before we go launch a Shuttle mission, it's absolutely required that we know we have lived up to and, in a closed-loop accounting fashion, answered each and every one of those requirements successfully.

In a perfect world, you would have your requirements on one hand and before we go to launch, you'd have the absolute and utter proof that you met each and every one of your requirements. We do live in that perfect world except there is such a thing as a waiver, in the sense that oftentimes if you can't meet the intent, indeed, the scripture of a requirement, then you have to come forward to the program, and specifically the program manager, and make the case for what you are offering instead is sufficient to allow a complete, productive, and safe mission. If you can pass that test, then with the waiver we are allowed to go ahead and fly.

So it's requirements, closed-loop accounting system, and to the degree to which they don't match up perfectly, we enter into the waiver process. That's the 37,000-foot view of what we do, and almost everything that we talk about from this point on could be tied back to that very simple basic process.

I know that after *Challenger*, it was recognized that these processes were not as disciplined and as rigorous as they should be; and I believe what I hope to tell you today and what I hope comes out of my presentation is that following the *Challenger* disaster, we went back and did rigorously enforce that discipline. In the degree to which we fell short in the *Columbia* accident, that's why we're here today and that's what we want to find out.

I think it might be helpful just to lay out a couple of other basic thoughts. The Shuttle was designed with the philosophy that you should not have a system in which you suffer a failure and you lose your vehicle or your crew. It needs to be fail-safe. Furthermore there was a high operational desire to be fail-operational -- that is, suffer a failure and still complete the mission. The basic requirements are that the vehicle will be and all of its subsystems will be fail-safe.

From the very beginning, there were three of the systems which it was acknowledged we could not achieve that desired goal. The thermal protection system was one. It was recognized as being a Criticality 1 -- that is, if it doesn't work, you're going to lose the vehicle and/or the crew and we don't have a backup system to it. Pressure vessels, whether it's the pressure vessel in which the crew resides or the pressure vessels which holds our fuels and our oxidizers and our cryogens, was another. And finally the primary structure of the vehicle. The vehicle was not built with the intent that you could lose anyone -- you could always guarantee that you could lose one primary load-bearing piece of the structure and still maintain your safety margins. So those are the three areas where the design of the vehicle, it was acknowledged, would not live up to the basic requirement of being fail-safe.

On the other hand, in the area of avionics, they designed it with a higher than fail-safe, that is, a fail-operational requirement. In our avionics area, it was designed to be able to suffer any one failure and continue to nominal end of mission. Those are my opening thoughts and maybe background that might help you as we delve down into the flight preparation process in detail. So with that, if I can press on to the next slide, please.

This is a flow chart that shows you the program level reviews. Each of these represents a review, a large meeting of all the relevant NASA and contractor personnel; and it's also just a program level. Below each of these program level reviews is a vast array of project level reviews, but let me just briefly go through this and it will give you the outline of what we do and how we do it.

Starting in the upper left-hand corner, the Flight Definition Requirements Document. That is the bible that a flight, a mission, in the preparation of a vehicle for that mission, where it all gets laid out. Normally this is presented to the Program Requirements Change Board, which is the program manager's venue for considering these top-level issues, about 16 months prior to flight. You can go from the front of the vehicle to the tail of the vehicle and talk about the level of detail, but basically that first block should be preceded by two or three years of preceding blocks where our customer and flight integration office receives inquiries from our potential customers to understand what payloads they want to fly, what mission requirements they are considering, and that's mapping those against the Shuttle capabilities and whether or not we can satisfy those requirements. In a very complete iterative process we go through understanding what do they want to do, what is it that we're able to do, and to the degree that it doesn't match up, let's try to better understand how we might be able to force a match there.

When you get to the FDRD, you know the vehicle you're going to fly on, you know the size of the crew, you know how much cryogenic oxygen and hydrogen's going to be on board, because that drives how long the mission can be because, of course, that's breathing oxygen for the crew and that's also what we use to generate electrical power for the payload and for the other systems on board the Orbiter. You know exactly what the payload configuration is going to be in the payload bay, down to the keel and the trunnion attachments on the side walls of the vehicle. You know probably the serial numbers of the engines you're going to fly. It baselines everything there is that you really need to start out to do the detailed final preparation for the mission, and that baseline can only be changed from that point on by going back to the Program Requirements Change Board and asking permission.

So that's the FDRD, and it's really the first milestone at the program level. The other blocks as we follow along there have names which are fairly self-explanatory of what they do and what we're there to do. The Cargo Integration Review highlights and further refines details with the payload that we're going to be carrying for that mission.

The Ascent Flight Design is a program-level review because that is understood to be the most dynamic phase of flight. It's the one where we have to tailor the software the most from flight to flight, given any one of a number of variables, not only the payload you're carrying and the weights involved and the load of propellants that you're going to carry on that particular flight. So we bring that to the program level.

The FPSR, the Flight Planning and Storage Review, is the one that's near and dear to most crew members' hearts because that usually happens at about the ten-month-or-so month prior to flight and that's just about the time that the crew has just been named and has started working together as a crew. So that's the first one that the crew normally supports; and the Flight Plan and Storage Review, it really summarizes the issues which are most importance to the crew. The flight plan tells everybody, including the crew, what you're going to be doing every second of every mission; and if you can nail down the flight plan and make it answer back to the requirements of the flight, it's a lot easier on the commander to be able to plan his mission and to plan his training for his crew, which is one of the primary jobs of the commander pre-launch.

The other important part is stowage. Living on board the Space Shuttle and working on board the Space Shuttle has been likened to a camping trip in a closet in that you have to know exactly where everything is so you can get to it in a timely fashion and you also have to get it back in the right place before you come home. And the degree to which you don't know that or you make it more difficult than it has to be, it directly impacts your ability as a crew to get your work done. So you try very hard after you're first named as a crew to get to the Flight Planning and Storage Review and understand the degree to which we have a high level of fidelity in that planning process, because that's your first key, your first clue as to how much work you have in front of you in planning the mission, the details of it.

The next three blocks really have to do with the same subject, and that is at the Kennedy Space Center what are they going to have to do after that Orbiter lands from its previous mission until you launch it on its upcoming flight. The first block, the Integrated Launch Site Requirements Review, is where you hash out what are the actual requirements. You know you've got to be able to get the payload into the payload bay. What are the requirements before and after and leading up to that event? What are the modifications that you want to do on this vehicle? At any given time in the Shuttle Program, there is usually a list of modifications which are ready to go to be implemented in any given vehicle, and you have to weigh is now the time to try to insert any of that particular modification to bring the improvements that it does either to the capabilities or to the safety level, or do you have to understand that the manifest at its current state is such that work would be better implemented one flow following this flight. So you make those trade-offs and those kinds of determinations at that time.

Then the Kennedy Space Center comes back at the Launch Site Flow Review and they tell you their ability to meet those requirements and that they're going to be able to do it and to the degree that there's a mismatch, we hash it out at that meeting.

There's one other meeting, the Delta Launch Site Flow Review. By the way, the timing is 60, 30, plus 15. That is, it's about two months prior to the landing of the Orbiter from its previous mission that you really try to nail down the requirements. It's about one month prior to that landing that you do the flow review and have Kennedy come back and tell you if they are going to be able to accomplish it. After the landing from the previous mission has accomplished and they've been able to roll the vehicle into the processing facility, you understand better the condition and any in-flight anomalies which it had during the previous mission, how that might impact what you had planned to do previously. You bring that back to the program at the Delta Launch Site Flow Review and that's where you make any final determinations and judgments on what we are and are not going to do on this particular flow. If necessary, you adjust the launch dates to meet those requirements.

So that's the program level review, starting at 16 months prior, to actually up to two weeks after the landing of that Orbiter and you start to process the vehicle. This is what's typically referred to as the flight preparation process.

The last block that I'll lead into with the asterisk is called Milestone Reviews, and this is going to be where we now tend toward more of a Certification of Flight Readiness flavor for what we're doing. If I could have the next chart, please.

I believe I've talked about all this. So if we could press on to the next chart.

The next chart, please. Here we go. Here's the wiring diagram to talk about the milestone reviews and the certification of flight readiness that results from this process. The chart flows from the left to the right. On the left-hand side, you have the different projects and elements, each one responsible for a particular major system on the Orbiter. On the far right-hand side, you have our flag -- I'll call it our flagship review, the Flight Readiness Review, which typically happens about two weeks prior to launch, where we present all the information to senior NASA management to determine the final readiness for launch; and everybody's required at that point in time to sign up to the Certificate of Flight Readiness. In between is an incremental improvement at each step in our ability and a refinement in our ability to say, yes, we are headed toward the satisfactory Certification of Flight Readiness.

Starting at the left on the project level, their major review would be the Element Acceptance Review. That's where the government project manager will accept from the contractor the piece of hardware. Once again, there's a whole hidden set of pre-reviews that led up to the Element Acceptance Review. I've talked to a number of project managers and I think they'll all tell you it would be totally unacceptable for them to be surprised or to hear an issue at the Element Acceptance Review that they did not previously know about.

So it's worked in real time, but we do lead up to the EAR for each major component of the vehicle. Then where I've gotten involved in my job as the launch integration manager are in the two double-bordered boxes that you see there. The ET/SRB Mate Review and the Orbiter Rollout Mate Review. Each of those represents a processing milestone that we want to be very careful and we want to be very studious, if you will, before we go through that milestone, without taking a breath and stopping and pausing and making sure we're ready to go do that.

I approach it from the point of view of two aspects. First of all, those mate reviews were my opportunity as the integration manager to actually understand the rationale that was going to be brought forward at the Flight Readiness Review for any of the major waivers, hazards, first-time flight items, changes to processes, in-flight anomalies to be considered up to that point in time. It was my opportunity to hear that in a formal forum and to understand how they were going to present it to the Flight Readiness Review.

Now, let me make it immediately clear that, just as it would have been unsatisfactory for a project manager to come to an Element Acceptance Review that did not know everything that he was going to be told, it would be equally unsatisfactory for me as the launch integration manager to come to a mate review and not know the details of everything that was going to be presented and have had a history of having known the development of all those issues over the prior months. Nevertheless, that's the first time we put it all together in one package.

ADM. GEHMAN: Let me interrupt. This is where -- I mean, you mentioned this. I just want to be clear about this. In the Element Acceptance Review, these EARs, as well as at these program reviews, previous waivers and waivers that are currently in existence, disposition of old in-flight anomalies would all be brought up, kicked around the table, and if they had been accepted in the past, the acceptance would be re-agreed?

COL. HALSELL: Yes, sir. I believe I understand the intent of the question. There is a requirement both at the project level and at the program level for us to fully understand in-flight anomalies as they apply to that particular piece of hardware and the mission that's about to be flown. There's a requirement to review and understand all the waivers that had been issued and, in particular, concentrate on any change of waivers or any new waivers. If it's a waiver which has previously been approved through the program and through the entire system and there is nothing different about it's applicability or this flight as compared to the previous flights, then it's not necessary that it be brought forward again and again and again; but what is absolutely required is that any new waivers or changes to waivers be highlighted at each of these progressive milestones.

ADM. GEHMAN: Just from an administrative point of view, if a system over a period of 20 years is operating under 25 waivers -- which, by the way, that's probably not an outlandish number; it might be more than that in some cases -- how does the system deal with the fact that a waiver's starting to accumulate.

COL. HALSELL: I am aware during the time that I was at the Cape that the program approached that exact issue at least on a couple of occasions. Just before I took over as the launch integration manager in the summer of 2000, my immediate predecessor, Mr. Bill Gerstenmaier, under Ron Dittemore's direction, had gone through a review of the waivers. The question was: How many are out there? Are they all still valid? How often do we review this situation so that we're not guilty of unknowingly accumulating waivers? To what degree are we confident that we have good rationale for retaining waivers in place?

What we found out from that review is that we do have a good process in place. There's an annual review of the waivers to make sure that it is still appropriate, it's still applicable, it's still necessary. Remember, we should probably back up a step and just talk a little bit about how you go through the process of granting a waiver. What you want to do, to the degree that you can't meet the requirements that you have in place, you want to try to change that and satisfy the requirements. So your first goal would be to try to execute some type of design change that allows you to satisfy that requirement. To the degree that that's not possible, then you look at other mitigating factors, if you're able to put warning devices or safety systems in place or crew or ground work-around procedures in place which mitigate the risks. Those are the kinds of things that have to be part of the acceptance of the residual risk when you do go forward with a waiver.

ADM. GEHMAN: Okay. Thank you very much. That answered my question. So the kind of legacy waivers then are reviewed annually or periodically, depending on what the project manager wants as a kind of bring-up.

COL. HALSELL: Right. Once again, we concentrate most directly -- in the Flight Readiness Review process and the Certification of Flight Readiness for a particular flight, what you want to know is what's changed from this mission to the previous missions or those waivers which need to be highlighted due to the operational flavor of this particular flight and maybe being different from recent previous missions. You'll make sure that those differences, those deltas, as we call them, that's what you bring forward. The same would be true for the failure modes and effects analyses, the hazards, the program hazards. So there is a family of processes which we sometimes capture in this one word "waiver," but they're all reviewed and all brought forward as required during the Certification of Flight Readiness process to make sure that we're not guilty of missing a waiver rationale that is in need of review prior to that upcoming flight.

MR. WALLACE: You said that it would be unusual at an Element Acceptance Review for something to come up that you hadn't heard of previously. I have to say in the weeks learning about the FRR process and even the Launch Readiness Review just done in the days before the launch at the Cape that this is sort of a recurring message, like the work is kind of done before these meetings. I'm curious is it fair to say that these meetings, then, don't get scheduled until the work is done or is it unusual things get stopped at these meetings? Does the meeting become sort of a sign-off formality?

COL. HALSELL: I guess the best way to answer your question would be to talk a little bit about my personal experience in this area. When I stopped flying on a Shuttle crew for a while and I went down to be the Shuttle launch integration manager, I perceived some of the same flavor that you're talking about. That is, the important work was being done and being done exceptionally well -- so well, in fact, that when we got to some of these milestone reviews, it appeared to me that all of the hard issues had been discussed, all of the hard decisions and trade-offs had been made. So I questioned the value to our senior management of these level of reviews; but after being in the job for a longer period of time and after having discussed this situation with a number of my project managers, they had a different point of view. They didn't disagree with the fact that the way we do business is such that most of these problems, not always, but most of them, have been flattened out prior to the formal review, but it's because of the presence of those formal reviews and the fact that you know that senior NASA management, the people that you answer to and the people who are ultimately responsible for the safety of the upcoming mission, 'cause you know they're going to be there to hear that story, it drives all that outstanding work that happens before. So from the point of view of the projects and the elements, they did not want to change or consider any dramatic changes to the forum or to the agenda of any of these reviews because, from their perspective, they were driving the kind of reaction within the system that was healthy and needed.

DR. LOGSDON: If I heard what you just said correctly, then what's presented to the senior managers is the situation after things have been smoothed out. How much visibility do the senior managers have to the process of resolving issues prior to the formal reviews?

COL. HALSELL: Let me see if I can say it in a clearer fashion. I believe that the senior management within NASA, since the *Challenger* disaster, serves a critical role in deciding upon the final readiness to go fly safely, and it's our job as the middle-level managers to provide them with the information that they need to make that determination. I believe that the process we have in place works very well to do that. I believe that absolutely if we get to a Flight Readiness Review where there are any outstanding issues or if there are any issues that need to be discussed to the infinite level of detail for that level of management, we do that; and I can recount a number of instances where a Flight Readiness Review which was marching along according to the agenda and there were no particular issues, we would come upon one that required the next hour of discussion. It would require a number of people to stand up ad hoc and discuss their participation and their rationale. The Flight Readiness Review board, as would my board on the Orbiter roll-out and the mate reviews, if there was something fuzzy or something that we did not agree with or something that we needed additional clarification, we would delve into those details at that board, up to and including the flagship review, the FRR. The point I was trying to make earlier was it's knowing that you are subject to that level of review and that level of detailed review, if necessary, that drives all the good work leading up to it.

DR. RIDE: This may not be quite the right time to ask this question. Maybe it should be further on in your presentation, but you've now mentioned twice that since the *Challenger* accident, processes have been improved and put in place. I just wonder whether you could elaborate on that and maybe be a little bit specific about changes that you are aware of. There were, of course, FRRs before 51L, PRCBs before 51L, senior management was pretty heavily involved in the key meetings leading up to a launch. I'd just be interested in your assessment of what changes have actually taken place.

COL. HALSELL: Thinking back to some of the *Challenger* findings and recommendations, I believe there were ten major findings and recommendations and then appendices behind that. I know that NASA responded to each and every one of those. The two that come to mind, one that's particularly important to me because it has certainly affected my life, was the thought that we needed to involve the astronaut corps in more of the middle and, if appropriate, later in their career, senior management jobs because bringing that operational expertise over to the managerial side of the house was value added to the entire system. I do know that, for example, immediately after the *Challenger* accident, a number of astronauts were consciously moved into management positions and we have retained that priority for astronauts as part of their career progression ever since then. I don't know the degree to which astronauts were involved prior to the *Challenger*, but I know that, after, the answer has been quite heavily and in numerous occasions.

I know that another finding from the *Challenger* commission had to do with the fact that on the specific decision to go ahead and fly, given the new data that was brought forward the night prior to that launch, that information, that discussion, the dissenting opinions and the method of which it was finally decided that we were going to go fly that day, all that was not brought forward to senior NASA management in a timely fashion; and I truly believe that today, given the processes that we have in place -- and you'll hear more about the Mission Management Team later on -- that would not be the case. That issue would have been elevated to the appropriate level, given the same set of circumstances today.

DR. RIDE: I guess I was just curious whether you could point to any specific -- and again, this may not be the time - - but any specific parts of the process that have been added or specifically strengthened in the pre-launch process.

COL. HALSELL: I guess I can speak to the strengths of the processes that we have in place. With regard to the details of comparison how it was pre-*Challenger*, which was prior to my participation, I probably would not be the right person to ask; but when I get to the part about the Mission Management Team and the process that's in place, I would invite anybody who is knowledgeable about being able to compare that specifically to what we did pre-*Challenger* to help me out there.

GEN. HESS: Colonel, before we get too much further in your briefing, which might be in question, I was curious about providing some balance in the discussion with regards to the line responsibilities to the requirements meetings and these various reviews and how that is balanced by the S&MA organization and recalling the Rogers Commission saying you needed an independent safety process. So if you could help us out at these various stages and give us some idea about how safety figures in and whether or not they can actually overturn one of these meetings because of their degree of questioning over a particular portion of the mission as it's going.

COL. HALSELL: Let me answer the last element of your question first, and the answer is absolutely yes. On each of the reviews that I've participated in, whether it be the Orbiter roll-out review or the mate review, the safety community is represented through several different channels. Also, the pre-launch Mission Management Team review at O minus 2 -- that's launch day minus 2 two days - - and then at the Flight Readiness Review, Safety is always there. They're always represented and they are always polled and they always expected to come forward with a dissenting opinion which would cause everything to stop at that point in time and we not press to the next review on the right side of that chart until we had it hashed out. So that's the answer I want you to hear is that Safety absolutely has not only the ability but the requirement to step forward if they believe that the engineering community is headed down a wrong path.

I believe that's the essential element of one of the strengths of the processes that we put in place. That is, that, in my opinion, a large part of your safety that's built into the system is accomplished through the strength and the viability of your engineering community and their in-house safety work that they do in line. But it's also important -- and I know that Ron Dittemore has always felt very strongly about this -- it's also important that we have an independent over-the-shoulder assessment of how we're doing from the safety community also. And the important aspect that we've always worked hard on is making sure that as we do our job in line, we have that independent assessment looking over our shoulder and then the fact that they are staffed, have the resources, and empowered to give that independent look at what we're doing. That's the fundamental strength, I believe, in the process that we have in place.

ADM. GEHMAN: Colonel Halsell, we're using the term "waiver." You already said this. I just want to clear it up. We're using this term "waiver" kind of loosely here because it really characterizes a number of administrative steps that are taken to account for processes. Can you mention what some of those other ones are called?

COL. HALSELL: Yes, sir. Some of the other categories that we talk about -- for example, hazards. Hazards are a top-down look. You start with a fairly limited number of ways that you can lose a vehicle or crew and then as you drill down deeper and deeper and you spread out farther and farther, you understand the more detailed failures that could cause that hazard to be recognized. The Shuttle Program is designed to avoid these hazards and, to the degree we are not able to do that, then we try to control them. You control them by looking at your design and implementing changes, if possible, or the safety controls or warning devices or crew operational procedure work-arounds that I talked about earlier.

ADM. GEHMAN: Is that what you referred to as a FMEA?

COL. HALSELL: Well, a FMEA CIL is actually a different process. It's from the bottom up. It's where you talk about, all right, what if that component of that box failed? Then at the box level, what if this avionics box fails or this component within my auxillary power unit hydraulic system fails? What's the worst thing that could happen to me as a result of that?

We have requirements within the system, as I explaining at the beginning of the discussion, with regard to our willingness to expose ourselves to risk. We always want to be fail-safe. We desire to be fail-operational. The degree to which we're not able to meet -- and you also use a risk matrix approach, if you will, in analyzing some of those risks associated with the different failures. Basically it boils down to looking at what is the probability of an occurrence of a particular failure and what are the consequences if that happens. Depending upon where you fall in that risk matrix determines whether it's unacceptable, in which case you don't fly and you make a decision to go fix it -- and I can give you examples of those kinds of cases -- or if it's an accepted risk because you believe that the mitigations that you have in place make the combination of probability and consequences a safe situation for you to go fly in. Then a totally controlled risk is where you don't believe there is any significant risk that you're being exposed to.

ADM. GEHMAN: If we took a case like the cause celebre of the day, foam hitting the Orbiter, if during the course of the years that foam shedding and foam hitting the Orbiter had been previously waived and had previously been disposed of, it's likely it would not even have come up at the ET review. Let me rephrase that. That's a question, not a statement.

COL. HALSELL: Yeah. And I believe my correct answer to your question is that I don't believe that to be true. We'll use that as an example, if we want to pull on this thread a little bit. I think it's well known that we did liberate a piece of foam on STS-112; and the process by which we went through understanding what had happened, how that related to our previously accepted hazards and FMEA CILs and what was the appropriate course of action from that point on all followed the processes that we had in place to try to ensure that the right decisions and the right trade-offs and risks got made.

For example, in the in-flight anomaly situation for STS-112, that did come to a Program Requirements Change Board. It was decided there that an in-flight anomaly designation was not required for this particular item because the previously accepted and documented hazards -- and if I remember correctly, there were two integrated hazards which were violated or which were called into question by this particular instance -- two of them dealing with the External Tank liberating foam and creating a hazard to some other vehicle component -- there was nothing about that particular instance which invalidated the rationale for the previously accepted risk. In other words, we didn't move up and to the right on the risk matrix, according to what we knew at that point that time. So the action that was levied at that Program Requirements Change Board was to the External Tank project to go back and fully understand what had happened, why it had happened, and what we were going to do to keep it from happening in the future. Also another action was levied to bring that item forward at the Flight Readiness Review to make sure it was discussed prior to STS-113. So using that as my example, I would say that that's an example of how the process worked properly and the item was brought forward to the Flight Readiness Review and it was discussed at some considerable length there.

DR. RIDE: How would that have been different if it had been classified as an in-flight anomaly after 112? What would have been different in the disposition process?

COL. HALSELL: Nothing. In the sense that whether it's designated in-flight anomaly or not, the important item is that two PRCB directives were issued at that time which directed the project to go back, analyze the problem, find out what it is, and fix it. Another action was issued to make sure this was brought forward to the Flight Readiness Review. So whether it's designated an in-flight anomaly or not, the answer is it would have made no difference.

Now, let me jump ahead and make sure that I'm not guilty of not answering the question you meant to ask, which is, if we had designated at the highest level, which is in-flight anomaly with constraint to next launch, then it would have been immediately an issue which had to be not only fully understood but resolved either with an engineering design change or an appropriate rationale for flight and formally documented. So on this particular case, I would maintain that that process was worked, because we did discuss this issue at the STS-113 Flight Readiness Review at some length. The process of making sure we felt comfortable and safe and that we understood the risks and the hazards and that there were no significant changes from those that had been accepted in the past, all that was done, despite the classification that we came forward with at the PRCB.

MR. WALLACE: If I could follow up. I understand from reading some of the PRACA documents that all PRACA reportable items must be dispositioned in some way -- I mean, prior to the next. Is that a fair statement?

COL. HALSELL: Yes, it is. However, there is sub-documentation that gives you guidance by which projects are allowed to enter into interim disposition as opposed to disposition prior to the very next flight. And it was the consideration of that particular set of guidance, of rules, along with what we thought was an understanding of no significant increase in risks due to the liberation of STS-112, that led the PRCB to decide that the appropriate way to deal with that particular issue was to issue the directive for the External Tank project to come back and find it and fix it and tell us what they had done and also discuss it prior to the Flight Readiness Review. In general, yes, all problem resolution reporting and corrective action items have to be dealt with. The level at which they get dealt with depends upon the criticality, Criticality 1 being the most significant and requiring the highest level of managerial insight and concurrence with. On the other end of the spectrum would be Criticality 3, which means you have no risk of loss of vehicle or crew. Those can sometimes, under the guide rules that we have written down, be dealt with at the project level and with different combinations in between going to different levels of management. I would hasten to add that, as a project manager or as a program person, you don't have the right to decide, on any given day, what level it's going to go to. That's all been decided for you, and it's documented for us in our processes.

MR. WALLACE: So this item which was a PRACA reportable item but not an in-flight anomaly on 112, there was an interim disposition?

COL. HALSELL: Yes.

MR. WALLACE: Which then didn't include any hardware changes -- it wasn't an assignment to --

COL. HALSELL: We can read the exact directive; but paraphrasing as I remember, it was: "ET Project, you've got until the 5th of December -- and I think that date was later extended due to some conflicts in scheduling -- but you've got until the 5th of December to go find out exactly what happened, reinforce for us what you're telling us today, which is you have no reason to believe that it's a generic issue and that we're at any increased risk on the upcoming flights of suffering this problem. We would like your options for engineering design changes which could be implemented to completely alleviate this problem in the future. Come back and report to us what your options are and what your recommended plan is."

MR. WALLACE: Could you tell us about the decision-making, I guess it was in the post-112 PRCB, the roles of different elements in the decision-making as regards the classification, in-flight anomaly or not, and the decision to go with an interim deposition, particularly the External Tank element and the S&MA office, if could you speak to that.

COL. HALSELL: I'm trying to think, Mr. Wallace. What additional information or what avenue are you trying to get me to talk about specifically that I haven't talked about already?

MR. WALLACE: Just really focus on who makes the call on that, on the in-flight anomaly decision and on the interim disposition items.

COL. HALSELL: You're doing a good job of doing my presentation for me -- and that's fine. That's good.

Let me. If I can go to the final two slides, as I remember, in the presentation, prior to the backup. Let's cover the two in-flight anomaly pages. After every flight, or as you're doing the flight, every element, every project, including Mission Operations Directorate, which Bob will have an opportunity to talk about here in a moment, they're compiling their list of things which have happened during this flight. Sometimes you hear it called the funnies list or the action log. It goes by a number of names depending upon which element or project you're talking to. I'll use the name "funnies list." That's everything that happened that was worthy of attention by somebody. In general, that entire list, all the projects, all the elements, all of their funnies get brought to the Program Requirements Change Board. Usually it's the first one following the landing of that vehicle. Sometimes it goes to the second PRCB. The program documentation says we need to do it no later than two weeks after landing, is our general goal.

It's a fairly long and detailed PRCB agenda item where you go through each and every problem that you experience, all the engineering information that you know that might have caused it, and the elements first blush on where we need to go from here. As part of that and as we go through each and every one of those items, it's a PRACA reportable item. You never have the option of saying, well, thank you very much but I don't think that's worthy of my attention. Everything gets dispositioned one way or the other, and part of the process that everybody is focusing on appropriately in this discussion is in-flight anomaly or not.

What you see before you are the listing of rules by which the funnies can get elevated to an in-flight anomaly. Just to go through them briefly, if it's a Criticality 1 or 2 --

meaning that we threaten the loss of vehicle or the crew, Criticality 1, and Criticality 2 meaning we threaten loss of a normal nominal mission, that's worthy of in-flight anomaly consideration. If it's software, either Orbiter flight software or the Space Shuttle main engines, it could cause Mission Operations Directorate -- and Bob can probably give us examples of these kind of situations where we got the nominal mission accomplished but they had to work extra hard and had to do a lot of work-arounds on orbit to make that happen -- then we don't want that to have to happen again. So we deal with that as an in-flight anomaly.

If it caused or if it could have caused a countdown hold or a launch scrub or a launch abort, then we want to deal with that. If it could have affected safety or mission success or caused significant impact on resources, logistics, or schedules for the future, or if it's any anomaly that the designated responsible design element wants to make an in-flight anomaly, they have the final word. So that's a list of things that we use as criteria for consideration as in-flight anomalies.

If I could have the next slide, please. As far as interim deposition is concerned, these are some of the items by which it's appropriate for us to give the elements more time to deal with these issues and not call them constraints to the very next flight. Let me run through those. Remember, it's one of the following criteria: If it's not applicable to the flight -- in other words, whatever broke last time, you're not flying next time, that's obvious; if the problem condition is clearly screened during pre-flight checkout or special tests and you know you're not subject to that same problem; if the problem is time/age/cycle related and the flight units will accumulate less than 50 percent of the critical parameters by the end of the upcoming flight; if there's no indication that this is a generic problem or if you have no overall safety-of-flight concern; if the problem is applicable to flights, however, the PRCB agrees that we have sufficient evidence that the system can be flown safely with acceptable risk, then those are the kinds of circumstances under which we would go to an interim disposition. And it's my belief that it was the consideration of these type of issues which led to the determination that the External Tank foam, using that as an example, would be an appropriate issue for us to talk about completely at the upcoming FRR but to give the project additional time to come forward with their corrective action.

MR. HUBBARD: I'd like to go a little bit to the hand-off between the end of one mission and the beginning of another. You just characterized what you do post-launch. Now, let's go pre-launch to the next mission. What is the process by which the collection of things that have happened over the various missions get put into a data base or some kind of a memory bank, other than just individuals around the table so that, as the missions go forward one after the other, you build up a sense of trends? You know, maybe there's nothing on one specific flight, but maybe there's an accumulation. How does that get brought to the attention of management during the review process?

COL. HALSELL: I believe the answer to your question is PCAS, which stands for Program Compliance Assurance System. Lately the new word is web PCAS in the sense that its been upgraded to a web-based system, and previously it had been a mainframe-hosted computer system. Web PCAS is a web-based system which allows any person associated with the program at any level, including senior management all the way down, to access all the sub-data bases. PRACA's been -- the problem resolution reporting and corrective action system, that's one of the sub-data bases which is part of PCAS, for example. The waivers list. The in-flight anomalies list. The FMEA CILs. All of these data bases -- and we could probably go on for quite some period of time to have an exhaustive list -- are part of the web PCAS which the engineering community and the safety community use equally in this type of trend analysis and in what we characterize as the paper close-out that has to happen before we go fly again. Before we fly, we have to be 100 percent sure that we have our requirements and our closed-loop accounting system has sufficiently -- you can't launch if you simply know nobody's elevated a problem. You have to have the reassurance that people have looked and that they have closed out all of the open paper, and it's only upon that positive affirmation that you can go fly.

MR. HUBBARD: So just to follow this one step further. This data base is available. Is there anybody who is charged with actually looking at it and as you go around the FRR and these other reviews saying, wait a minute, to take our favorite topic, I see a trend in foam-shedding or something like that?

COL. HALSELL: Yes, sir, and there are two somebodies. Every project and element -- and you'll see the participation in the Flight Readiness Review -- every project and element associated with the program has to say that verbally at the Flight Readiness Review. They are signing for that when they sign the Certificate of Flight Readiness that, yes, we have looked at this and we know we have closed out all these issues; and the independent assessment that we were talking about earlier, that's an important part of their function in ensuring safety is they look over our shoulder and they make sure that every project and every element has closed out those issues appropriately also.

ADM. GEHMAN: Could I ask you to go back one viewgraph here. I don't want to talk about STS-107 specifically. We're talking generic processes here, but I would like to talk about foam-shedding as a generic process. So if you can go back one viewgraph, please, to the in-flight anomalies, the IFA. Thank you.

Okay. So as I understand it -- and I don't know whether this viewgraph comes from NASA regulations or procedures or where it comes from, but I'm going to assume it's accurate for right now -- we, of course, will check that out -- it says there that any one of the following criteria makes it an IFA. I assume that damage to TPS, since it's Crit 1, that Item A there, any problem that affects a Crit 1 system which is damaging TPS, we've got ourselves an IFA.

COL. HALSELL: Yes, sir. I mean, reading No. A, that's what it says; and I would once again draw your attention to the second page which we've already covered, which gave further guidance which would allow an interim disposition.

ADM. GEHMAN: Now, I want to go to the second page. Once again, I'm not talking about the FRR of STS-107. We're going to go into that in some detail. I'm using this as a generic case. It looks to me like something hitting the thermal protection system or damage to the thermal protection system is a Crit 1 system and therefore anything that hits the TPS ought to be an IFA, looks to me, just using this score card. And if we look through the disposition here, it says that interim disposition is acceptable or a final closure is required if you meet any one of the following criteria. So I look at A, problems not applicable to the flight we're talking about -- that doesn't apply. A problem condition is clearly screened pre-flight -- that doesn't apply because you can't tell what piece of foam is going to fall off. C doesn't apply because it's not age related. D, I would say, doesn't apply because it's a generic problem and can happen anytime and anyplace else. Then we get down to E: There is no safety-of-flight concern. Now, can you tell me how -- or even the last one: The Board agrees that sufficient evidence exists that the system can be flown safely. How in the world does the system determine that there's no safety of flight? Do you know what processes there are involved or is it judgment or...

COL. HALSELL: I know you say we're not going to discuss and this is not STS-107 related, but it is ET foam related. So continuing with that as our example, as I remember, the particular presentation at that PRCB, the nature of the rationale that was presented in that forum was that the External Tank had gone back even at that point in time before they had responded to the following action and they had vigorously tried to understand did we do something different with the tank where we had this problem as compared to all the other tanks which had flown successfully. What came out of that was they felt comfortable that there was no new and generic issue that they could identify, either with changes or weaknesses in their processes of applying the foam or manufacturing or in the vendor that provides the raw material. They had already gone back and looked at all of that and they felt comfortable at that point in time that they had no generic issue that indicted follow-on future tanks that we were going to go fly. Furthermore, I do not know for a fact that it was presented in that form but I do know that as part of the Boeing transport mechanism there was no elevated level of concern that anything liberated from that location would have impacted the Orbiter. What all this added up to was the conclusion that we had not moved up and to the right on the risk matrix with respect to the previously accepted hazard, the two hazards that had been accepted and which we had flown for much of the life of the program, I believe, since STS-27.

ADM. GEHMAN: Thank you for that. To follow up on Mr. Wallace's question, is it the PRCB that would make that decision that there is no safety of flight or -- I mean, it wouldn't wait for an FRR; you would have settled this some other way, I assume.

COL. HALSELL: It isn't the Program Requirements Change Board, that the program manager has the ultimate responsibility for determining what are we going to classify as an IFA, what are we going to classify as an IFA with constraint, and which are we going to classify as an interim disposition with an action assigned to come back at a later point in time. But also it's important to understand that the Flight Readiness Review, upon review of any of those actions, certainly has the ability to upgrade any item that they so deem necessary.

ADM. GEHMAN: Absolutely.

DR. LOGSDON: I am going to ask a question about STS-107. If the mission had been successfully completed, would the foam shedding have been classified as an in-flight anomaly and, if so, by what criteria, since there was an analysis that said it was not a safety-of-flight issue. It was counter-factual, unfortunately.

COL. HALSELL: I want to make sure I answer exactly the question that you're asking, and it's in the context that we have had the foam liberation on STS-112.

ADM. GEHMAN: No, what he's saying is *Columbia* gets struck by foam just like she did but she returns safely. Do you have an IFA?

COL. HALSELL: Yes. Absolutely. And given that we have now had a second occurrence --

DR. LOGSDON: Go back to the prior slide.

COL. HALSELL: Before you do, just remember "D" there about the generic problem. At that point in time, I have absolutely no doubt that following the STS-112 incident and it happens again on 107, what you now have on your hands is a major issue that has to be dealt with before we consider even rolling out the next vehicle, much less flying the next vehicle.

MR. WALLACE: And the fact that on the 107 it struck the Orbiter, does this even make it way more clear that this would rise to the level of an IFA?

COL. HALSELL: Especially given that the Boeing transport analysis seemed to indicate that we were not at severe risk of having a strike against the Orbiter from a piece of foam liberated in this area. Now, to be complete and fair -- and I know you know this -- that same transport analysis also indicated that there were weaknesses in the program that was being used to do this analysis. Perhaps most specifically, they made the assumption that you were dealing with a non-lifting something and that as soon as you implied some lift in a direction, then that would have to undergo further additional analysis that took that into account.

ADM. GEHMAN: Why don't we let him move on here.

GEN. DEAL: Well, I'll go ahead and ask you an opinion question here, Jim, a little bit. It's based not just on your extensive experience in the Shuttle but also your flight test experience. If 1 out of every 25 flights you're flying a test development vehicle and it drops a panel forward of the intake, you know, I would think you would be a little bit concerned. We talked to some test pilots that say the deserts around Edwards are littered with panels out there, but, you know, I equate foam falling off of a bipod and hitting some part down below that's critical to the flight as being something forward of a jet intake. Can you give us any perspective about if we showed the right level of concern with four previous bipod ramp incidents where the foam broke off as compared to what type of precedents we put on it.

COL. HALSELL: I understand the context of the question you're asking me. As a test pilot and somebody involved in the job of acquiring the data with which a vehicle that's going to be flown for hundreds of thousands of hours over the fleet and making sure that we vet out all those issues while we're in the test phase, as opposed to in the operational phase, trying to transfer that experience to what we're dealing with here. One of the limitations that we've had over the entire life of the Shuttle Program is that we've never had the opportunity to accumulate the number of flights and the number of flight hours and the number of occurrences of any particular item to be able to apply the same statistical rigor that we're able to do in flight tests, for example, where you do quickly accumulate that kind of experience. I think trying to draw that analogy or that comparison might be an error on my part. So I would ask that I not be asked to do that because I don't feel comfortable doing so.

I will take what I think is the intent of your question, and that is at the point in time when STS-112occurred, we had not had a loss of ramp foam, if I remember correctly, since approximately STS-50. There might have been some interim problems with ramp foam, but nothing of that size and significance. Following STS-50, they had changed some of the procedures and some of the foams; and we thought that had been an improvement in our processes and in our materials. So when STS-112happened, whether it was appropriate or not, I think there was a consideration that this was a new occurrence, given a new baseline, and trying to statistically infer that what had happened prior to those changes were applicable to our current configuration was not appropriate. I'm sure that that consideration will be something that the investigation board will feel charged to draw an opinion on.

GEN. DEAL: I've got two other questions. Since we're controlling your briefing for you, if we can go back to Slide 10, I've got a question for you because we haven't covered that one yet. We bypassed it.

When I look at the FRR, Jim, I see a lot of people in there. Some of them are former astronauts. Is the mission commander involved in this? Are the current astronaut corps involved in the FRR?

COL. HALSELL: Yes. The Flight Readiness Review, the flight crew is represented to the board or the Flight Readiness Review through several different avenues. The Center Director for the Johnson Space Center, the astronauts are hired and work for that person. So he represents their interests. The manager of the Space Shuttle Program --

GEN. DEAL: On the three that you commanded, did you attend the FRR? Were you a part of it at all?

COL. HALSELL: No, I did not; and, furthermore, I think that that's the right thing to do because sitting right behind the board, not at the board table, as the commander of a Shuttle mission, I have my direct and immediate two people I consider to be my reps to the board. That is the chief astronaut, that's currently Kent Rominger; and the director of flight crew operations, currently Bob Cabana. Those two individuals, in my opinion represent the flight crew, the flight crew interests, the flight crew point of view, and that's who I want to be there and to concur with any issues having to do with the Flight Readiness Review.

Now, I think there's a page of presenters here; and I forget if it's forward or backward. But very close to here is going to be the agenda. There we go. You should see flight crew and the left-side halfway down, the flight crew operations director will make his presentation to the Flight Readiness Review board as to the readiness of the flight crew to press forward into launch countdown. At that point in time he's certifying that the crew has been fully trained, is ready to go fly, they have all the procedures, they've been trained on all the procedures, they have all the equipment and training on how to use it to accomplish the mission. Bob Cabana, the FCOD director, doesn't just stand up and say that. In preparation for the Flight Readiness Review, he has a pre-FRR at which the commander of the mission does attend; and it's at that meeting here at the Johnson Space Center approximately three to four days prior to the FRR. It's the face-to-face meeting where the FCOD director queries the crew commander and asks him: Are you ready to go fly this mission? Do you have any concerns? Do you have any issues? So I feel 100 percent justified in saying that even though the flight crew is not physically present at the FRR, they are 100 percent represented in terms of their ability to make it known to anybody and everybody if they have a question.

I guess I feel like I know something in this particular area that I would like to express. There are about 100 meetings that you don't want the flight crew to go at. Because at this point in time in their training, two weeks prior to launch, that's when their highest task loading is. That's what they're trying their hardest to -- it's actually now in the preceding two or three months they're trying to congeal together as a crew, ingrate all the procedures, all the issues, and at this point in time they're typically involved in the terminal countdown demonstration test where they go to Kennedy Space Center and participate in a full dress rehearsal where from the time you wake up that morning until you do the simulated emergency egress out of the vehicle, every step from waking up, suiting up, going out to the briefings, going out to the pad, getting strapped into the vehicle, going through all the procedures of the last couple of hours of the countdown, that's what you're concentrating on. And I would maintain that as important as it is to make sure that there's a chain of communication from the command to senior NASA management, it's also important that we don't overburden them with an unnecessary requirement to be at certain meetings. We just need to make sure they have that communication path; and I believe certainly for all our reviews, including FRR, we do.

ADM. GEHMAN: Go ahead.

GEN. DEAL: I've got one more follow-up, but I can wait.

COL. HALSELL: Did I miss a question?

ADM. GEHMAN: No. Go ahead.

COL. HALSELL: With the presentation? I've kind of forgotten where I was.

ADM. GEHMAN: Page 6.

COL. HALSELL: Okay. Thank you, sir. Let's see we were talking -- the vehicle preparations. Element Acceptance Reviews. And I think I got through the External Tank mate reviews. And we got taken down what I -- I said there were two things that as the launch integration manager I tried to concentrate on the mate reviews. The one we covered in a lot of detail. I called it the paperwork, but it is the close-loop accounting system to make sure that we have positive affirmation, that we have met all the requirements, that the rationale for the waivers that we need to go fly with are in place and still valid.

The other part I'll call the practical side of it. As the launch integration manager, I did not ever want to be guilty of getting caught having gone through a significant milestone such as mating the External Tank to the Solid Rocket Boosters or, later, rolling the Orbiter out of its protected processing facility and bringing it over to the Vehicle Assembly Building, going vertical and mating it and then finding out that there is something not right, something that I should have known about at the mate review or prior that, in hindsight, would have stopped me from going through that milestone. After you mate the Orbiter, for example, you don't have nearly the access that you do in the Orbiter processing facility. So there was a practical side to those mate reviews that it was important to make sure we had full understanding of, also.

Next slide, please. This slide probably does a better job than I did verbally of answering a question earlier of is there a process by which all the waivers, all the FMEA CILs, all the open hazards, any upgrades in hazards or FMEA CILs, that it's all brought forward, what is that closed-loop accounting process that we make sure we're ready to press forward to the next level of readiness. This slide gives you that, and I think we've touched upon some of the important elements of that.

Next slide, please. Now we're talking about Flight Readiness Review, which I think has been done. Let me see if there's anything on this chart that we haven't really talked about. I think the important thing to understand is that the Flight Readiness Review exists at its core for the Associate Administrator of the Office of Space Flight, Mr. Bill Readdy now, to make a final determination if he feels comfortable that we have done everything that we said we would in our requirements to get ready to go fly safely.

Next slide please. This slide should look very similar to the one that I presented two slides ago because it says basically the same thing. We review all the open issues, make sure that our baseline configuration, what we're flying is what we said we were going to go fly and, if it doesn't, that we understand why and that we agree with that. Any significant unresolved problems or resolved problems since the last review and the flight anomalies, any open items on constraints, any and all new waivers and any open actions from the Flight Readiness Review or any of the element reviews that led up to that have to be closed out at this meeting.

At the formal end of Flight Readiness Review -- could I have the next chart please. I'll continue my thought in just a moment.

Here is the participation of the board. What I might have in the backup charts but, if I don't, I want to make it clear to you, that this is not just a table with these people. It is, rather, a table in the center of a very large room with these people surrounded by literally hundreds of other people. Every project, every mid-level and lower-level manager of each project is represented there, each of the contractors, from the CEO down through every individual that he or she thinks is necessary to provide the necessary support. Literally a couple of hundred people at least are attending these meetings and are right there in the same room.

Next slide, please. Some of the logistics are talked about here. We try to hold this review a couple of weeks prior because that's soon enough so that if we identify any issues at that point in time that need to be dealt with, we have some chance of still making a launch date after having satisfactorily resolved those issues. You don't want to do it much earlier than that, though, because you're reviewing a flight for which issues and problems are going to arise in the interim period of time. So that seems to be the right middle ground.

We talked about how all the NASA and contractor personnel are there. One important aspect is that we insist that the whole world of the Space Shuttle Program travel to the Kennedy Space Center and be there in person. You do not participate in the Flight Readiness Review by telecon. You will be there and, if you can't, your designated alternate will be there. It's that face-to-face conversation, face-to-face interaction, that allows you to gain so much more information than you can from a telecon and a voice transmitted to you over the telephone. So the face-to-face nature, I think, is something that's important.

Also not only do we have minutes but we audio- and video-record the proceedings. I know, for example, in answer to

Dr. Ride's previous question, that's one thing in particular I remember was implemented post *Challenger* that we hadn't done such a good job of previously. Maybe we had been as good at analyzing some of our issues, but the documentation of the way we resolved those issues wasn't as stellar as we would have liked. We made sure that problem was fixed, hopefully, after *Challenger*.

MR. HUBBARD: This is a little bit of a subjective question, but let me start off with just a fact or two. You participated in FRRs as the manager of launch integration, and what you described is a big show. I mean, it's a big deal and it's a big room and a lot of people. Somebody once said if you have more than five people at the table, it's not a meeting; it's a conference. So you've got, as you said, a couple of hundred people, more than a hundred people in the room. What do you feel like when you're in an FRR? What do you think the tone is? You know, people have their antennae quivering, looking for issues? Do they feel like their working their way through a series of boxes? How do you feel when you're going through an FRR?

COL. HALSELL: I feel like it is the culmination of a very, very long and involved process. I feel like when we're there in that room, we are putting the important final touches on the work of thousands of people. It is thousands of people. Tens of thousands of people. That filters up at the engineering and manufacturing level, up through the element processes and reviews and the element project managers to what I'll call the mid-level to upper-level management that I participated in my reviews as the launch integration manager. But it certainly wasn't just me. There are a lot of other mid-level managers doing exactly the same thing in their areas of responsibility. And I feel like the Flight Readiness Review is that flagship review at which we have that last and final opportunity to present our story to senior NASA management. And we know that they've been made aware in an interim basis on everything that we've been doing. But I feel that at the table at the FRR board you have the representatives of the right organizations to lend that final not only senior managerial level but that experience viewpoint and common sense viewpoint and asking the straightforward simple questions: Have you done this? Have you accomplished that? Why do you feel comfortable that your assumptions that you made here allow you to make the conclusions that you're presenting to us? I feel that that's the level of inquiry that we get at the Flight Readiness Review, especially on issues which require that at that point in time. So I feel like it is an appropriate and exhaustive review that culminates an appropriate and exhaustive process.

MR. HUBBARD: Just one follow-up on that. People, in general, can feel very comfortable saying things one on one, maybe even in a group of five or ten. I don't know if your average engineer -- and, of course, this is a group of senior managers -- but do you think people feel comfortable raising an issue in a room with a hundred people?

COL. HALSELL: I know that in this particular forum there's absolutely no hesitation to raise your hand, even if you're sitting with your back up against the back wall, against the wall of the building -- and it happens every FRR. And I would simply volunteer to bring forward transcripts and also recordings to back up what I'm telling you. It would be highly uncommon for somebody not to interrupt a presenter in the middle of their presentation and say, "Well, now, wait a minute. How can you say that when we had something else happen two years ago which now seems associated. What do you think about that?"

At some points in time, as the secretariat, if you will, of this particular presentation, my issue has not been with getting full and free participation but just making sure I get it documented. I've got to stop people. I've got to say, "Please come forward. Make your way to the microphone. We need to get this recorded. We need to understand what you're trying to tell us." So my issue has been just to make sure that those types of input are recorded and documented properly. So I do feel that the Flight Readiness Review is a full and open forum.

DR. LOGSDON: If there is that kind of lively interaction at the FRR -- and this is really a question asked out of literal ignorance -- have there been FRRs that have resulted in a decision that the mission was not ready to fly?

COL. HALSELL: Yes, sir. We have a way and we have a process to document that. It's called the Exception to the Certificate of Flight Readiness.

Next slide, please. I'm trying to see if I have it up here.

Next slide, please. Okay. We'll stop right there. What happens at the end of the Flight Readiness Review is that after all the elements have presented, the chair, Mr. Readdy, will typically ask an all-encompassing question. He'll scan the room, try to make eye contact with everybody and say, "Is there anybody in this room who has any information that has not been brought forward that is relevant to making a decision as to flight readiness?" It is rare at that point in time that anybody raises their hand because they should have done it -- and they do do it -- during the element's previous presentation. Nevertheless, Mr. Readdy makes sure he gives that last and final opportunity for anybody to raise a hand and say, "Yeah, there's something here that we haven't talked about yet."

Also during the course of the presentation, prior to this point in time, the elements can take an exception to their Certificate of Flight Readiness, which is basically a way of saying: I certify that I did everything that's required by 8117, also the appendix to 8117, which is my element-specific requirements that I'm signing up to, and also the preamble to 8117 which applies to everybody equally. I am signing up that I did everything and I've closed up all the open issues in a closed-loop accounting fashion with the exception of this one following issue; and that's the Exception to the Certificate of Flight Readiness.

A last thing we do at the Flight Readiness Review is that Mr. Readdy will poll his board members and contractors and they will have the opportunity to say verbally if they certify to flight readiness. Anybody who has taken an exception to flight readiness will, in addition, at that point in time, verbalize that exception, say something to the nature of, "With the exception of issue of working with the Space Shuttle main engine thermocouples" -- I'll just use that as an example -- "we certify that we're ready to go fly the next flight and, furthermore, we will not allow the launch to proceed until we clear this exception to the COFR." You're kind of a good lead-in to the pre-launch MMT because that's going to be the venue at which we clear the exceptions to the Certificate of Flight Readiness, if you'd like me to continue on into that at this time.

DR. LOGSDON: As you do that, can you give me a sense of how often you get to a pre-launch MMT with significant open items?

COL. HALSELL: Exceptions? I would say that -- I'm going to guess. We can go back and get the exact percentage over the last couple of years, but it is not unusual, somewhere between 25 and 50 percent of the time, I would guess, that at least one exception to the Certificate of Flight Readiness is presented, and it's always presented with the conclusion of Mr. Readdy, "We think we can or cannot clear this exception in time to make the launch date that you're considering and therefore we do or do not recommend that you press forward toward that currently suggested launch date."

At that point after the flight readiness poll and everybody's had a chance to say their piece -- and this might play in a little bit to the question that Mr. Hubbard had -- it is tradition that Mr. Readdy adjourn to another smaller room with only invited participants. Usually that's going to be the Flight Readiness Review Board, the prime contractor CEOs, the launch director, the manager for launch integration, and a few other selected folks. In that smaller forum, Mr. Readdy makes it clear that if there's anybody who for whatever reason -- and I can't really understand why -- but if there's anybody who wants to say anything there in that smaller forum that they were not willing to come up with in the larger forum, now's the time and place to do that, before we set a launch date. And it is in addition to that information that's made available to the Associate Administrator at that time that he considers before he presses forward with setting the launch date or not. We can and we do set launch dates with exceptions to the Certificate of Flight Readiness still pending, but only if he has firm understanding and recommendations that we're going to be able to clear them prior to that launch date.

If you like, I'll press forward with the next couple of slides. So we've finished the Flight Ready Review process. The members of the board have been polled. We've adjourned. The Associate Administrator has adjourned and had his opportunity to hear anybody in private and also to decide if he wants to set the launch date. For the purposes of this illustration, we'll say the launch date was set and that we do have some actions and an Exception to the Certificate of Flight Readiness that have to be accepted prior to going to fly.

Let's go ahead now to two days prior to launch. Remember, the whole world came to the Kennedy Space Center for the Flight Readiness Review. They now go away and do their business. Two days prior to launch, we require once again that everybody come back to the Kennedy Space Center. We do it two days prior to launch because we want everybody to have a chance to get back, get in place in plenty of time to set their other job duties aside and to concentrate only on the next safe and successful launch.

Two days prior to launch, we convene the Mission Management Team. The Mission Management Team -- and I believe if we could go to the next slide, please -- I was thinking that I had a slide that showed the composition of the Mission Management Team. Basically if you go back to the FRR agenda slide, remember all the participants, all the people who participated in presenting the information to the Flight Readiness Review Associate Administrator, those organizations and their leaders now become the launch integration manager's Mission Management Team. It's totally appropriate to think that we've not had our review by the very senior level of NASA management and they are now handing off to the mid-level management, with their supervision, the job of launching this vehicle safely within the constraints and within the rules that have been set aside for us to work with them.

So that Mission Management Team convenes and we go through basically the same agenda that we did for the Flight Readiness Review. Every element, every project gets the opportunity to present any interim issues, anything that has arisen since the Flight Readiness Review. If there are any exceptions to the Certificate of Flight Readiness, the full and complete rationale for that is presented there to the same level of rigor that it would have been presented in the Flight Readiness Review.

As the launch integration manager chairing that pre-launch MMT, I felt it was important that I get input verbally and visually and in public from the program manager and from the Associate Administrator at the MMT that they concurred on that FRR COFR exception. In other words, it wasn't just the middle managers now clearing something that previously wasn't good enough for the senior managers to go with. At the end of that MMT, we, once again, poll all the participants to make sure that they are "go" to press forward with the countdown.

From that point on, the Mission Management Team is activated. I know where each of them is. I can convene a meeting in literally an hour's notice if I need to during the launch countdown. The next time we convene will be formally three hours prior to launch, in the Launch Control Center.

If I can have the very last slide in the whole package, I believe it's a picture of the Launch Control Center. As he's scrolling forward -- at three hours prior to launch, the Mission Management Team will convene in this room that you see.

Next slide, please. Here's another view of it. Up and in the dark to the upper left is where the Mission Management Team resides. The larger room is the Launch Control Team and the Launch Control Center under the direction of the launch director, who stands just about underneath that American flag in the center of the room.

It can help you to understand the relationships here as we go through the final hours of the launch countdown. At this point, the Mission Management Team has really done their job and we've handed off responsibility for the successful launch of the mission to the launch director who is directing the Launch Control Team, as long as he or she is able to work within the constraints of the Launch Commit Criteria. This huge, several-volume book which is the what-if of every launch and represents the corporate history of all the problems that we've either experienced or we've had the opportunity to think through ahead of time that we might experience and our reactive measures that we would take to further clarify the problem and our ability to go launch safely.

For practically all the launch commit criteria, when you run through the procedures, it ends up in one or two branches. Either you have resolved the issue as being safe to go fly, clear to launch or, no, we're not sure, you have to stand down that day, unless the Mission Management Team is offered rationale which allows you to press forward and approves it in real time. The Mission Management Team is there to provide guidance if the launch director gets outside the launch commit criteria and needs guidance.

GEN. DEAL: Jim, I just want to get back to in-flight anomalies very quickly and get your perspective because you experienced a very serious one on STS-83 personally. What I want to do is get your perspective on, following STS-83, how the process went, did it underscore the strengths in the program, or were there lessons learned by which we improved the in-flight anomaly process following STS-83.

COL. HALSELL: Certainly I can lend my experience from STS-83, and I think the question that you're asking about the in-flight anomaly process is one of the reasons that we invited Bob Castle, as one of the representatives of the in-flight MMT team, to comment. So I'll hand off the remainder of that question to him.

The issue you're talking about on STS-83 back in 1997 was that after we launched, we experienced an in-flight anomaly concerning some out-of-family and unacceptably divergent fuel cell substack delta volt readings, which is a way of saying there were some increased level of risk that if we were to continue the mission with that fuel cell powered up that you could experience crossover and that could lead to fire and/or explosion. So that was deemed to be an unacceptable risk. It was equally unacceptable to shut down and save that fuel cell and continue the mission to nominal conclusion on just the two remaining fuel cells. So the Mission Management Team came to the conclusion that the only safe and prudent thing to do was to have us close up the lab, prepare to make an early entry back home; and we did so after only four days in space.

The conclusion of that story is that between then and STS-84which, as I remember, wasn't the very next but the one-after-the-one-after flight, on STS-94, they resolved that particular issue, they understood it after they were able to get the fuel cell and do all the testing back at the vendor to understand that, in fact, it had most likely been an indication problem, not an actual issue, and that we could have stayed up on orbit. But there was no way to have known that in real time and I, certainly as the recipient of the safest course of action, I appreciate the action that the MMT took at that time. So I think that is an example of how, when faced with extremely difficult choices, expensive choices both in terms of money, in terms of the manifest having to be replanned for probably several years downstream, but still when confronted with that highly undesirable set of consequences for making the safe decision, the on-orbit Mission Management Team did make that safe decision. They brought us home and we re-flew that mission a couple of flights later with a full measure of success.

ADM. GEHMAN: Okay. Let's let Mr. Castle give his introductory remarks, and we can always ask questions later.

MR. CASTLE: Okay. Well, that does lead into what I was going to start talking about a little bit. I don't have any charts. So you can feel free to interrupt me even more freely than you have already.

As far as the way the real-time team goes, we pick up at launch. Right after liftoff is when the real-time team picks up and starts conducting the flight. I would call flight director the mid-level management team that Jim referred to.

The flight director also has his set of requirements. The specific ones that come to mind are the flight rules and the SODB, which is the Shuttle Operational Data Book. The flight rules is a large book. I didn't bring one around. It's about yea thick for the Space Shuttle. It's what I call pre-made decisions, decisions you've already done your what-if'ing and you've thought about them and you've thought about the situations and the cases very carefully and you write down what it is that you're going to do for each of these particular cases.

In the one that Jim mentioned, the loss of one fuel cell, it says you need to land what's called a minimum duration flight to minimize the length of time we stay in orbit because if you lose another fuel cell, you can land with only one fuel cell but the power-down you have to get into is dramatic and it impacts your avionics in lots of other ways. So we've already gone through that debate. If we lose one fuel cell, we're going to land and we're going to cut the flight short, early.

The MMT got involved with his flight because it wasn't really clear from the indications whether we really had a bad fuel cell or not. So that's where we had to call the engineering guys together to look at that. But if it's clear we've lost a fuel cell, the flight control team doesn't have to consult anyone. We'd say, okay, the flight rules say go do this, so this is what we're going to go do.

The SODB is the Shuttle Operational Data Book. That is another book that is maintained by the Space Shuttle Program. It's a list of how you operate the Shuttle. You can operate the Shuttle with the temperatures on this loop, greater than this and below that. This type of information. Kind of like an owner's manual for your car except, again, it's several volumes. It's fairly thick.

The flight rules are controlled by the Shuttle Program. The final version of all of them are taken forward to the PRCBs for approval. There are several lower-level boards chartered by the program that manage those rules.

People have asked about the safety process. Any changes to the rules, that's done on what's called a CR form, a change request. The Safety folks review those as well, as all the rest of the disciplines -- engineering, program offices, space and life sciences, FCOD, MOD, all the different areas. There is a mid-level board, what's called the Flight Rules Control Board, which is chaired right now by one the deputy chiefs of the Flight Director Office. Again, all of those same organizations represented and then their approved set of rules come forward in a change package to the PRCB for final approval by the program. A very similar process used for the SODB, the way it's managed.

So those are two things that I start off with as my requirements, if you will. There are a couple of other things that are like the flight requirements document which are a mission-specific document. Okay. The other two I just mentioned, that's how you operate the Orbiter, how you fly. The FRD says, well, here's what we want you to go do. We want you to conduct a space lab mission. Here's how long we want you to stay in orbit. Here are the priorities of things we'd like you to do. That type of information.

There is also a much smaller book of flight rules that are flight specific. In that again, you're writing down rules, mainly a priority list, rules that are specific to the payload or the particular operation you have on that flight. Those are flight specific. Also approved by a very similar process and finally approved by the Shuttle Program manager at the PRCB.

Also I want to say that the flight rules are things that when we train people, we take these things very, very seriously. The simulation folks try to put in failures and various scenarios that will stress people's thinking. Okay? They'll break a piece of instrumentation someplace in the simulator. Well, do people recognize what's just failed? Do they recognize the instrumentation they've lost? Do they understand the implications to the flight rules? Have you just had a flight rule violation because of this failure? Sometimes just loss of instrumentation is no big deal. Sometimes you really have a rule violation because we've thought through if I don't have this measurement, then this thing that's really bad can happen to me and there's nothing I can really do to detect it or I've actually impacted the safety of the vehicle because I can't measure something. Sometimes they don't.

Each rule is also annotated. Let me back up.

Jim talked about the top-down hazard process and the bottoms-up failure modes and effects process. Anytime that this hazard control process says we need to control this hazard by a certain operational constraint, we want you to always flip this switch before you flip that switch, a flight rule gets written that says always do it in this order. That flight rule gets annotated that it's a hazard control. So anybody reading the rule book knows that this is a control for a hazard that's been identified for the program. That does a couple of things. The main thing it does for you is when somebody comes along and says I'd like to change this rule for whatever reason, it's in black and white, right in front of you, that you've got to run this by the safety community, you've got to look at it carefully, look up that hazard control, make sure you're not undoing what we carefully did.

They're also flagged from the bottoms-up review. Anybody in the bottom-up review that comes up with a classification of either a Crit 1, 1R, 1S, and 2, I believe, gets classified on a Critical Items List or a CIL. So we flag those rules, as well. It says, okay, this rule is part of the rationale for saying this critical item is acceptable. Again, you get the same type of things that are controlled operationally. If you have Failure A, then you must take this following action to make sure another problem doesn't sneak up on you.

Everybody works really hard to understand those, even though the book is very, very thick. We train them very, very heavily. Our simulation guys are very sneaky. They will put in an instrumentation failure here and a power system failure there and an avionics box failure here and you've got to realize that when you add all those three things up, you've really got a much more serious problem than it seems like. Generally they'll set us up that you need to recognize, hey, one more failure could really be bad. So we work that very, very hard.

Again, I'm just going to keep talking until somebody wants to stop and ask me other questions. Let's see. The basic rule, again, is the flight rules and the SODB -- when I say the real-time team, let me talk a little bit more about who the real-time team is. There is the Flight Control Team, which is led by the flight director who sits in the middle of the room. I don't have a picture, but you've seen the room. There are flight directors there 24 hours a day during a Shuttle mission.

We also appoint a lead flight director who is appointed generally at least on the order of a year before the mission. They oversee not only the real-time mission but all the launch preparation, all the preparation times, all the crew training, everything else that goes on for that prior year. That includes the little first chart that Jim put up, all the little boxes. Either the flight director or some member of his team plays in every one of those boxes throughout the preflight process.

There are other members of what I call the flight control or the real-time team. A very important team is the MER, the Mission Evaluation Room. That is a room that's down on the first floor of Building 30. It is run by the program office and is staffed mainly by people out of engineering and various contractor support -- Boeing, various subsystem contractors. Their function is evaluation. They watch what's going on on the vehicle. They look for more subtle trends, things aren't clear black and white but maybe more subtle problems. If there is a problem, of course, they're ready to be activated, ready to go work any details. Things are never quite as crisp and clean as they look like in simulation. So you always like to have the engineering talent there, ready to go. That group in the MER includes a safety console position, who is, again, always watching what's going on as we operate the mission, understanding all the hazard controls and all the things that have been preflight analyzed.

There's another room in the building which is called the Customer Support Room and that is a program office room. Representatives who report directly to the program manager staff that room hours a day. Again, they're watching out for programmatic requirements. They're there to be consulted. If we get into a situation where I can't do what their priority list says I need to do, they're there to go rework that. “Okay. This just happened. I can't do your No. 3 item on the priority list. What would you like to do? What options would you like to invoke?” So they're there 24 hours a day, 7 days a week during the mission, ready for consultation; and they pay attention pretty well.

There's a formal CHIT system. It's called a CHIT. I don't know what CHIT stands for, but there's a formal paperwork system where if we make a request for information or a request for special analysis, we write down exactly what we want. It is coordinated through the appropriate person who we're requesting this of. Anyone in the building can write such a CHIT. It comes back with an answer, and we don't close that CHIT until the originator agrees that whatever they wanted done has been done and done correctly. Again, it's a very formal process, I think, that works fairly well.

ADM. GEHMAN: Let me interrupt. To carry over the discussion, I asked a hypothetical question, as did Dr. Logsdon, that if *Columbia* had returned safely from this mission, we still would have an IFA of a major foam strike.

MR. CASTLE: We would. It's interesting. People have talked about it from the flight director's perspective. That's one that would come in through the program office and not the real-time team, because the real-time team didn't know the foam came off the tank. It was only the photo analysis folks the next day who came in through the MER who knew something had come from the tank. During the real-time ascent, I'm pretty sure the team didn't know anything about it.

ADM. GEHMAN: That's right. But a day later or a day and a half later, whenever the photo analysis of the ascent, the launch photography was made available and the MER was informed that there was a strike, is it formally classified as an IFA at that time or does it take more paper and more meetings or something like that? I'm thinking MMT now. Are members of the MMT or the flight team, are they aware now that we have something to deal with?

MR. CASTLE: Yes. As soon it came through the MER, it should be made known to the next MMT, whenever that was. MMTs like generally every two or three days. Now, I'm going to have to talk generically here because I had really very little to do with STS-107. I was there for a tiny period of time.

ADM. GEHMAN: That's perfectly all right.

MR. CASTLE: The real-time team, we probably would hear about it from the MER even before it came to the MMT. I say probably because we talk to those guys a lot. We play in their games a lot.

ADM. GEHMAN: Since damage to TPS is a Crit 1 issue, if you had debris striking the TPS and the system was aware of it, I mean, both the flight directors and the MMT personnel, they use the same rules and the same categories and the same processes.

MR. CASTLE: Yes, we do. Sometimes the in-flight anomaly list or the funny list will vary. The flight control team may have different items on their list than the MER has on their list and the CSR has on theirs, which I think is a healthy thing. You get together and decide which ones you want to carry forward on a formal programmatic level.

ADM. GEHMAN: You mentioned that loss of one of the fuel cells is in the flight rules.

MR. CASTLE: Yes, it is.

ADM. GEHMAN: What about damage to TPS? Is there a flight rule for damage to TPS?

MR. CASTLE: I would have to go look it up. I don't think there is one, mainly because I'm not sure what the flight control team could do about it, is the real gotcha there. If you knew exactly where it was, then maybe you could do a little something about it. But if there are any rules, they just tend to go with --

ADM. GEHMAN: So if it's outside the flight rules, then it would be kicked up to the MMT.

MR. CASTLE: I think it's kicked up to the MMT, yes.

ADM. GEHMAN: Correct me if I didn't get this right. Did you say that changes to the flight rules are approved by the PRCB?

MR. CASTLE: Yes, they are. All changes to the generic rules are approved up at the PRCB level. We don't take individual changes. What we do is we process individual changes at the Flight Rules Control Board, which is one board down. Then when we collect up enough that we need to make an actual page change to the book, we bring that forward to the PRCB.

There is a real-time flight rule change process that is in place where the flight director or the mission ops representative, the representative of essentially my boss, John Harpold. Those can be signed off by the flight director or by the mission ops representative; and the actual process allows it to happen without the MMT. That is there so that if there's no time to go have an MMT meeting, you can go do what needs to be done. As a matter of practice, I don't think any of them have ever been signed off without being fully briefed to the MMT; and the number of real-time changes is very, very small.

ADM. GEHMAN: Okay.

MR. WALLACE: Can you give a rough sort of breakdown of the MER in terms of contractor versus civil servant size?

MR. CASTLE: I really don't think I can give a good breakdown because I don't really know. We operate very much badgeless, is the term I like to use around here. Even on the flight control team, the people that I know, I'll tell you their names and their wife's name but I can't tell you whether they're a contractor or a civil servant because it's not really important. So I really don't know about the MER. In the flight control world, simply because I've seen other statistics, it's about 30 percent civil servant and about 70 percent contractor.

MR. WALLACE: So when you say that anyone can write a CHIT, then that includes contractors can write CHITs?

MR. CASTLE: A contractor write a CHIT. They bring it to the MER manager for forwarding on into the system.

MR. WALLACE: Does the CHIT guarantee a certain level of elevation, and what would that be?

MR. CASTLE: Well, it guarantees that it goes through a controlled process. They can write a CHIT. So, for example, someone in the MER could write a CHIT to the flight control team saying I would like to go do this or I'd like this particular information retrieved from the vehicle via a data dump or something. It's guaranteed to go to the MER manager; and if the flight control teams has to do anything, then, of course, the flight director will hear about it. That could be as far as it goes and the CHIT gets closed.

MR. WALLACE: Does the CHIT go to the MMT in an appropriate case, or is the CHIT something that's with the flight control team?

MR. CASTLE: It's within the flight control team, the CSR and the MER. It could certainly go to the MMT if either the missions ops rep or the MER manager or the CSR reps wanted to elevate it to that point as an issue, but CHITs routinely do not go to the MMT.

MR. WALLACE: Generally, could you describe the sort of level of contact, day to day, between the MER members and the real-time flight team?

MR. CASTLE: Fairly routine contact. Generally, at the system level an electrical power guy will talk to the EGIL electrical power guy on the flight control team probably on a daily or shift-by-shift basis. They will talk to each other on voice loops and just say, "How are things going? Were you working anything?" I know they did that back when I worked in that level. At the flight director level, probably daily we talk to the MER managers to see what's going on, or they will talk to us.

MR. WALLACE: Is there a process at shift change, sort of a formal tag-up process, or is that by individual?

MR. CASTLE: There's a formal shift change of the flight control team where we hand over to each other. It's all done on a voice loop where we go around the room: "What are the issues that you're working?" The MER is certainly available to listen to those loops, and I know from experience that they often do. MER is not usually, as an entity, polled during the handover for Shuttles. Last time I did the Shuttle flight was a couple of years ago. Now, on the Station Program, we do poll the MER if they're there. They're not there nearly as often.

MR. WALLACE: I have heard it said that typically the MMT might become involved in a decision if it's sort of outside the book, outside your flight rules.

MR. CASTLE: Yes, that is by definition. I look at the flight rules in a couple of ways. It is pre-compiled list of decisions that have been agreed upon. It's also what I consider kind of my contract with the program manager. If it's inside this book, then he's already agreed that this is something that's appropriate for me to do with the vehicle that really is his responsibility. I'm being delegated it during the flight. So if it's inside the rules, then that's perfectly my right or the flight director's right to go operate inside the rules, within whatever the program has laid out. If it's outside the rules, it needs to go to the MMT. It needs to go to the MMT for approval of whatever I'm about to do, if there is time. There is a caveat, again, since you're flying, if there's not time, the flight director and the mission commander do what they think needs to be done. If there's time to consult the MMT, then by all means you do and you get your approval before you press forward.

MR. WALLACE: Would the sort of real-time flight team expect to be aware of most anything going on between the MER and the MMT as a general practice?

MR. CASTLE: As a general fact, yes. We have a representative to the mission. We call him the MOD. It's really, again, a representative of my boss, being the director of MOD, who attends all the MER meetings, I mean, all the MMT meetings. So anything that goes on in that meeting, the real-time team has a representative there who comes back and consults, talks with the flight director. So the flight director will be aware of anything that's going on in the MMT; and like I say, we not only come back and talk about that, the rep comes back and writes a little short report: Here's what got discussed; here's what the flight team needs to know about what's going on in the MMT.

DR. RIDE: You said that there are no flight rules that cover tile damage.

MR. CASTLE: I can't remember any off the top of my head. I'd have to go look it up.

DR. RIDE: I'm curious whether that would have been a conscious decision by the program. I know the flight rules are reviewed periodically. They are really the bible that the flight team uses to operate. So I would have thought that at some point someone would have brought up should we have a flight rule on tile damage. So I'm curious about what the discussion around that would have been and why there isn't one.

MR. CASTLE: I remember some of that from quite a few years ago. Again, a flight rule is a decision, is the way I like to look at. It's a decision that's been made. So it should be, if you know you have tile damage, then you go do this. If you don't know what to do and there's nothing you can do differently, then there's no point in having a rule. So to my knowledge, we've never had an answer to what you do if you have tile damage, because there's nothing we can do in real time to do much with trajectories or anything else that I'm aware of that would make any difference.

DR. RIDE: Let me ask maybe just a little bit of a different way. You know, suppose we're back in time and 107 is in orbit and the crew happens to look out and sees damage to the left wing. Then it would have been reported, essentially, into the flight control team. I just wonder whether you could describe how that situation might have been handled and whether it would have been handled differently, whether the assessment would have been handled differently or whether the flight control team's involvement would have been different than it was.

MR. CASTLE: How it would have been handled, the flight control team would have immediately reported that up the chain because we're going to need more resources than the real-time flight control team has to do anything about it. I'm sure we would have turned on all sorts of effort in the mission evaluation room to look at possible repair. The trajectory guys would get turned on yet again to go look at is there any other way, anything we can do to fly the vehicle differently because of the specific damage that we see. We would have worked on it very, very hard. I'm sure we would have pulled out all the stops to try and do anything about it; but, again, there are no flights rules on it right now because, as Jim talked about in the very beginning, there are three areas that are simply Crit 1. If they fail, there's nothing you can do about them. Thermal is one. We do not have a flight rule on structural damage either. If you found a broken member someplace, there's no flight rule that says what to do about that. Pressure vessels -- actually we do have flight rules on that. Because if you have a leak, you know, you can take action before whatever it is all leaks out. But structure and TPS, there really aren't any rules on that. But, yes, if we had known about it, we would have pulled out all the stops and done everything we could to try and find the answer, I'm sure. The real-time team would not have been able to do much but implement whatever somebody else figured out.

ADM. GEHMAN: Mr. Castle, is your reporting chain to the Center Director?

MR. CASTLE: My reporting chain, yes, is through the Center Director.

ADM. GEHMAN: I mean, I understand under the flight rules and when you're flying, you're working as an agent of the program manager; but your reporting chain is to the Center Director.

MR. CASTLE: Yes, my reporting chain is to the director of MOD who reports to the Center Director.

ADM. GEHMAN: I don't know whether or not the slide presentation that Colonel Halsell put up there is retrievable or not. I don't even know where they come from. Could we have Slide No. 12? Let's go back to Slide No. 12, which is the FRR agenda. What does the S&MA organization say when it's his turn to speak?

MR. CASTLE: He talks about any program safety paper that is open or any hazards that are open, need to be closed, any new hazards that have recently come into play, even if they've been safely controlled, that type of thing. He gives a report on that and are there any things in the safety reporting system, this anonymous safety reporting system that's been set up, are there any of those that are out there that affect this mission. He talks about and reports on all those areas.

COL. HALSELL: In addition, the safety community, prior to the Flight Readiness Review, has their own pre-FRR review. I believe they call it the PAR. Really that stands for Pre-launch Assessment Review. That's done by all the elements in the project safety organization reporting up to Code Q, which is Bryan O'Conner at headquarters, in association with the Johnson Space Center safety Space Shuttledivision. All of these elements come together to review all the issues. In addition, if there have been any increases in hazards -- and I wasn't taking good notes -- but all of the elements that we've talked about that the safety organization is responsible for being the look over our shoulder to make sure that we're doing our closed-loop accounting system. They report that there. Once again, they report it in the affirmative and also the negative. It's not good enough that they say we don't know of anything; they come forward and say we looked and we did not find anything. In the degree to which it's not possible for them to stand up and say that, then we have an exception to the Certification of Flight Readiness.

GEN. HESS: Let me talk about the MER just a second. I really have a simple question. During the course of the mission, the MER works for whom?

MR. CASTLE: The MER works for the MMT.

GEN. HESS: Now, do you have any direct authority, as the flight director, over the MER?

MR. CASTLE: In general, I can ask them to go work on things. I can send them CHITs asking for things. Do they absolutely have to do what I tell them to do? No, they don't; but, in general, I think it's been rare that if a flight director really wants something with good rationale that they don't jump in and do their best.

GEN. HESS: That's a good lead-in to my other question here. We've heard a lot of characterizations about the preflight FRR process and then the on-mission process that goes; and some would say that one part, the pre-launch part is very, very formal but then it tends toward being a little bit less structured and less formal because you have this book of rules and so the communication is decidedly different. How would you respond to that?

MR. CASTLE: I think it is a little less formal during the flight, for a couple of reasons. One, I think since things are moving much more rapidly, I think it needs to be a little less formal. I think we also, unlike the previous meetings and all the other work in the offices, everything that the flight control team and the MER team does with each other, they do it on voice loops. All of that is recorded so we've got records of everything that's happened. We can go back and sort out exactly what's happened. Things do need to move a little faster when you're flying than when you're sitting on the ground deciding whether you should fly or not. And that's what's built in to allow more flexibility and a little more speed in making decisions. We try to have everybody in the building on a voice loop who has got a stake in the situation and can listen and participate in making the decision right then. The MER manager is listening to what the flight director is talking about doing on the flight loop. In my experience, those people have a remarkable lack of shyness. If they feel they need to stand up and be heard from, they will stand up and be heard from. Is it as formal with normal paperwork going back and forth and signatures and all of that? Yes, it is less formal, considerably less formal in that perspective.

GEN. HESS: Following on with that, we all have in the back of our mind this perhaps Hollywood picture of Apollo 13 and, you know, failure is not an answer and the flight director was the center of gravity in running that particular event, but what you're describing today is that if it's something that's outside the bounds of the flight rules, it's not the flight director that's the center of gravity, it's the MMT.

MR. CASTLE: The MMT is the center of gravity for making all the decisions and deciding which way to go, yes. In terms of actively solving a technical problem, I think you'll find the MER and the flight director are the ones most involved in trying to come up with a solution to a technical problem.

I was not in NASA for Apollo 13. I'm not quite old enough for that, but I do know quite a few folks who were here in that time frame. The movie, as all movies do, simplified things. There were a lot more people involved in working on Apollo 13 than the few that you see on the movie. There was a huge number of people in both the MER and the flight control team that did a huge amount of work, pulling all those pieces together.

GEN. HESS: So then would your expectation as a flight director be that, in the case of 107 where we had the debris strike we know about and then the visual debrief of the ascent video showed this debris and the engineers were beginning to work and decide whether or not that there was a problem with the Orbiter, that the CHIT system and the request for information would have led to a filling in some of the blanks that the engineers were obviously after?

MR. CASTLE: I don't know if it would have or not. Again, I was not working 107 specifically during the orbit phase.

GEN. HESS: I'm just talking about normally. I mean, if you had been, would you expect that process to formalize itself and get into a formal CHIT if the engineers wanted information.

MR. CASTLE: If they wanted information that they felt we could provide, I would expect them to write a CHIT; but again, if they know we can't do anything or know we can't provide the information, they don't spend their time writing a CHIT for it. If they thought we could get it, I would have expected them to do so.

MR. HUBBARD: One question about who's "in" box problems end up in. You described a very rigorous process with a lot of opportunities for people to speak up and simulations that involve all manner of different processes, things that could go wrong and the evil simulator sitting back there failing things on you and so forth. So that captures a way of doing business that encompasses a whole great raft of problems.

Now, looking at the other side, you have damage to the thermal protection system tiles, every single flight. You know, something greater than 30 divots, greater than an inch and more than a hundred total; yet TPS is a Critical 1. It's one of the handful of three things for which is there not a fail-safe and there's no flight rules probably on this. So that problem, whose "in" box does that kind of conundrum, that problem end up in?

COL. HALSELL: We'll tag team this one. The short answer is that it's the Space Shuttle Program manager's job to organize the appropriate response to any and all issues when it comes to making the final determination if we can recommend to the Associate Administrator that we're ready to go fly safely. So if Ron Dittemore were sitting here in front of me, he would he say, "It's my 'in' box" because he's the one who controls the resources and the application of those resources; but at a personal level, I think each and every one of us involved in any way, shape, or form with -- touching the particular example you're talking about here, the TPS, a lot of people have responsibilities which touch upon that, whether it's myself as a launch integration manager and that means that last year the person who runs the interagency imagery working group, the people who took the imagery that first revealed this issue to us, for example, that person reported to me. So that's one area that I'm involved, one of many; or if it's systems integration, the people responsible for grabbing hold of these issues -- and this would be a perfect example of where what one element in project over here is doing or not doing may or may not impact another element over here and we need to make sure we're never guilty of not communicating back and forth. And it's the systems integration group which is responsible of being the accountability hounds to make sure that that kind of conversation takes place. And then you get down to the elements themselves. External tank, if they're shedding foam, it's got to be their primary responsibility for understanding that issue and then dealing with it. If it's the Orbiter vehicle who has an issue with the environment within which their thermal protection system is being asked to operate, then they are equally accountable for raising their hand and making sure those issues are brought forward. You can say that the solid rocket booster element could possibly either be the source of or recipient of debris also. So everybody has a responsibility in this area, and it all goes uphill to the man who's in charge.

ADM. GEHMAN: Well, gentlemen, Mr. Castle and Colonel Halsell, thank you very much for your very, very forthcoming and complete and responsive testimony today. It's very helpful to us. We agree with your opening statements that we're all here for the same reason, to find out what happened to STS-107 and to recommend measures to prevent it from ever happening again. So we all have the same goal here.

You've been very responsive, and your answers have been very complete. We appreciate your patience, and we're going to take a short ten-minute break while we seat the next panel.

Thank you very much.

(Recess taken)

ADM. GEHMAN: All right. Board, if we're ready, we'll resume. I'll ask the people in the room to please take your seats and be quiet, please, so we can get back to work.

The second half of the afternoon public hearing will be looking more specifically at foam events and debris events. We have with us Mr. Scott Sparks, who is the department lead for External Tank issues, and Mr. Lee Foster -- both, I believe, from Marshall, if I'm not mistaken.

THE WITNESS: Right.

ADM. GEHMAN: Before we start, gentlemen, I would ask you to affirm that the information you provide to the board today will be accurate and complete, to the best of your current knowledge and belief.

THE WITNESSES: I will.

ADM. GEHMAN: Thank you very much. Would you please introduce yourselves and tell us a little bit about your background and what your current duties are.

LEE FOSTER and SCOTT SPARKS testified as follows:

MR. FOSTER: My name is Lee Foster. I've been at the Marshall Space Flight Center for over 30 years. Currently I'm with the Space Transportation Directorate. I'm an old technical guy. I've spent many years working aerodynamic design and aerothermal design of the Marshall Space Shuttle elements, and I've been involved with the aerothermal testing of the TPS. Currently to the External Tank I'm kind of a gray beard that they call on occasion.

ADM. GEHMAN: Thank you very much.

MR. SPARKS: Scotty Sparks. Academic background, a Bachelor's in chemistry, Master's in polymer chemistry. I have been employed with NASA since '89. I been working External Tanks since '91. I have worked other composite cryo tankage issues. Just recently I mainly have specialized in the areas of cryo insulation.

ADM. GEHMAN: Thank you very much. We're ready for you to begin. If you have a presentation for us and whichever one of you is first, go ahead.

MR. SPARKS: Let's go ahead and get the first chart up and we'll start, hopefully.

ADM. GEHMAN: We have copies of your presentation. Let's go ahead, and they'll catch up with us when the electrons do.

MR. SPARKS: The objectives that Lee and I want to discuss would include cryoinsulation's purposes and its characteristics in the External Tank, material development and qualification, flight environments, debris history, and some past issues, some efforts, to try to tell about our efforts to reduce debris, and also some recent observances.

ADM. GEHMAN: If you could go through quickly the first two or three. We're really interested in the environment and debris history and efforts to reduce debris and recent observations. Please proceed.

MR. SPARKS: The purpose of cryoinsulation. The main purpose of cryoinsulation pre-launch is to minimize ice formation, but it also maintains the oxygen and hydrogen boil-off rates to acceptable levels. We try to eliminate cryopumping totally and we also try to densify propellant so we can get the maximum mass per the finite volume that we have. Upon ascent, we have to protect the tank from aerodynamic heating as well as plume-induced heating. We minimize effects on the structure of aerodynamic loading, static loads, unsteady aerodynamic load. Also, upon re-entry, we have to maintain a certain breakup altitude window to make sure it doesn't break up too early to scatter some debris over a large area or too late to scatter larger pieces of debris.

ADM. GEHMAN: My understanding is in pre-launch, even if ice formation were not a problem, you still would want to insulate in order to slow down the rate of heating of the liquid hydrogen and liquid oxygen.

MR. SPARKS: That's correct. There would have to be some level of insulation to control that.

ADM. GEHMAN: When you say you try to eliminate cryopumping, are you going to tell us what that is or later?

MR. SPARKS: We will tell you about that.

ADM. GEHMAN: All right.

MR. WALLACE: Mr. Sparks, in following the Board's tradition of never letting anybody get through their briefing, can you give just a general sense in terms of the bipod, the whole bipod insulation structure, as to its purpose as between pre-launch, ascent, and re-entry? I mean, does it really have an important purpose particularly as regarding re-entry?

MR. SPARKS: Upon re-entry? No, it does not.

MR. WALLACE: Using this page of criteria -- pre-launch, ascent, and re-entry -- could you sort of speak to the relative importance of those areas?

MR. SPARKS: Sure. Upon pre-launch you are going to get some level of possible ice formation in that area, and that's one reason why we do have some cryoinsulation in that area. There is some level of rotation that that structure has to go through. So there is some small areas that do not contain cryoinsulation. That's the reason why we have the heater inserted into that bipod to try the minimize that frosting or that ice formation in that area.

As far as ascent, Lee, you might want to talk ascent. There's not an appreciable amount of loading in that area, but you might want to talk to induced --

MR. FOSTER: It's a very complex flow field in that region, which we'll go over in a few charts. We have a ramp on our bipod to lessen the aerodynamic loading on there. So all the TPS works for the ascent part. It's a very massive piece of structure, the bipod fitting itself, and the structure it's on. So during the re-entry part of this, there's really no effect.

MR. SPARKS: Going to the next chart, please. One of the questions we are often asked is why don't you just fly one type of cryoinsulation. We're currently flying four types of foams on there, and it's driven mainly because we've got different environments for different locations of the tank.

In the areas where we don't have high heating, we'll be flying a polyurethane foam; and the two types of polyurethanes are the BX-250 and a PDL-1034. And on the LOx tank, we'll fly a polyisocyanurate material, which is a little bit higher heat-resistant material; and that's the NCFI series, the 24-124 materials. The thicknesses vary upon the tank also, but the thicknesses are driven primarily to minimize ice formation and if there is additional thickness required because of re-entry, then that's added there upon that design.

Next chart, please. Me personally when I'm working a foam issue, I like to think of the issue in four terms as far as structure when it comes to working a foam issue. First is a polymeric structure, and very quickly this is a polyurethane or a modified polyurethane, polyisocyanurate materials that we're talking about. That forms the basic backbone of the polymer and generally determines the strength of the material. It also determines the strain capability at cryogenic temperatures. Polyurethanes are extremely compliant at cryogenic temperatures, and that's the reason why we use these materials. There are very few materials that can take that strain.

The next level of structure would be cellular structure. Generally, it's very important to at least understand your cellular structure. We'll look at a few pictures here. As you see the sort of semi sort of random behavior of those cells, certainly they are important in that some of your thermal insulation characteristics are driven by your cell structure.

Knitline geometry. This material likes to be sprayed in fairly thin passes. In other words, if you spray it very, very thick, all at one time, it tends to pull away from itself upon cure and forms internal stresses. So it is better to spray in passes. So what that does is once you spray a pass, it skins over on itself and the subsequent pass forms what's called a knitline.

There in that bottom picture is radiograph of some materials that have been sprayed on to a substrate. That's complex geometry in the intertank region. That's a rib geometry. But the radiograph magnifies the appearance of the knitlines, just to show that feature.

The strength can change due to that knitline structure. In the region of concern that we've been talking about the past few weeks, the bipod area, especially when you manually spray an area, it's very hard to determine from part to part an organized or a specific structure as far as the knitline geometry. On the automated sprays, the barrel sprays on both the LOx tank and the hydrogen tank, you have more of an order to those knitlines.

Finally, substrate geometry. A flat panel with foam on it, that foam's going to react differently if that is sprayed upon, say, just a rib geometry, for example. We found in some in-flight anomalies a couple of years ago taken on a thrust panel that that material would perform nominally on a flat panel but when applied to a ribbed situation that the expansion coefficient pushed up and the stress became great at the tops of the ribs and contributed at least to the loss of that material in that area. If that had been on a flat substrate, that effect probably would not have been demonstrated.

Next chart. Again, here's some photographs of some foam blown up. You can see in this picture here the story is mainly the cell structure. You can see the semi sort of random structure, what I call the football nature. The rise direction is going vertically, and you can see that it is preferential to rise direction. You can also see the bar there, being 100 microns, and the picture just a little bit lower is 500 microns. It's the same photograph blown up. Fairly small cells. That is one of the key elements of this foam is that it's close cell and that it does have a very low thermal conductivity gas in those cells.

What you're looking at are struts that form on the outside of the cell in what I would call windows that maintain that gas in that cell. Again, polyurethane is a very compliant material. Also foams, all of us here are sitting on polyurethane foams right now that's just not a rigid foam. The chemistry of that material is just a little bit different to make it flexible. So it's a very compliant material if formulated in that fashion.

Knitlines. You kind of have to look closely there to see that knitline, and that's a 100-micron bar. So knitlines can vary in thickness, depending upon the spraying conditions and also the time allowed before you spray the next pass. That was just a picture to show you how thin that knitline can be and also how it is knitted, more or less a continuous polymer running through that area.

By the way, this is material that has been pulled off just recently from the ET 120 dissection that we're doing out at Michoud. This is just a random anomaly that I picked out of the laboratory and showed. We have rollover phenomena; and that phenomena occurs generally when you have, I guess, a complex geometry underneath it that you're spraying. The rollover, when you spray foam, it will push up on itself and start to rise; and if you have a complex geometry, it won't fold over on itself, much like a wave in the ocean will fold over on itself and it forms a small void.

Can we hyperlink that? Can we show that video, please?

Talking about the relative hardness of the material. This is going at approximately 700 feet per second, which is visco-elastically. You see the foam. That's a 3-inch piece of foam, about an inch in diameter. BX-250, the material used in the bipod.

If you can click that again and show that again, please. Maybe it has to quit before you click it again.

Undoubtedly, you see the flexibility of those struts and that material able to absorb that energy, and then finally the shock wave does break it apart. We haven't looked at those materials yet or at least I haven't seen the analysis, the electron micrographs of those materials, but we're going to look at that and I conjecture that those windows in that cell that we're looking at are probably burst but the struts may be somewhat maintained. So the material looked like it was still holding together somewhat, even though the pressure in the cells probably were blown out.

That was a load cell. I think that was a steel load cell. They were trying to understand the amount of energy in that material.

MR. HUBBARD: Two questions here. When it says chilled, how cold is that?

MR. SPARKS: I believe they submerged in liquid nitrogen and it was a best effort to take the foam bullet, put it in a sabot, and then fire. I believe it was around --

MR. FOSTER: Minus 38 degrees or something. It was only chilled. It wasn't cryogenic temperatures.

MR. HUBBARD: C or F?

MR. SPARKS: F.

MR. HUBBARD: I mean, minus 38 --

MR. SPARKS: Fahrenheit.

Okay. Go back, please.

MR. HUBBARD: And the little stripes in what looked like five segments along your column there, are you highlighting the knitlines, or is that something else?

MR. SPARKS: That was half-inch gradations, just showing that was a half inch.

MR. HUBBARD: Oh, to see the compression.

MR. SPARKS: Correct.

Next chart please. Very quickly, this is a top-level chemistry view. One of the things that we're talking about is polyurethanes in the form of BX-250. On the side wall we're talking about NCFI materials; and that's a polyisocyanurate, which is a modified polyurethane. The difference between the materials generally can be explained here. You have a general polyurethane reaction occurring between a diisocyanate polyol. It forms a very flexible urethane linkage.

On the lower half of the chart, it describes the first reaction for the polyisocyanurate. It's a trimerization reaction that then undergoes urethane reaction with its R components. It's a little bit more ring structured which forges a little bit higher heat resistance. This comes into play when we look at processing conditions. One of the reasons why we use polyurethanes in some locations is that we can spray it out on a floor because the substrate does not have to be heated. For polyisocyanurate processing, the substrate has to be heated. One of the reasons why is because this reaction here is a little bit slow in kicking in. So you have to give it a little bit of help thermally to kick in to start the reaction.

Next chart, please. Again, a little cartoon here showing the constituents of NCFI. I just chose NCFI as an example. You have a Component A and Component B. The Component A is the isocyanate, Component B is a polyol and all the other ingredients such as blowing agent, flame-retardant packages, surfactants, and catalyst packages.

ADM. GEHMAN: Is this a good time to talk about blowing agents, or are we going to talk about it later?

MR. SPARKS: Let's go just a little bit more.

Next chart, please. This is really an eye chart, but it is in your package and I wanted to include that so it would be in your package. Maybe what I just want to speak to is the blowing agent issue. I listed the HCFC material on the top, and the CFC material is the second material in the top row, materials that have been transitioned away from.

One of the questions we're asked often is, generally, from a material properties perspective, what happens when you transition from an HCFC to a CFC. Generally, what we've seen and what this chart points out fairly well is that at room temperature and elevated temperatures your tensile properties and compression properties went down a little bit only on your NCFI series of materials. The other materials, the PDLs and the BXs and also the cryogenic properties of the NCFI materials seem to be equivalent or superior with the HCFC materials, blowing agents.

MR. HUBBARD: One question before you leave this chart here. I think I'm correct in saying that this column here is the bipod ramp material, right?

MR. SPARKS: That's right, Mr. Hubbard.

MR. HUBBARD: Specifically, BX-250?

MR. SPARKS: That's right.

MR. HUBBARD: One of the issues that people have been debating is how heavy a piece it was that fell off the bipod ramp and hit the wing leading edge. I notice that there's a range here and the density which, of course, tells you how heavy it is; but you have a typical number. How typical is the typical number? If you were to go take 15 samples, would they all be very closely grouped around 2.4 or are you going to see this full spread which is something like, you know, a 40 percent spread?

MR. SPARKS: Right. If 2.4 was typical in an area, the foam is going to give you variation. It's going to give you variation in mechanical properties. It's going to give you variation in the density. I would presume a 2.2 to 2.6, that much of a spread; but that's just a guess, Mr. Hubbard. It might span that range. I don't think it's going to go down to 1.8 all the way up to 2.6, but it's going to come close probably. I think Lee's got a chart also that might discuss that a little bit also.

Next chart, please. Moisture absorption. I did pull some limited information, but I did not want to present that. The bottom line of the story is the material is fairly moisture resistant as far as to absorption. This is a study that was done, again, back in '98, I believe, done upon 1-foot-by-1-foot panels that had a substrate. They were sprayed upon a substrate. So they were exposed on top in accelerated exposure chambers, at 7 days for 125 degrees F, 95 percent relative humidity. You can view the amount of moisture gained for the NCFI 24-124 at .12 percent. The BX material's at .16 percent; SS, .42 percent; PDL, .83 percent.

Personally again, in working with a lot of foam materials and measuring those foam materials, those essentially are about the same because you're going to see a lot of scatter in the data that you receive lot of times from those materials. It would be hard for me to say that there is a difference here. I tried to go back and find the numbers of samples that each of those numbers were up against and I couldn't find that, but I would guess that the range certainly you couldn't differentiate between any of those as far as moisture gain.

MR. HUBBARD: Do you know of any studies done, instead of at 125 degrees, closer to freezing?

MR. SPARKS: No. We're looking at that. We've been made aware of that. We're going to look at that and investigate that possibility. We know that possibly that might be linked to the chemical formulation, the ethylene oxide or propylene oxide ratio. We're also going to try to figure that out and see if it's applicable to our cryogenic situation.

One of the issues, though, Mr. Hubbard, the tank very rarely would be at 32 degrees, being at Florida. Say, if it was frosty during loading, it would be for a limited amount of time; but still we're going to check into that and make sure we run that down and possibly set up some tests to look at that.

Next chart, please. Actually this is a chart that I presented a few years back, just a high-level chart of some of the things that we do when we go off and try to look at qualification. Physical properties, we look at bond tension. In other words, material that's been sprayed on a substrate. We test it all the way down from cryogenic temperatures up to positive 300 degrees F. We do a flat-wise tension, which is blowing ice, just looking straight at the foam material. We do plug pulls, density, and compression on those materials. To give you a rough feel, probably maybe several thousands of those tests in that test series.

Mechanical properties. Cryoflex is a very severe strain, checking the ultimate strain capability of that cryogenic temperature material. Monostrain is getting design information as far as modulus, and we do that at cryogenic temperature and elevated temperature. We do some shear and some Poisson ratio. Again, a lot of these pieces of data are feeding into analysis; and we're doing, again, a swag, thousands of those.

Thermal properties. Thermal conductivity, we take it down to cryogenic temperatures and measure it all the way up 200 F. We look at the oxygen index. In other words, what percentage of oxygen. Is it flammable. We look at the flammability as far as its flame capability of extinguishing itself. Specific heat and TGA, more or less looking at when the material starts to lose its weights as you increase the temperature. We do aero-recession and hot gas wind tunnel and we do thermal-vac, which is a synchronized radiant heating and vacuum profile. We probably do hundreds of those tests.

Then we do major flight acceptance tests that are more or less all up config tests. Of course, you don't do as many of those, but those ultimately receive a little bit more visibility and really have a little bit more fidelity as far as representative of the hardware.

Next chart, please. A processing chart. Again, this is for BX-250. The message for this chart is, looking at the two bipods, the ET 93 -Y bipod and the ET 115 -Y, that did shed debris recently. We went back and looked at the processing conditions to see if there was anything outstanding about those. To this date, we haven't seen anything that's really sticking out. I very quickly put in a processing chart here. The white box -- you can barely see it on this chart -- is more or less the invisible processing area that we can conduct our activities. They're grouped in that certain area there because that generally is the temperature and humidity inside the factory at Michoud.

Qualification tests have been run at the corners of the box, and you generally get about as much variation from a sample down here and a sample up here as you do if you get two samples in the middle. Again, foam sometimes can be quite frustrating in terms of data analysis because it does have certain variations in the material.

Next chart, please. Again, looking at mechanical properties of the past few bipod ramps and looking at the 112 and 107 bipods. Both are falling in the population average, if you will, of those I think being sprayed. Almost going back to ET 106 through ET 116.

These two points here, the chart is not very clear on that. Again, this kind of demonstrates the variability sometimes we'll see in the material. These two low values were pulled, and requirements are that you pull right next to it to see if it was just a variation of material. I believe on this one it's a 49, and on this one it's a 60, pulled right next to it. That's one of the issues that you have often with performing plug pulls is that you will get a bad plug pull where the value will be low, but right next to it, it will be just fine. If you dissect the material, it looks just fine.

Next chart, please. We have these charts for all the different materials; and this is just kind of walking through, I guess, more or less a day in the life of a person that follows cryoinsulation. It's fairly frustrating as far as obsolescence issues and as far as other issues mandated from other organizations. BX-250 to SS-1171 to BX-265 is a good example. Originally, of course, BX-250 was the original ET material chosen for ramp and closeout applications. In '93, the CFC 11 blowing agent manufacture was discontinued. It was because of the accelerated EPA date. In '95, the SS-1171 material was chosen to replace the BX-250; and we secured the available stock of CFC 11 to use with the remaining BX-250 that we had.

In '95, we had a flame retardant issue. We have to obtain some material from overseas to back-fill. In '98, production issues identified with the use of SS-1171 sort of making us scratch our head. This is about the time that we were qualifying all new materials going from HCFC to CFC, I'm sorry, to HCFC materials.

What was occurring with these processing anomalies were the SS material was processing just fine in component shop, a little bit more control of environment; but on the floor it was not processing as easily. In 1999, again, SS was continuing to have issues; and we discontinued that material in 2000. Mondur Dark was the type of polyisocyanurate used in BX-250. It was phased out of production. In 2001, BX-265 is qualified to replace BX-250. Stepan is the manufacturer of BX-250, and that's the BX-250 material with a HCFC 141b blowing agent. And we implemented in 2002, 2003, EPA phase-out of HCFC 141b. A waiver approving that exemption was granted just recently, March the 5th, 2003. That's generally just the life and times of somebody trying to work these issues with the materials sometimes when the raw materials are becoming obsolete.

ADM. GEHMAN: BX-265 doesn't appear on your generic tank. It's used in the acreage and replacing BX-250 now.

MR. SPARKS: That's right. I didn't really label it very well. The previous tank, that was ET 93 configuration. On that real big eye chart, you'll notice the transition in the upper right-hand corner from BX-250 to SS-1171 to BX-265 did include that material there. So that material will be phased in and used in the areas where BX-250 is used now.

ADM. GEHMAN: And the shift of blowing agents back in '93 was done strictly to comply with EPA regulations, not because there was a better blowing agent or your blowing agent wasn't working or anything like that.

MR. SPARKS: That's correct.

All right. I'm going to hand the ball off to Lee here.

MR. FOSTER: Okay. Scotty's first chart said the TPS had to take the flight environments and protect the structure. This is a sketch showing what some of the environments are.

External tank, as also the rest of the elements, have to take the aerodynamic loads and the heating. We show this as hot spots, like on the front where you have high aerodynamic heating. On the back end of the tank, you have plume-radiation heating and plume recirculation. You see in front of the Orbiter and SRB noses that there are shocks generated that all impinge in the intertank region and even some of those shocks coalesce and they're shown as separated flow and recirculation region right ahead of the Orbiter nose shock. As you can see from this, a lot of the areas on the intertank and specifically in front of the bipod are a very complex region.

The next chart is a computational fluid dynamics chart that basically we borrowed from JSC, and it is to show the complex flow field. I'm not really going to go too much into that. I'm just going to let you look at the pretty lines and see that the flow is going every which way.

ADM. GEHMAN: Can you point out if there are any shock-shock interfaces or reinforcing places in here?

MR. FOSTER: Well, yes, I can. The previous chart showed the shock coming off the nose of the Orbiter. It's impinging there. The SRB on the other side here has a shock coming through this way. You can see the flow from the nose of the left-hand SRB here. So it all coalesces into this area. You can see that we're getting some vortices formed here and it also has the LOx feed line here that influences the flow.

ADM. GEHMAN: And you point out the left bipod ramp. The density of the lines indicates more stress, I guess, or aerodynamic pressure?

MR. FOSTER: I apologize for not being able to answer well the CFD. I can barely spell it. I told you I was an old technical guy, and this is a lot of new stuff here. But, yeah, I guess it's like watching the weather. When the lines are close together, it's higher pressure there. I can get back with you with the specific numbers there.

DR. LOGSDON: One more question. Foam came off on 107, about 81 seconds into the mission. Is that the Mach speed at 81 seconds?

MR. SPARKS: Yes.

DR. RIDE: Just one more. From this picture, you know, we're looking more directly at the left bipod. I can't quite tell whether the flow around the right bipod looks the same. Does it, or is it just the perspective?

MR. FOSTER: No, it is different because of the presence of the feed line here; and this particular solution did not have real high fidelity geometry upon the right bipod. They're working that. This is a chart that's used for illustration here.

DR. RIDE: Okay. So you would expect the flow to be the same around the left bipod and the right?

MR. FOSTER: No, it's going to be different. We can get those numbers for you, but what we've shown with our flight history is that if we have good foam and it's not affected by, I'll say, some of the hypothesized failures we have -- and I'll show you later on -- both sides take the environments. We'll get into that in just a little bit.

Next chart, please. What we're doing here is looking specifically at foam loss and debris. There are three things that we, on the ET side, look at to quantify the debris for us. One is the ascent photographic coverage. You know we have hundreds of cameras watching the ascent. We have groups at each Center that look over those things and try and identify if there is debris coming off at whatever times they can identify it. We also have the separation photos that are in the umbilical well cameras. These, of course, don't come back until the Orbiter does. We also have several occasions where the crew has the hand-held cameras. Those are usually not quite as much information that we get from that 'cause it's a while before they can take those. Also, after each flight, there's the Orbiter tile damage assessment; and we look at all of those things to try and quantify what kind of debris we're getting from the tank.

There were some additional methods lately. We had several SRB cameras to look at the intertank region. That was a result of IFA 87, which I'll talk about in just a little bit; and we had one flight where we put a camera on the ET. It was really a very neat view until, at separation, the BSM clouded the lens.

Next chart, please.

MR. HUBBARD: Before you leave that one. No. 3 there. Post-flight Orbiter tile damage. Is it your understanding that the tile damage that is seen every flight mostly derives from ET debris?

MR. FOSTER: Not really. Let's go to the next chart, and I'll answer it there.

This is the number of hits on the lower surface of the Orbiter. There's also charts for the side and the top and all that. The blue here is the total hits on the lower surface, and the red is the hits that are judged to be greater than 1 inch in diameter. There's some rather large numbers, you know, of total hits. I guess we can average somewhere in here. A lot of those are very small, that are due to other things than ET foam debris. Like on the aft end of the Orbiter the heat shield, you have a lot of ice forming on the SSMEs and the aft heat shield and you get little dings, lots of those. There are areas where you get some ice, I guess, from the attach points, the Orbiter ET attach areas. Usually there's a lot of dings around there. It kind of goes to a baseline number somewhere in the 13 to 25 hits greater than one inch, which I'll again get to in the next chart, if we can go to that.

What you'll see here is where we had ET debris events. We had some higher numbers. I'm slowly getting around to answering your question, sir. This is the same data as was on the previous charts, only this is the hits greater than 1 inch. First let me talk to this one at the very top. That's STS-27R right after we got back to flight. That was a very large number of hits. Most of that was caused by SRB debris. There was a large investigation that worked that, and so I'm really not going to talk to that particular one. We will talk about these areas where there are large numbers that we say are correlatable with the ET debris. Then the rest are very small numbers, relatively speaking. So, yes, we can tell when it's ET derived damage; and I'll show you how we have correlated some of those and what we've done about it.

GEN. HESS: Before you move on, have there been any instances where you have foam striking on the RCC that have been documented? This is just tile acreage mostly, is it not?

MR. FOSTER: I really can't answer that question. We use the data that's provided by KSC, the Orbiter damage maps; and we're looking at the numbers here. So I'm not the right one to answer that question.

MR. SPARKS: To my knowledge from the laboratory perspective, I've never been informed that the RCC was damaged due to foam debris. That's not to say that it hadn't been. I've just never had knowledge of that.

MR. FOSTER: Next chart, please. This is an umbilical well photo from STS-26, where we had a very large number, 179 hits greater than an inch. Let me point out that there is an area around the flange, extending up into the intertank and then around the feed line fairing, where we have what we call two-tone foam. This was initiated when we went to the lightweight tank series, and it was an attempt to reduce the environments by filling in stringers with BX-250 foam. Then we could spray a smooth layer of the CPR on top of that and reduce the environments. That worked quite well, and these data start at the first lightweight tank. It worked quite well until STS-25. And then STS-26 -- 25 we did not have umbilical well cameras; 26, we did. These were flights that were three weeks apart. This is where we had a sub-tier vendor make a change on the isochem material that we put between the two layers of foam. And this caused a reaction and got a blister area, a void that then popped off during flight. You can see there some rather large areas where we had divots come out.

So after this flight, we went to a process of drilling holes in all of these two-tone areas, on 3-inch centers, in order to relieve the pressure so the foam wouldn't divot. And it worked quite well. As you see, the numbers went down.

ADM. GEHMAN: Is this an ET separation picture?

MR. FOSTER: This is ET separation, umbilical well camera.

ADM. GEHMAN: Oh, but it's from the umbilical well, not from the crew hand-held camera.

MR. FOSTER: Yes.

DR. RIDE: Can I just ask a question on your numbering system? STS-26 was return to flight?

MR. FOSTER: No, that was 26R. I do have to apologize here. What I did was sorted these data by ET number; and as you're well aware, the numbering system was really messed up. So this is not in chronological order. Case in point: 27R is return to flight, and 27 was way before. So although on this chart those data would be together, you know, chronologically they're far apart.

DR. RIDE: So could you just tell us what flights these referred to?

MR. FOSTER: This is STS-26 -- I've put down the STS number; and a little later on, where I talk about some of the efforts we made to reduce the debris, I'll talk specifically the ET numbers here.

DR. RIDE: I just needed the STS number. The flight labeled STS-26 --

MR. FOSTER: Yes. That's correct. That is STS-26.

DR. RIDE: That is STS-26, the return to flight?

MR. FOSTER: No, ma'am. STS-26. There's R. In our wisdom, we've flown an STS-26 and a 26R.

DR. RIDE: Okay. What's STS-25?

MR. FOSTER: STS-25 was flown in June of '85 and STS-26 was flow in July of '85; 27, in August of '85. So there were three of them right close together there; and then, as I said, the 27R, this one up here, wasn't until December of '88. So I apologize for not putting these in chronological order.

DR. RIDE: So the one you labeled STS-25 is actually before the *Challenger* flight.

MR. FOSTER: Yes.

DR. RIDE: So it had a different designation then.

MR. FOSTER: Yes, it is.

DR. RIDE: And it was not the 25th flight.

MR. FOSTER: Right. STS-25 is close to -- it's the early 20s, I think. It's hard to keep up with. I'm sorry. I'm going to redo this chart with everything done in chronological order.

DR. RIDE: It would just be useful to be able to track these back to the actual flight numbers.

MR. SPARKS: We can get that.

MR. FOSTER: Go to the next chart. This is 32R, which is a return to flight. This one, you see we're missing a big piece of foam there that people have looked at and said, oh, that's a bipod missing. What you've actually got is -- this is, again, the two-tone foam area. We see that we have lost the foam in that two-tone area and it has taken the first part of the wedge from the bipod. So really the bipod foam loss here at the front edge is a result of another divot as opposed to being, quote, a bipod foam loss. This one here, I've got it shown 13 hits greater than an inch caused by this amount of foam coming off.

MR. HUBBARD: Would you just remind us why 1 inch is an important number?

MR. FOSTER: That 1 inch is -- I guess the system came up with that break point because they were getting very large numbers of total hits. So they wanted to come up with some criteria of things they should look at for trending so that they might want to take some action if they saw a large number.

MR. SPARKS: I think they had numerous very small-speck hits they didn't attribute to possibly debris falling from the External Tank. So they wanted another classification, and that's where they drew the line. Of course, it was easiest to say 1 inch.

MR. FOSTER: Next chart, please. This is STS-47; and as you can see, there was one large divot here, a bunch of smaller ones, and even something on the outboard side. The purpose of putting this chart in here is twofold. One, the damage result was only three hits greater than an inch. What I'm attempting to show here is that it's a time-dependent thing, depending on where you lose the foam. Now, going back -- I don't have any information of exactly what time that came out, but if it's early in flight or later in the ascent flight, you're dynamic pressure is not at its maximum and so you don't put as much momentum on a piece coming off and therefore it's not going to have as much damage to the Orbiter. So there's a lot of people studying the transport of debris; and it is a function of when it comes off, how much damage it can do. STS-112, we had a very large piece come off, but it never hit the Orbiter at all.

By the way, this second point here is that even though there were only three hits greater than an inch, an IFA was taken on this tank, to go try and investigate why you're losing foam.

MR. HUBBARD: Just to be sure I understood that point you just made, which I think is an important one, is that it depends on when in the flight the foam shedding occurs, how much damage a given piece might cause?

MR. FOSTER: Yes, sir. Both from the trajectory -- the transport over to the Orbiter. Because the flow field is constantly changing and then also the amount of entrainment you can get in the flow and therefore the more damage potential.

MR. WALLACE: Sir, you said on STS-47 an IFA was taken.

MR. FOSTER: Yes, sir.

MR. WALLACE: Was that the decision or recommendation of the External Tank project then?

MR. FOSTER: Most IFAs, I believe, are a system call which the ETs along with everybody else is in the decision-making process. I don't think I can say that it was something requested by the ET here or whether it was just the system said, you know, this is a big piece of debris, we need to go look at it. I really can't answer that question.

MR. WALLACE: Do you have any further recollection as to whether it was a constraint to flight or what actions were taken?

MR. FOSTER: I know it was not a constraint to flight. All of the debris that we have here has been judged by the system as not a safety-of-flight issue but a maintenance issue; and we have all in the past been involved in those decisions. Rightly or wrongly, they were all declared a maintenance item and not a safety of flight.

MR. WALLACE: Might affect the turn-around of the Orbiter.

MR. FOSTER: Yes, sir.

The next chart is STS-50. This one had hits greater than an inch, but it was one where we lost the bipod but, again, in this one it was initiated in that two-tone region. Now, you've heard a lot of the two-tone. After STS-50, we changed away from the two-tone; but this one is one we looked at recently where we tried to get a solid model to show what the dimensions were. The weight calculated for this particular area, which included the front of the ramp and a little bit of the two-tone area, was about a pound.

MR. HUBBARD: When in flight did this one occur? How many seconds after launch?

MR. FOSTER: I don't have that information.

MR. SPARKS: I don't know if we know that, Mr. Hubbard.

MR. FOSTER: We asked the photo guys to go back and look at all of these; and, quite frankly, I haven't seen the results of that yet. I think, though, that they said they did not see this piece come off during flight.

MR. WALLACE: So in some cases you only know that it happened when you see the separation?

MR. FOSTER: Right.

MR. SPARKS: I think one thing that they're additionally doing now also is if they came back with a, well, we did see it come off, I think also they're going out and saying, well, this is the window that we did not see it come off also, which would be helpful. And I think they're working that right now.

MR. HUBBARD: Maybe this is a good point to ask a different version of my earlier question. If you go back to -- you don't have to go back on the slides. But on Slide 17, the data commonly available for assessment. You have ascent photos, Orbiter separation photos, and post-flight tile damage. If you were to look at all the flights and say what is the preponderance of the data that you're using to assess what goes on, which one of those three would stick out as where you have the most data?

MR. FOSTER: Well, basically the Orbiter tile damage, you know, we have that on every flight. It's easily done. It's numbers that you can bean-count. The umbilical well cameras, sometimes you're launching in darkness and so you don't get good coverage. We have one Orbiter that doesn't have the umbilical well cameras. So that's some information that is -- I don't even know what percentage of the time we get that. It's over 50 percent but by no means 100 percent.

MR. HUBBARD: And you may or may not happen to catch it as it's coming off.

MR. SPARKS: Right. Or the camera may be out of focus or a cloudy day.

MR. HUBBARD: So is it a fair statement then that, by and large, we know what we know about the damage that External Tank debris-shedding causes, by virtue of looking at the tiles after the fact, with some other data tied in?

MR. SPARKS: Right.

MR. FOSTER: We look at whatever we can to get information.

MR. HUBBARD: So do you feel then, given where the data comes from and how much you have got, you feel fairly confident, then, that there is this direct connection between the tile divots, at least the larger ones, and the External Tank debris?

MR. FOSTER: Yes.

MR. SPARKS: Let me take a cut at that because the tile count, if you will, when it gets back, is the one thing that's always consistent. You're always going to get that data, but it is confounded. That's the reason why it's so important to get ascent photography or separation photography. You know, the tile count is confounded. So any of that data that we can get upon ascent, upon separation, on crew hand-held are value added. Very much so.

DR. RIDE: Could I just ask, right along those same lines, can you characterize roughly the number of flights or the percentage of flights where you've actually had ET-sep photography or ascent video that clearly shows the bipod ramp? What I'm getting at is: How do you know what percentage of flights foam has really come off the bipod?

MR. FOSTER: I don't know that we can make statements with certainty. All we can say is that by looking at all these resources we have, we can see things like this that give us that information. The ones we don't know about, it would just be guesswork. However, a lot of them that we could not see, we also did not have big debris damage. So I'm not sure if there's any comfort in that.

GEN. DEAL: Rephrasing her question a different way, do we know how many we have seen either through the separation or hand-held? Because we've got the ones at nighttime we definitely didn't see and we've got the ones where we didn't get the camera shots out of or where the tank had rolled around. Do we know how many we have seen?

MR. SPARKS: We've got that. I don't have that, General Deal, on top of my head, but we've got that. I have seen it, but I just can't remember what it was.

GEN. DEAL: 'Cause we throw around terms, you know, four out of 112. It may be a lot more than that 'cause we can't confirm that.

DR. RIDE: Right. That's what I was getting at in a pretty badly phrased question. How many tanks shed debris where it could have come from the bipod but we just don't know because we didn't have the photography.

MR. SPARKS: And I think in between 112 and 107, I believe 113 was a night launch, if I recall correctly.

MR. HUBBARD: If you expand the question to the whole External Tank and all of the foam that you've got there on the acreage, is it fair to say that if you look at any one of these plots that go up through more than the 100 flights there that all those little red triangles probably, or many of them, probably relate to the External Tank?

MR. SPARKS: I would say the majority of them do, Mr. Hubbard. That's Scotty speaking, though.

MR. HUBBARD: Okay.

MR. FOSTER: Let's go to the next chart. Well, before then, let me answer that question, the previous question just a little bit more. We think we have evidence of five flights, I think, where the bipod has come off. Of those, the ones I showed on the previous charts, we don't see the bipod as being the initiating mechanism. That two-tone foam was. So really it's kind of, well, we've only had a couple that we know of that were bipod alone.

This chart shows STS-87, which was 109 hits. This one was the initiation of the IFA 87, it was called, because we had a lot of popcorning type foam loss on the thrust panel side of the intertank. That was worked very hard through the investigation procedure and it has been handled with the application of thousands of vent holes --

ADM. GEHMAN: This was the first flight after the shift of blowing agents, right?

MR. SPARKS: It was the second flight.

ADM. GEHMAN: After the shift of blowing agents?

MR. SPARKS: Right.

MR. FOSTER: Next chart, please.

ADM. GEHMAN: Let's go back one before we get off that chart. I haven't done any kind of a scientific analysis, but we've looked at about seven or eight of these charts now with those little red diamonds down across the bottom. By rough order of magnitude, it looks to me like the number of hits greater than an inch is a straight line, a straight horizontal line. It's not obviously diminishing.

MR. SPARKS: Correct. It looks like it's averaged about

16, 17, I believe, 20. I think I ran the numbers before I came in. For the CFC materials, it was 20 some odd; and for the NCFI materials since the full-up venting, it's been, I think, about 16, 17.

ADM. GEHMAN: The point is the trend is not going down, not by any order of magnitude, anyway.

MR. SPARKS: Correct.

MR. HUBBARD: Is that taking out or leaving in the large events?

MR. SPARKS: The CPR numbers are taking out that 27R event. I did take that one out. So it would run it up just a little bit.

MR. FOSTER: Next chart. This is a list, a not completely comprehensive list of everything we've done but a list of efforts to reduce debris. I apologize that the font is so small on this. You could probably do better reading it on your handouts.

STS-1. We had some instrumentation islands on the LOx tank. There was a concern that we were going to make ice on those. So we removed them until we could verify that instrumentation islands wouldn't form ice. So, you know, we've been concerned from Day 1 with debris formation.

When we got to the lightweight tank series, which started with ET 8, this was a block change to the lightweight tank series and it enabled us to go do a few things to help reduce debris. One of the things was redesign of the bipod ramp angle from 45 degrees to 30. Now, this was done on lightweight Tank 7. So these things I talk about as a block change are incrementally implemented; but that was to reduce the loads, the air loads on the bipod ramp.

Now, STS-7, which I do not have a -- well, I guess I do have a picture somewhere in here. STS-7, at any rate, had bipod foam come off, but there was a very large repair done to the bipod ramp and it was judged that that was the key driver for losing the bipod ramp on STS-7. So we did two things. One, we incorporated the maximum repairable defect limit on the bipod ramps, said if you have to repair more than this size -- and it's a very small size -- take it off and start over again. And also we changed the ramp angle, saying that's going to reduce the air load. So those two in concert should really help the bipod ramp.

Also on some of the STS-7, we saw that cable tray ice frost ramps had come off. The block change to lightweight enabled us to change to a two-step single-pour application process versus the old one-step multi-pour process, and what this did was gave better structural integrity to those ramps. We also reduced the super-light ablator areas on the tank. We had large areas of the super-light ablator running all the way down the pressurization lines, and we removed a lot of that and also deleted the anti-geyser line. So there were a lot of things done at the lightweight tank initiation, one of which was incorporating the two-tone foam configuration. That was an attempt to reduce the environments and help in foam loss prevention.

ADM. GEHMAN: What does two-tone have to do with it?

MR. FOSTER: Two-tone was the area that I showed around where we filled in the stringers. What that did is reduce the aerothermal environments in that region by having a smooth surface as opposed to localized stringer effects. It turns out that was probably not one of our best decisions; but, you know, we weren't planning on the vendor changing in the material.

On STS-27, we saw some large intertank divots that I showed you, the umbilical well camera for STS-26. And a corrective action was to drill holes in the two-tone areas to take care of the debris due to the isochem bond line issue.

STS-32R in 1990, we had the intertank and associated bipod part come off. The problem there was the vent holes that we were drilling did not go down far enough. So they pin-gauged them to make sure everything was going down the right amount, fully vent this area where we were getting de-bonds.

The STS-35 in 1990, also there were ten areas on the flange where divots were observed. This started a process to investigate why we were getting flange divots, and the result was that there was an improved process to spray the foam around the flange bolts. They were getting a void underneath the bolt because of the spray pattern. They changed the technique for spraying it so that you could ensure you weren't getting a void underneath there. That helped and we're still getting flange divots, but not as many as we were before that change. So it's gone in the right direction.

STS-50 in 1992. The jack-pad area, which is an area between the bipod where we have a tool helpful in holding the bipod during mating operations, when you remove that tool, you have to close out that area. The method that they were using led to void areas. They changed the process to keep from forming those void areas. Even though I don't have it on this chart, there were two or three other changes made specifically on the jack-pad to ensure we didn't get those coming out as debris, the foam in that area.

And, Scotty, do you know? Have we seen jack-pad area debris recently?

MR. SPARKS: It's performed very well since that configuration change.

MR. FOSTER: STS-46. Again, this was the result of the observation on STS-50 that there was an intertank/bipod divot. Added some more vent holes right in front of the bipod ramp in that two-tone area to try to decouple those things and see if we could keep the intertank two-tone region from ripping off the front of the bipod.

Finally in STS-54, ET 51, because of all these previous problems that we talked about on the two-tone foam on the intertank, we incorporated a two-gun spray foam application to replace the two-tone foam. So ET 51, STS-54, was where we got rid of the two-tone foam.

STS-56 in '93, we saw ten large divots on the -Z intertank acreage area and there was a study that looked at that and the process was changed in order to try and reduce the rollover and crevassing that Scotty talked about a little bit earlier.

MR. SPARKS: I think there were some processing changes that were made, and that process has also been approved, has improved the performance of that intertank area.

MR. FOSTER: Then STS-87 was the popcorning of foam off the intertank, and there was an increase in the number of tile hits. So there was the large IFA effort that the External Tank program went through, and basically we incorporated the vent holes to keep that from happening and that has worked well.

On 112 we saw the bipod foam loss. This was at 32 seconds, I think. It basically was the first bipod foam loss that we could say, you know, this was not associated with the two-tone; and it was the first thing that we had seen in quite a number of years. So there was a corrective action that was kicked off, and I won't go into what they were really going to change there. They were going to remove SLA from under the foam; and that is hypothesized as one of the factors that can lead to bipod foam loss, which I'll get into in a minute.

I'm going to switch over to cryopumping and cryoingesting before we have any other questions on this.

MR. HUBBARD: Before you leave that and go to cryopumping, this is a very impressive list of all the things that have been done over the last 22 years to address the shedding of External Tank debris. Nevertheless, if you go back to any of the charts that have the red triangles that indicate the divots greater than an inch, which is one of the characteristics that you look for, the line is pretty much a flat line there. I mean, whether it's 10 or 15 or 20 or whatever. So do you see any way to drive that line down to zero or near zero?

MR. FOSTER: I'll let Scotty go first on that one.

MR. SPARKS: Well, I think we're always trying to improve the product, but we don't want to change the product unless we're justifiably sure that that's going to improve the product. One of the things we did that's not captured on this chart is we changed from a nose cone that did contain insulation to a composite nose cone that has no insulation. That took us completely out of the realm of shedding debris, of course, in that area. So that's one of those things that you know you're going to remove a failure mode out of the way if you do that. So that's one of the things that has happened.

So there have been several improvements that I think the program or project has been proactive in pursuing. Indeed, there's still a level and, you know, generally they're coming from those closeouts in that intertank region that seem to be problematic. We try to improve our processing to the extent possible, but thus far it's staying in that 15 to 16 range.

MR. HUBBARD: To follow on that a little bit, I guess if I had a problem that, in over 20 years, the average stayed essentially constant, it seems to me that that might argue something about the basic chemistry or basic properties of the thing you're dealing with, the foam itself. I mean, do you see the foam as being difficult to control in a very precise manner?

MR. SPARKS: No, I don't Mr. Hubbard. Really what I'm seeing -- again, from my opinion and I think probably a generally held opinion -- is that it's an issue of trying to process that material the best you can. You know, if I had to take a guesstimate as far as the location where we're shedding the most debris, it would be in that hydrogen intertank flange area. That's just a hard area to close out. There's lot of bolts there and when you're spraying that material, a lot of potential for shadowing of that foam and possibly having some voids behind that. We've always attributed that to the reason why we're losing some of that material in that area. Of course, the other closeouts. Just a little more difficult. A little bit more random as far as being able to shed that debris. Even though, say, in the hydrogen tank where it seems like the environments, as far as cryogenically are more severe, it's robotically sprayed upon, a very smooth, flat surface. It's those closeouts on complex geometries, I think, that's tough.

MR. HUBBARD: So then just to follow this to the end of my question on it is that it's the system. You know, you've got a foam and it has to be applied over a certain type of underlying structure and making that so that it is free from shedding seems to be, over the last 20 years, a tough thing to do.

MR. SPARKS: Yes, sir. Generally, I mean, you've really got to go back to the beginning, as far as the design of the tank. I'm not so sure that the TPS processors were in the same room when they designed the tank, because it was designed structurally to be optimized. It's not designed for the TPS to be processed on there. If you were to redesign completely a tank, you would make the external a bit smoother, you would have those people in the same room, and you would do those trades. You know, if it's worth it, you would do it. So you've got to insulate what you've got, and I think they're doing a heck of a good job. They maintain a lot of skill in that area and, indeed, it's flat line about 15 thus far.

MR. WALLACE: I think you were probably sitting in the earlier session today.

MR. FOSTER: Yes, sir.

MR. WALLACE: There was a discussion about whether this 112 event wouldn't be an in-flight anomaly or not. Can you speak to what the ET project's position was on that?

MR. SPARKS: I think the position was that it was a random occurrence of faulty processing and that it was nothing had changed in the system to indicate that that was a systemic issue as far as processing of material. They had gone and done their homework, as far as that goes; but I think when 107 did occur, I think that would have kicked it into another issue. If I recollect right, I think there was an issue of an IFA pending photographic analysis upon return of 107.

MR. WALLACE: And with these two observations, Mr. Foster noted that this was the first time since you had changed, gotten away from the two-tone foam and it was not associated with two-tone foam and also the fact that it hadn't happened in ten years. Would that sort of argue more in favor of or make it an IFA or against that?

MR. FOSTER: I guess that would have to argue in favor of making it an IFA. I can say that after the 112 the project did say, okay, we do not want to release that big a piece of debris. It hit the SRB and did no damage there, but still it was a large piece and the project said let's go look at redesign options.

MR. WALLACE: When you say it did no damage there, do you mean it didn't threaten the flight? I sort of understood that it actually did some damage.

MR. FOSTER: The 112 particle that came off at 32 seconds, it came down and hit the IEA box on the SRB and I believe -- and this could be secondhand information -- but I believe that it didn't do much damage at all to the foam and the TPS on top of the IEA box. I'm sure there's better information available from other people, but I don't think it was a large impact.

DR. LOGSDON: Is there a program-level requirement for debris-shedding or lack of debris-shedding on the External Tank?

MR. FOSTER: The program-level requirement is that we shall release no debris that is harmful to the Orbiter. So it's a very subjective thing; and while we have been working hand in glove with the system over the years, you know, we've worked with them on debris teams and the debris panel and all that, again, everything was judged as a maintenance item and not a safety-of-flight issue. I'm not going to say that was right or wrong in the past, but that's the way it happened.

Next chart. We'll go on to cryopumping, and I'll go through these rather quickly. I'm sure you've all heard of cryopumping, but the mechanism of cryopumping is simply the transformation from a gas to a liquid at cryogenic temperatures. The little graphic shows barely a little crack from the ambient at room temperature. When you get down to low temperatures, the gases are condensed within a void or it can be a porous medium and when the air in the cavity or this porous material liquefies, which is what happens at structural temperatures below minus 297 degrees F for oxygen and minus 320 degrees F for nitrogen, it can liquefy inside the cavity and what that does, it locally reduces the pressure and basically sucks more air into the void. This is a process that continues until you can fill up the void.

Now, in and of itself, that really doesn't bother you. It's what happens when that liquid tries to gasify and come out. If you have a sufficiently large vent path for the gas to come out, you know, no issue. You might see a condensation cloud. If you have not a sufficiently large vent path but one where you crack the foam and get, that way, more of an escape path, you can relieve the pressure without causing debris. But if the vent path is not sufficient, as shown in the bottom sketch, you can physically pop debris off. And we think we've seen that on a few of the flights, like in the flange region where it looks like it's a dinner plate that came out. We can recreate that in the laboratory.

GEN. DEAL: Mr. Foster, say for the sake of the argument if you looked at that and you had a piece of tape or something that was blocking that from escaping, that would make it that much worse and cause a divot at that point?

MR. FOSTER: Yes, sir. It's a matter of whether it's got enough vent area to get out. And cryopumping is interesting because you can slowly, you know, suck in air in hours as you're out on the pad. But when it comes time to gasify that, it usually happens quickly and you build up large pressure and it doesn't have a vent path to get out. Now, that is cryopumping. Now, we have in the bipod region created a term just so we can communicate. We call it cryoingestion.

Next chart, please. This is with a postulated method for getting cryonitrogen ingested into the SLA. Let me orient you here. This is the bipod spindle. This is the super-light ablator that's over the spindle, and our heater element has a wire that comes down here and the wire runs up through this stringer into the intertank. What we're doing here, this view is a view in this direction. So you see the bipod spindle. Here's the wire that comes into the intertank, and the shaded areas are the SLA. Then you've got the foam over top of it.

Next chart, please. We have a nitrogen purge in the intertank. We have an area -- during fill, you will fill up liquid hydrogen in the tank and it will go all the way up into the dome. You will get the metal surfaces cold, below the liquefication temperature of nitrogen. So we have our nitrogen purge in the intertank and you're forming liquid nitrogen down in this Y joint region. You also can get the nitrogen purge in through this single stringer associated with this bipod there. We have two bipods, so there's two stringers that have this SLA over the wire, going up into a stringer. In this area you can also get liquid nitrogen temperatures.

What I've shown on the right side is that in this scenario that's postulated for cryoingestion, you get liquid nitrogen that is sitting right on top of the porous SLA material and it can absorb into the SLA. Now, this is a photograph of the flange between the hydrogen tank and the intertank. This is an area between shims so that you can have an area that goes all the way into the intertank here. I show that as also being postulated area where you can get some liquid nitrogen to come into the SLA. We don't know if that's a true hypothesis, but we're trying to look at everything to see if there's a mechanism for this thing called cryoingestion to knock off bipod foam.

Next slide, please. With time, you can absorb more of the liquid nitrogen into the SLA and at some point your temperatures are going to be above the liquid nitrogen temperature and you won't fill this whole area with liquid nitrogen. The --

ADM. GEHMAN: That's all assuming the heater is on and working, but the heater doesn't work back off the top of it.

MR. FOSTER: That's a true statement.

This postulation here shows that we form solid nitrogen; and the timing is real critical here, you know, whether you can ingest or absorb the liquid nitrogen and how much you get in here before you get solid nitrogen forming. The key to the solid nitrogen forming is that blocks the escape path back through the stringer. So you could have an area of nitrogen here that during flight could generate pressure to try and push off this bipod.

The next chart, though, shows you some temperatures. Here's the temperature on the outboard, on the top of the bipod. You see that it's basically room temperature when you launch and then it goes up with the aero heating, but what's happening outside here doesn't really transfer into this area. The blue line right here is the substrate, the aluminum substrate, and what happens is at this time the liquid level in the hydrogen tank has gone down and so your ullage temperature is warming this area up a little bit -- "warming" being a guarded term because we're still below minus 300 degrees F.

The other point here is that this area which is between the SLA and the BX -- or it is that interface -- it really doesn't respond to either of these temperature changes. So there's a real critical timing in both how you get liquid nitrogen in there and how you get it out for this scenario, but it's one that we are looking at very seriously and have a bunch of tests that we're going to run to say yea or nay on this hypothesis.

DR. RIDE: What's the temperature of the solid liquid transition?

MR. FOSTER: Minus 346 degrees F.

MR. SPARKS: Dr. Ride. Nitrogen? Minus 346 Fahrenheit.

DR. RIDE: That happens before a hundred seconds.

MR. FOSTER: Let me point out that this thermal analysis here did not take into account the effect of nitrogen in the SLA, which would change the thermal conductivity a little bit and would change these numbers. We have programs to try and put liquid nitrogen in SLA and measure the conductivity, but that's a tough thing to do. But we're going about trying to get that.

The next chart. Notice in big words this is preliminary graphics. We have gone through the dissection program on ET 120. What I wanted to show you was that we do have some defects, rollovers, voids inside the foam and the SLA. I wanted to show you a solid model and make it real pretty, but this is an early shot at it. We'll get better in the next couple of days, but this is showing you where during our -Y bipod dissection. The yellow are little foam items that we saw. Most of these are rollovers. So don't judge anything by the shape here too much. And we had green areas, some SLA items, which are very hard to see. We had a couple right in there. The clevis itself, while it's shown as green, is not a SLA item.

We'll be showing you these Thursday, I think, when you're coming down; and the graphics will be a little bit improved. Basically the intent of showing this chart was to say that we have gone through the dissections and we're proceeding on getting ready to go to dissect ET 94 to see what kind of foam we have and what kind of SLA underneath there so that we can take those into account in the testing we do to try and look at what happened on 107.

Next chart. I'll let Scotty finish up with this chart here. It's the progress, I guess, we've made.

MR. SPARKS: This is a chart showing -- the top picture being STS-7/ET 6. All materials were CFC-based materials. It kind of shows certainly the craftsmanship that has improved to STS-112, a separation photo. You certainly can see a lot of improvements as far as the workmanship of that material. So certainly there's been significant improvement and there's a lot to be corrected, but I think certainly the material and the processing has been improved over the years.

ADM. GEHMAN: All right. Thank you very much.

GEN. HESS: Y'all have a very rich history with this particular problem, and I can see visually by the chart that improvements have been made over time. My question really is: Did you ever think that it was possible to pop a big enough piece of foam off of this External Tank to severely damage the Shuttle itself?

MR. FOSTER: I'll take a shot at it first. The answer is yes, you know. We have large areas where we have closeout materials that we know are hard to spray. So, yeah, we are always worried that there's going to be a big piece that comes out that would throw us over that maintenance item line.

MR. SPARKS: Let me throw in my opinion there, too. I agree with Lee. We watch very closely ascent. That's because we know that that material could come off and cause some damage. So we understand that that's a potential and we understand that it does require a lot of focus on processing that material to make it not do that.

GEN. HESS: I get a little bit lost in this characterization that it was not a safety-of-flight issue, it becomes a maintenance issue, which is what we hear on most instances, frankly.

MR. SPARKS: You do hear that a lot, and maybe that was because, you know, maybe the predominance of those pieces of material coming off have been small in the recent past, but there is still a lot of concentration, a lot of focus upon not shedding debris.

ADM. GEHMAN: Let me ask a question. Have you discussed or ruled out or considered a pre-formed bipod ramp piece of insulation, a molded piece that would be physically attached and that would be in some way reinforced with some structure that would not come apart.

MR. FOSTER: Yeah. There's a separate group that's working the redesign options. Scotty and I have been working the investigation. I assume later on we'll transition over to looking at the redesigns. They're doing exactly what you're talking about, looking at ways to keep from having a complex geometry to have to spray or encapsulating. So all of that is working towards making sure that the spray is not too big a challenge to the techs that do it.

ADM. GEHMAN: As I understand it, there are other places on the ET where there are pre-formed pieces of insulation like along the lines, for example.

MR. SPARKS: Right. Right.

ADM. GEHMAN: Gentlemen, again, thank you very much for being so patient with us as we worked our way through molecular structure and polymer bonds here. This is obviously a very serious issue and an issue that's going to get a lot more attention before we're finished here, and I want to thank you for answering all our questions so completely and helping us do this.

I also want to wish you all the very best of luck in the two or three different hats you wear as you both do your day job and also work at finding out how we're going to fix this. So thank you very much.

All right. Board, we are finished for today. See you tomorrow morning.

(Hearing concluded at 5:01 p.m.)

April 8, 2003
Houston, Texas

Columbia Accident Investigation Board Public Hearing
Tuesday, April 8, 2003

9:00 a.m. - 12:00 noon
Hilton Houston Clear Lake
3000 NASA Road One
Houston, Texas

Board Members Present:
Admiral Hal Gehman
Major General John L. Barry
Dr. Sally Ride
Dr. Sheila Widnall
Dr. Douglas Osheroff
Dr. John Logsdon
Mr. Steven Wallace

Witnesses Testifying:
Mr. Richard Blomberg
Mr. Dan Bell
Mr. Gary Grant

ADM. GEHMAN: Good morning, ladies and gentlemen. This public hearing of the Columbia Accident Investigation Board is in session. We're going to continue learning about various parts of NASA's handling of safety items, safety issues. This morning we're privileged to have in our company Mr. Richard Blomberg. Mr. Richard Blomberg used to be the Chairman of the Aerospace Safety Advisory Panel and has looked at these issues for many years and probably is as knowledgeable as anybody. So we're delighted to have you with us and thank you very much for helping us.

Before we get started, I would like to ask you to affirm to this panel that the information you're giving us today is correct and accurate, to the best of your current belief and knowledge.

THE WITNESS: I affirm that.

ADM. GEHMAN: Thank you very much. If you would introduce yourself and give us a little bit of a biographical sketch, and then we'll ask you to make an opening comment.

RICHARD BLOMBERG testified as follows:

MR. BLOMBERG: Thank you, Mr. Chairman and members of the Board. I am currently the President of Dunlap Associates, Incorporated, which is one of the oldest human factors consulting firms in the world. I have been with Dunlap for 35 years. My work focuses on transportation safety and particularly on how humans, hardware, and software can work together to prevent accidents. I've also been extensively involved in accident analyses.

From August 1987 through March 2002, I was associated with NASA's Aerospace Safety Advisory Panel as a consultant member, Deputy Chair, and Chair. The ASAP, as it is sometimes called, was formed by an act of Congress after the Apollo fire in the late 1960s, to be an independent safety adviser to the NASA administrator and the Congress itself. Although the panel dealt with the full range of NASA's aeronautics and space activities, the Space Shuttle was obviously a main focal area.

For much of my 15-year tenure, I was the team leader of the panel's subgroup that examined activities at the Kennedy Space Center. As the panel's human factors expert and then its Deputy Chair and Chair, I participated on most of the other fact-finding teams and visited all of the NASA human space flight facilities and major contractors on a regular basis. Since leaving the Aerospace Safety Advisory Panel, I have continued my involvement with the Space Shuttle as an independent consultant to some of the contractors.

ADM. GEHMAN: Thank you very much.

MR. BLOMBERG: You're welcome.

ADM. GEHMAN: Very impressive. Let me ask the first question, and then we'll pass it around to the panel. I noticed that the ASAP has been concerned over the years about NASA's investment in basic infrastructure and test equipment and things like that, based on an assumption that there would be a system that followed the Shuttle; and then there were some announcements that the Shuttle is going to be extended much longer, to 2012 or maybe even 2020. So that takes care of that problem. I mean, now we've got enough time to amortize investments in infrastructure and test equipment and things like that, which is good. Now we've got a problem about ageing aircraft and whether that's a reasonable engineering goal so the Shuttle can operate safely until 2020 or 2012 or whatever the number is. Do you have views on that issue about how we would determine what is the proper life for a research-and-development vehicle like the Shuttle?

MR. BLOMBERG: Yes, I do. The panel looked at that very carefully, both from the top down, so to speak, and from the bottom up. In other words, we looked at the total system and tried to consider its ability to fly to 2020 or beyond, because we were firmly convinced that it had to. Even with the rhetoric concerning a new vehicle, we didn't see the capability to develop such a new vehicle on the time frame that people were talking about. So the notion of having a new human-rated space vehicle, for example, within eight years just was unrealistic, by the time you go through all the funding cycles and approvals; and, further, there were no new enabling technologies. We felt that there were two main areas where you would need some breakthroughs before you would have a better vehicle than the Space Shuttle, and those areas were propulsion and materials. We didn't see anything out there that was notably better than what was being used in the Shuttle.

So we really came to the conclusion that if you built a new vehicle, what you'd end up with is an upgraded Shuttle-type vehicle, so why not upgrade what you have and follow the models that commercial aircraft and military aircraft had used for years. So we felt very strongly that the vehicle was capable of flying as long as NASA needed it and was capable of doing the job safely. What concerned us was that there was no investment in the future and therefore there was no ability to take advantage of new safety improvements that could make the vehicle even safer. And it was an opportunity loss that really, really concerned us more than a degradation of safety. Because we were absolutely confident that the NASA folks and the contractors would never fly the vehicle if safety deteriorated. It's a requirements-driven system. They either met requirements or they didn't fly. And in my experience, I've never seen a program and a workforce as dedicated to safety as the Shuttle and its contractors. But they also were dedicated to achieving their goals and sometimes those two objectives can clash if you don't have sufficient budget.

So what was happening and what concerned me and what I reported to the Congress last year was that they were deferring a lot of safety upgrades and deferring investments that were needed for the future. That wasn't sacrificing safety immediately because all the requirements were being met, but they were pulling in the funding needed for long-term improvements in order to fly safely today and they would not be able to recover from that down the road.

ADM. GEHMAN: Could you comment, what are your views on how you get out of that loop? As the Shuttle gets older, it requires more maintenance and, as you mentioned, it's a requirements-driven system, but the requirements of today are not the same as the requirements in the early Seventies and so essentially every flight gets more expensive. You have to start making infrastructure upgrades and safety upgrades; and metal which was not designed to last 25 or 30 years; you have chronological problems. So I think it's not hard to imagine that while you could continue flying the Shuttle safely as long as you invested in the things that you mentioned, essentially it keeps getting more expensive every flight. So you're in a loop where you can't invest in the things that you need to to get out of this -- that is, the next program. I hate to use the word "gracefully degrade," but how do you break this loop?

MR. BLOMBERG: Well, I don't think the loop is quite as difficult to break as you're characterizing it, Admiral. I think, first of all, if expense is the issue -- expense and safety, first of all, are not necessarily tied. There can be things that are expensive to deal with that are not safety related; but if you have an obsolescence issue and you're dealing with expensive parts, that's when an upgrade is called for. And in most cases with the Space Shuttle, there were upgrades identified that would deal with the cost issues. Now, you were never going to deal with the basic problem that the vehicle is very difficult to maintain. It's very labor-intensive and it takes a lot of care and feeding, even when it was brand-new, but that's inherent in the design.

In terms of safety, I think the two things that were needed, as I mentioned, one was upgrades, where you've got new technology that's safer. An example: the general-purpose computers. The Space Shuttle's computers are back literally from the dark ages. They're performing very well, but there's additional capability -- for example, giving the crew predictor information -- that they don't have right now, that the new electronic displays are capable of doing if they had computer power behind them. I mean, that's an upgrade that would improve safety.

The other area is additional analyses. The analyses on which the design was based, as you point out, were quite old and they were based on flying a hundred missions but over a relatively short period of time. So it's time to go back and find out where those analyses break down when you extend the life. The hydrogen line on the pad, for example, that failed and delayed a launch was an example of something that, had one said this has to last for 40 years, there would have been weld inspections on that line; but since the requirements weren't stated for 40 years, nobody inspected the line. So I think you have to revisit those requirements and change them as necessary to fit the age of the vehicle; but if you do that, I think you could fly the Space Shuttle at a reasonable cost for the Space Shuttle and certainly at an increased level of safety from where it was being flown.

MR. WALLACE: I'm from the civil aviation sector. You mentioned the sort of civil aviation model. I'd like to pursue that a bit further, as to whether or not there are advances to be made that would be sort of in the nature of what we call a derivative aircraft. I mean, the Boeing 737 was designed over 40 years ago and it's still being produced at a great rate although what's produced today, in many respects, systems, aerodynamics, and engines, bears little resemblance to what was produced 40 years ago. Is there likely to be derivative or incremental improvements to the Shuttle, or is it time to start with a clean sheet of paper?

MR. BLOMBERG: Well, as I mentioned earlier, Mr. Wallace, I think that starting with a clean sheet of paper means going back to do some basic research in propulsion and materials that hasn't been done yet. So if we were to start a new vehicle today, I think a derivative vehicle would be the way to proceed because we have a lot of operational experience with the Space Shuttle and it's well characterized. I think the civil model that you're pointing out, I think there are two variants of that. One is the derivative aircraft like the new generation 737, which takes advantage of all the operational experience of the older generation. The other is retrofitting the actual old vehicles, which some of the airlines, for example, have done with the DC9 and gotten a very efficient and passenger-friendly and pilot-friendly vehicle. I think both could be done.

There was an example of that: *Endeavor* is a derivative of the Space Shuttle. It was not certainly the same as *Columbia* or *Challenger* or the earlier vehicles, but it was based on them and then putting the multifunctional electronic display system in the Space Shuttle has upgraded the flight deck quite a bit. There were other derivative kinds of proposals on the table, some of which may have been worth doing and others may not have been; but it would have taken some more R&D to determine whether they were valid or not. So I think both of those models would have worked; and from my opinion, I think the Space Shuttle could fly well into the 2020s without any problem if it were the subject of a program such as the airlines or the military do with their older aircraft.

MR. WALLACE: Would you point to any particular guiding principles for driving the derivative upgrade process? I'm thinking about the current ASAP report which just came out in the last couple of weeks which identifies the current human-rated requirement of a crew escape system which will function through the full range of powered flight and recommends that that be retroactively applied to the Shuttle. Could you speak to that?

MR. BLOMBERG: Well, that was something that we started working on; I guess it was three years ago now. This is the third year that that's been in the ASAP's report -- two years when I was Chair and now this year. I think that's tied to the themes that we had also of the reality of the service life of the Space Shuttle. The government -- and I won't say NASA because NASA is not master of its own destiny when it comes to budgets -- the government had made decisions at first that the Space Shuttle was only going to fly to 2006 and that the new vehicle was going to be on the drawing board. Then when that didn't happen, it kept creeping out in two-year-or-so increments; and so there was never a payback period that would warrant looking at an upgrade as significant as a crew escape system, which is clearly in the billions of dollars, not millions of dollars.

What the panel started saying three years ago was, look, this vehicle is going to be flying for 25 years more probably, that's the reality, and the lead time for anything -- and you've picked an extreme example -- the lead time to get a full crew escape system into the vehicle is maybe a decade under current engineering. Maybe you can move it down to eight years; but in reality the new brakes when they were put in, took eight years. The last upgrade to the general-purpose computer took eight years from authorization-to-proceed to first flight. So something as complex as a crew escape system, assuming a decade is not unreasonable. We were saying, "But you've got a decade. If you get it in there in a decade, you've still got probably 15 years to use it; and that's very beneficial."

That's what we were trying to get everyone -- the Congress, the Office of Management and Budget, and NASA -- to listen to, that you can't creep up on these things because it takes too long to respond. The latency, the response time in the Shuttle system, even for just procurement -- if you just decide to buy spare parts of the same vintage that you have now, many of the critical components can take three to five years to acquire. That's not counting the paperwork and the authorizations and the contract. And that's just from the time you sign the contract. Some of the turbine wheels, for example, take 13 or 15 months to machine. So you've got to stay ahead of this, and they were not, because they didn't have the budget.

So the budget shortfall was forcing them to take a very short-term view in order to maintain safety. They had to meet all the current requirements, and so every cent they had, just about, was going into meeting the short-term requirements with Band-Aid solutions.

DR. OSHEROFF: Well, we now know that there are only three Shuttles left; and I dare say that if we lost another one, I suspect that the entire manned Space Shuttle Program would be in jeopardy. I'm not wishing to predict something. Do you consider the design of the Shuttle to be an intrinsically safe design?

MR. BLOMBERG: Well, Doctor, as a safety professional, I never say anything is or isn't safe. I think you're dealing with a risk-management issue and what is safe under certain circumstances or acceptable under certain circumstances, may not be under others. As an example, this country is at war right now and the military will be flying aircraft in conditions that they'd never fly them on training missions, because of the risk trade-offs of not flying them. If we had a crew stranded in space and we needed to launch a Space Shuttle right now, my recommendation would be to go ahead and launch it because I think it is inherently as low-risk a vehicle as we have to carry humans into space and do the job.

Can it be less risky? Yes. Absolutely.

There are identified risk-reduction measures that can make it safer or less risky to fly the Space Shuttle, but we're still dealing with an inherently dangerous environment. We've got seven million pounds of thrust at liftoff. The analogy I like to use when I speak to people is that's on the order of 45 to 55 Boeing 747s stacked end-to-end, at full thrust. That's a lot of power. The re-entry conditions are extremely hostile. No atmospheric aircraft comes close to meeting those conditions.

So we're never going to have a perfectly safe vehicle. We're never going to have a vehicle, at least with the current technology, that's as safe as the airliners we all fly on; but I think for a human-rated vehicle, the Space Shuttle is a good design, a risk-manageable design. It's a design that is well understood, that the folks can manage well enough to keep the risk as low as is humanly possible for that environment. I think that's all you can ask for when you're dealing with a dangerous situation.

DR. OSHEROFF: Well, let me ask another question, then. That is, how would you characterize the safety record of the Shuttle, given that it is, in fact, an experimental craft?

MR. BLOMBERG: Well, I don't want to be flip about it, but I would use two terms: "magnificent" and "unacceptable," because any accident is unacceptable, but given what the Space Shuttle has had to do and has been asked to do and the environment in which it flies, I think its safety record has been actually very good. Again, I'm not saying that two accidents is an acceptable number by any means; but it is a very, very dangerous situation. If you look back at the history of military aircraft test flights in all of the services and you look at the loss rates -- in the Fifties, for example, a jet aircraft, which is about the same maturity level that we're talking about human space flight -- the loss level and the accident level was much, much higher.

DR. OSHEROFF: Then you would characterize this more as a vehicle under development rather than a ready-for-flight vehicle. Is that correct?

MR. BLOMBERG: Well, I think the Chairman described it as an experimental vehicle; and I think it is an experimental vehicle and will remain an experimental vehicle certainly for our lifetime. You cannot fly six times a year, let's say, on average -- it's actually less than that -- in any environment and call a vehicle operational. That's just not realistic. I don't know care if it's a submarine, an aircraft, a ship, an airplane, or a space plane. If you're only flying it a few times a year, it is an experimental vehicle.

DR. OSHEROFF: Thank you.

GEN. BARRY: Mr. Blomberg, good to see you again.

MR. BLOMBERG: Nice seeing you.

GEN. BARRY: Two questions, if I can.

ADM. GEHMAN: John, pull the microphone over to you. There you go.

GEN. BARRY: I'd like to afford you an opportunity to comment on your testimony last year. You were quoted -- and I'm paraphrasing -- in April that you were more worried than you've ever been before on the safety of the Shuttle Program. Not the exact words you used, but I'd like you to give the full context behind that comment. I know you've already commented on a few things; but if you could give us a full context, that would be helpful.

The second question that I'd like to just have you comment on is when you were in charge of the ASAP, under your purview you reviewed the movement from Palmdale to KSC and JSC and then also the movement from Huntington Beach to JSC and KSC -- Palmdale to KSC and Huntington Beach to Kennedy and Johnson. If you could give us a little background on your views on those moves and how significant they were.

MR. BLOMBERG: Okay. Well, as to your first question, General, my remarks to the Congress were, I think, almost verbatim what you said. I said in all the years I've been involved, I've never been as concerned as I am right now. I went on, though, to say I'm not concerned for this flight or the next flight or perhaps the one after that but I am concerned in the long term. You can light a fuse that is slow-burning and takes a long time; and my concern was, as I've stated earlier, that the failure to put some money into the long term and to plan for flying this vehicle in the years 2012, 2015, and beyond, was sowing the seeds for a decrease in safety or an increase in risk out in those years and doing it in a way from which you could not recover because there was no way to just go down to the spare parts supplier and buy new parts, that you had to take action and it had to be done quickly.

I was trying to get their attention, frankly, and say, look, you've got to act now. This is not something you can argue about for two or three years because if you argue about it for two or three years, you run the risk that the safety level of the Space Shuttle is going to decrease over time; and that's unacceptable to all of us. It's unacceptable, I know, to NASA, it's unacceptable to the contractors, and certainly it was unacceptable to the ASAP to see the safety level slide backwards when there, in fact, were identified ways to have it move forward.

So what I was saying was, please act now, because the really dedicated people who are maintaining this vehicle are getting to the limit of what they can do with ingenuity. Sooner or later they're going to need cash; and it's really sooner, not later. So that's what I was saying to the Congress.

If you take the quote out of context, as has been done, it sounds as if I was predicting this tragedy; and I certainly was not. I was as surprised as everyone else that there was an accident and I still do not see necessarily a connection that something that they failed to spend money on in the past caused this. When your board comes up with a probable cause, it may show that. It may show that there was something that could have been done if some research money had been spent that was identified early on, but we won't know that until you come to a conclusion.

As for your second question, I think it relates very strongly to what we on the panel identified as one of the three major components of safety for the Space Shuttle: and that is workforce. The Space Shuttle is a very labor-intensive vehicle, and it requires people who fully understand how it operates and its care and feeding and, also, the differences among what was then the four vehicles. While they are similar, they're by no means identical. The folks at Palmdale, to take your first example, were experienced initially in building the vehicles and then in doing the major overhauls, the Orbiter Maintenance and Down Periods and the upgrades, installing the electronic displays and so forth. That heavy maintenance experience was somewhat different from the line experience that the folks had at the Kennedy Space Center; and, in particular, the management of heavy maintenance in the aircraft industry and aerospace industry is somewhat different than line maintenance management.

On a line maintenance basis, you want to get your aircraft back into service as quickly as possible and as safely as possible for the next set of flights. You want to meet your passengers the next morning. When you deal with heavy maintenance, you're talking about rolling a vehicle out that's got another five years of service life and is as close to zero time as you can get it.

From a management standpoint, those philosophies are quite different; from the floor workforce, it's not so different. They get a job card to do a particular job, and they do it. We felt that Palmdale had unique experience in the heavy maintenance arena and therefore maintaining that experience was an asset to the Program, although an expensive asset. It was a luxury.

What ended up happening with the budget cutbacks was that the workforce at Palmdale kept getting cut back. Every time an Orbiter rolled out, a major proportion of the workforce was laid off; and each time they recalled them, they were getting about 75 percent and then 60 percent coming back. So you were dealing with new workforce anyway, and that was a difficulty.

The Program decided to move the heavy maintenance to KSC, or considered that. We looked at it very, very carefully on the ASAP; and we concluded that under the then-prevailing circumstances with this loss of workforce and capability in Palmdale that, as long as the requirements were maintained, as long as there was no cutting back on the requirements, that the work could be pursued as safely at KSC as it could at Palmdale. We did not delve into the cost issues because that was not within our purview. We took it at face value that it was going to produce a cost saving. With respect to the move from Huntington Beach to JSC, I think many of the same things applied. We were very concerned about the potential loss of engineering talent and experience that was in the Huntington Beach workforce, which had already moved once from Downey to Huntington Beach -- and that was a move that was more easily controlled because it was basically local, you just changed your commute. This was requiring people to uproot their families and move from the Los Angeles area to the Houston area.

We had numerous exchanges with the Boeing folks about this and got reassurance that the process they were dealing with was sensitive to this and that while there would definitely be a perturbation in the system that everybody acknowledged, they were aware of it and knew its dangers and would therefore track it. So we were comfortable that if it was the right thing to do economically and from a program standpoint, that the people were on top of it and it would settle down eventually and it would not compromise safety because nobody would allow it to. In other words, if they didn't have the engineering talent to make the decision, they just wouldn't fly.

DR. WIDNALL: I have a couple of questions. You mentioned earlier that you saw no new enabling technologies, say, in the area of propulsion and material that would really justify starting a new program. Do you see new technologies that are related to ease of maintenance, because you also mentioned how expensive the Shuttle Program was? And part of that is do you think the new technologies related to ease of maintenance would be viewed as exciting by the researchers and the engineers who would be pursuing such technologies?

MR. BLOMBERG: Well, Dr. Widnall, I think the answer's very clearly that there were lots of new technologies or new applications of technologies that would help both maintenance/obsolescence issues and safety, would improve incrementally safety, not a breakthrough, not a hundred times, but certainly meaningful breakthroughs in many areas. In terms of the romance of it and the excitement of it, I wish you could have been, for example, on our visit when we went out to meet with the people who were looking at new technology for an electric auxillary power unit, just as an example. Those people were so excited about what they were doing and so involved that it was really impressive.

I think the people involved in the Space Shuttle -- and I know in aerospace in general, because I work all sides of aerospace -- are very, very caught up in the field. I sometimes refer to it as an addiction. Those of us who are involved in aerospace don't do it for the money. Certainly it's not the most highly-paid industry around. It's because of the romance. It's because it's the only way to deal with your interest. If you're interested in human space flight, there's one program. That's it. You're on the Space Shuttle. If you're interested in building the next generation of commercial aircraft, really right now there are two or three manufacturers.

So I think there was more than adequate romance and more than adequate enthusiasm even for the smallest components down to literally 30 and 40 thousand-dollar changes in processes that the people really believed in, suggestion box items. I've been out to third-tier suppliers for which the Shuttle is a very, very small proportion of their income -- it's not a financial issue -- but where they really want to make an improvement and have been thwarted because there's just no budget for it.

DR. WIDNALL: I guess another part of my question is -- because we have talked about this strain on resource and balancing the future with the present. Do you think there's a minimum number of Shuttle flights per year that could be conducted safely? I'm talking about workforce issues and facility issues and, you know, dropping below a sort of certain critical number.

MR. BLOMBERG: Yes. I personally believe that, and I think most of the members of the ASAP believe that there was a floor. As I recall, the National Research Council Committee said the floor was four; and we resonated pretty well with that. Clearly, if you go below some level as yet to be specified, you lose capability. You also aren't really saving all that much money because if you keep your workforce around, your cost is there and they're just idle and that's not particularly beneficial.

So my own feeling personally, not speaking for the panel or anyone else, is I would certainly not want to see it go below four unless there was some compensatory development programs going on simultaneously. For example, if you were building a new Orbiter, you could then fly maybe three or two and still keep capability. But it's just absolutely essential to keep that experienced workforce involved, engaged, and working on the vehicle to keep their skills up.

DR. WIDNALL: Let me challenge you just a little bit on this issue of culture because, as you know, I'm a professor at MIT and so I'm dealing with our students. I can only imagine the discussion if I went into the class of these students and told them that they weren't going to go to Mars but they were going to develop a new pump. I think there is a discontinuity there that would affect many of the sort of what I would call aerospace advocates, and I believe very strongly that we have to kind of make that cultural change to emphasize the importance of doing the job right and doing it reliably. So I really resonate with what you say.

MR. BLOMBERG: Well, and I resonate with your comment. It's been quite a few years since I taught at the university level, but I do give guest lectures every once in a while and I've met with a lot of students. You're right, but part of that -- and I'm not saying this in a pejorative way -- is the naivety of youth.

DR. WIDNALL: Thank God for it.

MR. BLOMBERG: Thank God for it. Absolutely. But part of it is also the lack of a firm objective. When we had the Apollo Program, the nation was committed to putting humans on the moon; and everybody was caught up in that. Right now we have that spirit within the NASA programs because everybody is caught up in the Space Shuttle and the International Space Station; but when you back that up to the university level, it looks as if it's mundane. When those folks come out, however -- and I would recommend to you, if you haven't done it, that you track some of your five-year-ago graduates, even from an elite university such as MIT, that have gone into the Space Program and find out what they're doing. You'll find out they are working on what they would have considered minutia back in school and they're loving it because they can see their involvement in the total program and the criticality of it.

So I think we need both. We need to have a mandate for a national commitment to a space program with some reasonable short-, medium-, and long-term objectives; and we also need to support our current flight programs better than we're doing. They can't be done on the cheap, and they can't be done based on just the ingenuity of the workforce. It can't go on forever.

DR. WIDNALL: Thank you.

DR. RIDE: Just a little while ago, you mentioned that there are some numbers of identified risk-reduction measures that could be put into place. I wonder if you could discuss those.

MR. BLOMBERG: I could discuss a few of them. I didn't bring a list of them and, of course, not all of them will prove out by any means; but I think I mentioned one that's near and dear to my heart because it's a research area that I've done a lot of work in, which is adding predictor information to the display so the crew have a better situational awareness of what's going on. It's great to have all the ground support for the flights, but still it's the crew that are on the leading edge, the cutting edge of what's going on, and they have to know what the vehicle is doing. Right now they're not getting the best information that they could get. So that's a safety improvement I would like to see.

The general purpose computers was another area where the Program has been forced to work out ways to extend the current GPCs as long as the Program lasts, which is just not taking advantage of modern technology.

The auxiliary power units. Right now they're hydrazine powered, which causes significant explosive risk during flight and significant risk to the workforce on the ground. Electric APUs were looked at. They were very close to a reality. They were expensive. That was a fairly expensive retrofit. They were lacking a little bit of battery technology development which the industry said was, as I recall, something on a less-than-two-year time frame with a reasonable development program. They could have had the battery technology.

There's health monitoring of the Main Engines that I recall, better health monitoring systems which would get you out of a lot of first-stage difficulties, first- and second-stage difficulties in the launch. For example, you would not have premature shutdowns of a healthy engine which could get you into an abort profile situation when you could actually reach orbit. The panel was very concerned about aborts. They're not something that you want to fly. I'm just thinking through the vehicle.

There were TPS improvements that were probably more in the area of obsolescence and cost but also toughened the tiles a bit against impact damage. The foam that everybody has been speaking of. There were programs looking at different blowing agents that were on the drawing boards.

Then there were the larger-scale things that were longer-term, like adding a fifth segment to the Solid Rocket Motors so that you could reach orbit with a Main Engine failure right off the pad, and other things such as that that were on a larger scale. So there were things -- and I didn't dig out my list of all these things that were briefed to us -- but there were things literally from the $50,000 kind of level up to the $5 billion level, I guess probably the most expensive one being the full crew escape system, that were all at various stages of conceptualization and development. Some were actually developed and virtually ready to go in. GPS navigation is an example. We just kept after that on the panel because it just never got in. There were some antenna problems and some minor difficulties; but with a concerted effort with the smart engineers around, those could have been solved. But, again, they took money; and there just was no money available.

DR. RIDE: What about in the area of risk assessment?

MR. BLOMBERG: There were some advances in risk assessment. NASA had used risk assessment, we thought, pretty well. The risk assessment models that were developed at Headquarters were used appropriately. From a safety panel's viewpoint, one of the things that concerned us was that people have a tendency to use probabilistic risk assessment numbers as gospel, and they are really a relative design tool. You know, whatever numbers comes out of your model is not an absolute. It depends on all the assumptions that you put in. So we looked at that and we followed the development of the new risk model at Headquarters and we were rather satisfied it was being used at an appropriate level and used also appropriately to supplement the engineering judgment of the people who knew and understood the vehicle very well.

ADM. GEHMAN: I'd like to follow up on -- go ahead, Dr. Logsdon. I'm sorry. Go ahead.

DR. LOGSDON: Earlier you said, Mr. Blomberg -- and I think I've got the quote right -- that budget shortfalls forced meeting short-term requirements with Band-Aid solutions. Could you give a few examples of Band-Aid solutions?

MR. BLOMBERG: Well, one that comes to mind -- and this is certainly not, by any means, at the top of the list of most important or most significant from the safety standpoint -- is the data cables that run from the data center out to the pads at Kennedy Space Center. These are old paper-jacketed cables, metal cables, into which water has intruded; and they keep losing pairs over and over again. The solution is to put air pumps on at various places along the cable and blow air in to keep the water out, as opposed to spending the money -- and it was not an enormous amount of money in the scheme of things -- to put fiber optic cables in and replace them completely, which inevitably will be needed.

Now, the argument was -- the rationalization, I should say -- was that it's probably not safety related. If the cables fail, we just don't launch. But it doesn't take much imagination to say if the cables fail at just the wrong time, just the worst situation, that it could be a safety problem. So it all depends on how you look at it. If you look at worst case, then maybe it was. Was it priority one? Absolutely not. But is it an example? Sure.

The siding on the Vehicle Assembly Building, which blows off in the wind and is a problem, is another example of something that really needed attention that was just Band-Aided, just stick it back on for now. The roof of the VAB.

And then lots of things, mostly in the infrastructure. Test equipment. There's still cathode ray tube test equipment, even when the systems that they're testing have been upgraded once or even more than once; but the test equipment was never upgraded with it.

Dr. Widnall was talking about her engineers. I would venture that she doesn't have too many engineers who understand vacuum tube technology too well coming out of MIT right now or who can program in HAL. So those are the kinds of things that we're talking about.

DR. LOGSDON: Let me go to the other end of that quote: "In the days after the accident, there were a fair number of press reports that the Shuttle's safety budget had been cut by 40 percent." Does that comment make any sense to you? Is there an identifiable Shuttle safety budget, and where would that 40 percent number have come from?

MR. BLOMBERG: Well, my guess -- and I haven't analyzed it -- but my guess would be that it comes from the budget for the Safety and Mission Assurance Office and function within NASA and probably within the contractors. Again, that has to be placed in context because after *Challenger*, there was an enormous expenditure in that arena for things such as redundant inspections. And the aerospace industry has realized in recent years that redundant inspections not only don't improve safety but they can actually be detrimental to safety. So a lot of that reduction in budget, I would assume, having not looked at the press' numbers, came from what were rational and reasonable cutbacks in excessive expenditures for things like redundant inspections and for things that were passed over to the contractor to do and were still being done.

So we did not on the panel see that level of cut. We did comment several times and expressed concerns several times about the degree of workforce cutback across NASA, which included the Safety and Mission Assurance function but also included the engineering functions and the training functions and everything else. We felt very strongly that they were going down way too far and way too fast; and we spoke, I think, loud enough and long enough that we got heard and turned the curve around and got it to go back up. Because, again, of the experience level you need. This is not an industry where you can go out and just hire new people when you need them and have them be productive immediately.

DR. LOGSDON: Did you look at the mentoring relationship between the new folks coming into the Shuttle processing world and the people that had that experience?

MR. BLOMBERG: We sure did. Not only that, we looked at that very carefully in the context of giving more responsibility to the contractor, because we said that the new NASA folks coming in in a smaller workforce were not going to have the ability to learn on the job and get that hands-on experience. And we argued very strongly for a mentoring program across the two groups so that NASA folks could mentor with contractor folks and vice versa because unless you kick the tires, so to speak, you really can't understand this vehicle. There were programs such as that in the works. So we were pleased with the response to our recommendations in that area and the actions that were taken.

DR. LOGSDON: You say programs in the work. Did they happen?

MR. BLOMBERG: Yes. A lot of times the ASAP made recommendations to NASA and they were concurred with, but the following year we'd look at them and it was a concurrence in name only, there was no budget, nobody did anything. In that area, the area of mentoring and the area of training, there were some very, very positive steps taken to correct the issues that we raised.

DR. LOGSDON: Did ASAP have a view on the privatization effort and its impact on Shuttle safety?

MR. BLOMBERG: We probably had about 30 views on it, Dr. Logsdon.

DR. LOGSDON: Well, you're here today. Let's hear yours.

MR. BLOMBERG: Okay. Well, first of all, it depends on how privatization is defined. Privatization was initially defined as going to the Space Flight Operations Contract, the current contract; and we had some concerns about the form of the contract that, frankly, turned out to be unfounded. They were theoretical concerns, and they were very well handled by both sides in the transition.

In recent years there's been talk about a total privatization, essentially giving the vehicles and the infrastructure to a private contractor and just letting them operate; and, very frankly, I feel that that is very naive, very unrealistic, and will never happen. I mean, there is nobody out there, I think, who would want to take on that responsibility unless they're indemnified; and if they're fully indemnified, then the government is gaining nothing except the contractor's fee.

So the cost is going to go up. So if there's some political reason why you don't want government workforce working on it, then I think that can work; but you'd have to be very, very careful of the transition. It's not the steady state that you worry about in those things; it's the transition from one state to another. You've got a program that's over 20 years old, 25 years old really. It's been flying for over 20 years; and to try to change its culture overnight by saying it's totally privatized and removing the checks and balances that everybody has become accustomed to could entail some increased risk. It could be done. I would prefer to see it done in the next program and design it from the ground up.

If you want a privatized program, then design it from the ground up. But with one customer, the government, and a limited number of flights and an unknown liability for things like the infrastructure -- you know, what does it cost to change a roof on the VAB or the side panels or to meet environmental concerns if they should come up -- I just don't see it being realistic to transfer to a private contractor completely.

DR. LOGSDON: Under SFOC, there are a particular set of incentives. Was there any concern that those incentives diminished the emphasis on safety by USA or were you -- you, I guess, as an individual in this case -- confident that USA could operate the vehicle as safely as the civil servants had done in the first 20 years?

MR. BLOMBERG: Well, my answer on that has to be time-dependent. When I read the Statement of Work for the contract to USA, I had great concerns. I was concerned, for example, about the incentive fee for meeting launch on time. I thought that was ill-advised because the last thing you want to do is tie some money to a launch decision. That has to be made purely on risk grounds. I was also concerned that the safety measures against which the contractor was going to be evaluated were defined by the contractor, and so you could end up with a situation where you managed to the metrics rather than managed to the safety of the vehicle. That was in theory.

In practice, we looked at USA's performance very closely. I know the folks there very well and have followed their performance, and I think it's been exemplary. They have called launch halts whenever necessary -- in fact, at points where I probably would not have called them personally because I thought it was ultraconservative, but it's better to be ultraconservative than the other way around. So I think the performance has been right on what you would want. They have the safety culture that is necessary. That doesn't mean it's 100 percent effective. That doesn't mean it can't be improved, but my concerns at the outset really did go away.

DR. LOGSDON: One final question. This is, I think, a giant extrapolation from what you have said this morning; but let me ask you about it. You've said you see no progress in materials or propulsion that would justify investment in a new vehicle, that the Shuttle has to fly past 2020, and that there are lots of improvements that could be put into the Shuttle. Would you recommend building an updated version of the Shuttle design, one or two?

MR. BLOMBERG: Again, without knowing the full budget picture, just from an operational safety standpoint for the Space Shuttle Program, I would absolutely recommend that. I think the finest thing that could be done right now would be to take all of the knowledge that the people have of the Space Shuttle System and all the additional knowledge that your board is going to produce, which is scrutinizing the System more than it's ever been scrutinized in recent years, and put that into one or two additional Orbiters and when those come online, maybe retire the oldest of the current ones. I think that would be the best thing that we could possibly do both for the safety of flight and for expanding our knowledge of human space vehicles.

Absent that, I would certainly like to see the existing vehicles upgraded with as many of those things as is reasonable to put in. We were talking about escape, for example. It might be a lot easier and more cost-effective to put an escape system into a new Shuttle vehicle than to try to retrofit the existing vehicles and cut through the existing mold lines.

So I would certainly love to see that and I think it's a way to go while simultaneously commencing the basic research-and-development programs that you need to have a radically new vehicle. Because it's not just going to happen. There's no market out there for building efficient reusable rocket engines unless it's for a human space vehicle. So NASA and the country are going to have to do that and work on the materials side, but it's unclear how long it will take to get the breakthrough you need to have a significantly better vehicle than the upgraded Shuttle that you're talking about, the Shuttle derivative, would be.

DR. LOGSDON: Thank you.

ADM. GEHMAN: Mr. Blomberg, among the other tasks that the Board has, including finding the direct cause of this accident and making recommendations to prevent it, we also have to place our report, in terms that I've used, "in context." As the Chairman, that's one of my specific problems is to place our recommendations in context.

One of the contexts is the life of the Shuttle Program, which is something that we've talked about before. If we're near the end of the Shuttle Program, our recommendations would have a certain flavor to them. If we are only 50 percent of the way through the Shuttle Program, as has been suggested that we're going to be flying Shuttles until 2020, we're at the halfway point. We've lost 40 percent of our vehicles at the halfway point.

This problem of putting it in context is weighing on my shoulders, and I was struck by some words in the last ASAP report that you signed, which was last year's, technically, 2001's. I would like to read something here. I'm not going to throw these things back in your face, but I want to allow you to talk to us about it.

This was finding and recommendation number one: "Last year, concern arose that the planning horizon for the Space Shuttle and the International Space Program was too short, imperiling the development, advancement, and adaptation of safety improvements" because you couldn't amortize them or you couldn't justify them (my words). "It is now recognized that the Space Shuttle will be used well beyond 2012, a longer life span than was originally anticipated. Now serious safety concerns are currently ranged around the potential for lost opportunities in safety improvements which can lead to safety problems as ageing systems deteriorate." In other words, now we've got a new set of problems. "The panel believes that the Space Shuttle is fully capable of supporting the ISS for its entire life."

So my understanding of what I just read is that by extending the Program life, we now have eliminated the excuses for not making infrastructure upgrades and all the safety things that you have mentioned, which I value that as a good thing -- that is, if there's money there -- but now we have a new problem and the problem is, of course, ageing and deteriorating systems. My first question is: Have I characterized that dilemma approximately correctly?

MR. BLOMBERG: Yes, I think you have, although I think it's a matter of emphasis. I don't think the ageing issue per se is anywhere near as great as the other issues, the issues of not upgrading the vehicle. I think the ageing issues could very likely give you some graceful deterioration, whereas the upgrades could give you some quantum jumps in safety or reductions in risk.

ADM. GEHMAN: The reason why the ageing problem is stuck on my forehead so well is because of the theory of the unknown unknowns, that it's turned out that the parts of the Shuttle Program, the parts of the STS which were viewed to be the most dangerous have not failed -- it's always something else which has gotten us, it seems -- and we feel that if you're going to continue to fly this thing for twice as long as it's already flown, there has to be an aggressive program out there looking for what we call the unknown unknowns. In other words, you've got to start looking for trouble. I believe that can be done, that we have other examples of aircraft that are working kind of at the edges of their margins, that are old and things like that -- military aircraft and civilian aircraft. The second part of my question, though, gets to the comment about the relationship between the Shuttle and the ISS. Do you believe that they are linked?

MR. BLOMBERG: Absolutely. I mean, the ISS was designed to the Shuttle's capabilities, with some help from the Russian vehicles and a little bit from the European vehicles, but basically to the Shuttle's capabilities. Frankly, from my own perspective, it would probably be a poor economic decision for the country to build another vehicle to service the ISS because the next-generation vehicle might have a totally different mission. So why not, as long as the Space Shuttle is capable of servicing the ISS throughout its entire life, keep that symbiotic relationship going. I mean, it was designed to re-boost the Space Station. They were designed to exchange consumables in both directions, if necessary.

So I think just a very simple answer is to keep the Space Shuttle flying as safely as possible as long as you are doing the Space Station and then think about what the mission is for the next vehicle, whether it's the support journey to Mars or some other purpose.

Going back to your first remarks also, I would like to point out that the kinds of safety improvements that we're talking about are not only hardware, software, and even ground infrastructure. We're talking about training. We're talking about re-analyses to understand and characterize the vehicle better for its now realistic lifetime. So that while there were life limits placed on every component -- you could only keep an External Tank in storage for so many years and you could only keep a Solid Rocket Motor segment in storage -- those limits are no longer realistic, and it's time to redo those analyses.

Well, as Dr. Widnall was saying, that's not romantic -- romantic from the Congressional standpoint. "Why do I have to redo an analysis? Did you get it wrong the first time?" It's millions and millions of dollars, but really that's what's necessary. It was done after *Challenger*. The failure modes and effects analyses were all redone. The critical items list were all redone, based on experience.

Well, now we have a lot of additional experience in both directions. We know that there are things that were originally characterized as Critical 1 items that aren't Critical 1. They're not Criticality 1. And there are other things maybe that were not categorized as Crit 1 that are now, because of ageing conditions, and either should be changed out or made redundant or some other changes. We need to recharacterize that.

All of the computer models that were used to develop the Space Shuttle in the late 1970s have been upgraded multiple times, the materials models, the flow models and so forth. What are the implications of those on the vehicle in both directions? Were we too conservative with those things, or were we too liberal? Did we misunderstand?

I believe that the requirements, down to the most minute requirements, need to be revisited by the people who understand the system, to determine whether they need to be upgraded. The simple example that the Program went through, I don't know, about five or six years ago with a new pressure-sensitive adhesive in the Solid Rocket Motors -- they couldn't use the one that was spec'ed, because of environmental concerns, and they had a requirement of a certain peel strength.

They went out and found another adhesive that met the requirement. It was right in the middle of the range of the requirement, and it didn't work. When they went and re-analyzed it, now scrutinizing it, they found out that they had been flying at two or three times the requirement and they really needed that. They bought the best off-the-shelf stuff and it was much higher than the requirement, and that was absolutely necessary.

So falling back on a spec that was written before you flew the vehicle doesn't have a lot of meaning. You now have over a hundred flights. It's time to re-do that. It's a costly process, it's not a romantic process, it doesn't produce things that are impressive to the public, but it is absolutely what goes on with commercial aircraft, with military aircraft, and it should be going on with the Space Shuttle.

ADM. GEHMAN: You are aware, of course, of NASA's budget and the kind of limitations on their budget. As I understand it, you are recommending that we consider upgrades to the Shuttle to keep it fully capable of flying for another 20 years, given certain conditions that you've outlined here; but we also have to get to work on the next manned spacecraft. This is going to be a tremendous pressure on a budget.

MR. BLOMBERG: Well, maybe. You know, there was a lot of money spent in the NASA budget, during the 15 years I was on the ASAP, that was not productive. Billions were spent on the advanced Solid Rocket Motor. It never flew. Millions or billions -- I'm not a budget expert -- were spent on the X-33 and the X-34. They never flew. I think that even within the present budget confines, it's possible to support the International Space Station and the Space Shuttle to the fullest extent that they need and have a technology development program that will support a next generation. But if you try to initiate a new vehicle program, to develop a vehicle from scratch when you don't have the technology -- so you're doing the technology development and the vehicle development at the same time -- then you're not going to have enough budget. That's what happened, I think, with X-33.

Instead of going and working on the technology areas that were clearly needed to make X-33 work, they embarked on building a test vehicle. I just am a believer in finishing what's on your plate before you take more, and I think supporting the ISS and Shuttle adequately is the first priority for the country.

MR. WALLACE: Let me switch to sort of a pure human factors type of question. We're a little over two months in this effort, and I have to say there are no lack of processes at NASA. I mean Flight Readiness Reviews and COFRs and Launch Readiness Reviews and all the processes leading up to that; and every time we ask a question, we get lots of paper.

Really, I mean it's a tremendously methodical, thorough set of processes; yet the investigation has raised some troubling questions about sort of communications and decision-making and flow of information up and down. My question is sort of human factors. Is there a point at which people find too much comfort in processes, where processes might actually stop thinking? Admiral Gehman talked about the unknown unknowns.

MR. BLOMBERG: You certainly can be over-proceduralized and can be process-bound. That is one of the things that can happen to an organization. I don't think it has happened to NASA. However, any big organization, any organization as large as NASA will have some communications issues and it is always difficult to determine how much should bubble all the way up to the top, to the Administrator's level, for example. Frankly, there is a real question of whether you want the Administrator making ultimate technical decisions because the Administrator is just that, an administrator.

I think in the 15 years I observed NASA, I think the processes were not perfect but certainly as good as you could expect from a large organization, and improving. It's an overused phrase, but continuous improvement was there. Now, not everything that was done was an improvement; but people were watching it. I think the processes are sincere. I think everyone within the system is truly dedicated to safety; and the big change that I saw over the 15 years on both sides, contractor and NASA side, was when I first joined the Panel, I would say that the likelihood of a randomly picked person in the system standing up and saying, "Time out. We're not going to fly. I'm stopping the flight," was very low. Today I would say it's virtually 100 percent, that anyone out there, from somebody turning a wrench to a middle manager to a senior manager, would feel absolutely empowered, if they were uncertain, to say, "Stop," and they would be listened to, that it would not be something that they would say, "No, you don't know what you're talking about." It would be at least run to ground very professionally before a decision was made; and certainly if time was of the essence, they would not fly. That, to me, is the essence of a good safety system.

MR. WALLACE: Well, I didn't mean to suggest that more decisions needed to go to the Administrator's level at all. But just to follow up on your answer where you say anybody can stop the process, in your experience, is there any change, post-launch, in terms of that sort of thing?

MR. BLOMBERG: Well, of course, the options available to you post launch are fairly limited. The post-launch environment and the launch countdown environment, I think once you start into a launch countdown and then you go on from there to the post-launch, you really do want to be procedurally bound. You want to be requirements-driven. You do not want to be defining waivers on the fly.

A waiver sounds like a terrible thing. I know when I first got into the aerospace business, I said, "You mean you're waiving a requirement? You're agreeing to fly in an unsafe condition?" Well, that's not the case, in virtually every situation. A waiver is a carefully thought-out process by which you decide that something is an acceptable risk. You don't do that under time pressure while you're in the middle of a launch count. You don't do that while you have a crew up in orbit and make decisions on the fly.

So, you know, if the flight rules say, "If such and such happens, you come home," you come home. Then you work it out. You know, if it turns out that you were wrong, that it was a sensor failure rather than a true failure of the system, you've taken the conservative approach. So I think that that's where you have to draw the line in this is when do you have to be procedure-bound and when can people have some leeway in the system and call it.

Even though it might sound conservative, I would not want somebody, while a flight was in process to say, "Time out. Bring it back." That's not the way to go. But, "Time out, we ought to study this and see whether we ought to bring it back tomorrow," that's what the Mission Management Team is for and things will get elevated to that team very rapidly. It depends on the context of what you're dealing with.

GEN. BARRY: One of the things we're trying to understand is a little bit about the management structure, and I'd like to see if this resonates with you. We're going to talk pre-launch and post launch. Pre-launch, obviously *Challenger*, a lot of focus has been spent on improving the process, particularly in a Certification of Flight Readiness.

If we characterize that and we said, okay, pre-launch is centralized, it is focused on competition between Centers a little bit, where all the Centers are involved in Certification of Flight Readiness, and there is, some would some argue, an attitude that you've got to prove there is no problem. Post-launch is more de-centralized. It is only really one Center primarily involved and that's the Johnson Space Center and, as some would argue -- and we're trying to figure this out -- that you have to prove there is a problem. Does any of that resonate with you insofar as pre- and post-launch considerations are?

MR. BLOMBERG: Well, it does resonate; but I think, General, that it may be a bit of a simplification. Pre-launch, I think you have a whole series of what I would call challenge-and-response meetings that culminate in the combined Flight Readiness Review. But really, every element and every subsystem has its own Flight Readiness Review that start way before that and it's a series of challenges based on what you know about the system and its recent performance. So if there was a hiccup on a previous flight or during processing or the previous flight of that vehicle, then you've got to clear that; and that starts way down with the sub-tier people, each of whom goes through a bunch of challenges. I would agree with your characterization that it's "Prove to me that it's safe to fly," but it's an incremental process.

Once the flight is up, the focus shifts to JSC for sure, but, remember, there's a Mission Evaluation Room operating not only at every Human Space Flight Center, but at all the major contractors and those rooms are there specifically to support their elements and the issues. So I guess my short answer is I agree with you except with the caveat that it has to be clear that the JSC folks are not trying to make technical decisions that are outside of their technical areas. They rely completely on the technical specialists. If it's a propulsion issue on the Thrusters, for example, they would go to the Thruster specialists. What they are specialists in is mission operations and once you're operational and once you're flying, they know all of these requirements and the rules and so they know to really turn to you and say, "You told me from your analysis that if this happens, if so many of these fail, we have to come back. We're coming back because you told me that." And if the specialists were to say, "Well, we really didn't mean that. It's okay to go on," then -- I can't recall a situation where that's happened and they've won; but if it were to happen, they would certainly have to produce some very, very compelling analyses and produce them virtually instantaneously. For example, they would probably have had to have a change request in the system already for that to happen. So my take on it is that your characterization is a good one and the system is a good one. That's about the way it should go.

GEN. BARRY: Let me follow up on that, if I may. If it is a rather structured process going up prior to launch with the Certification of Flight Readiness -- and I think the next hundred flights for the Shuttle are programmed to go to the Space Station, a couple are going to Hubble, so other than just the Space Station -- some have proposed an idea of having a Certification of Re-entry Readiness. In other words, you have an associate administrator who signs off on the Certification of Flight Readiness and we have a decentralized focus with the Mission Management Team, the MER, and you have also, of course, the Flight Director involvement. If we are on the Space Station, should there be a more centralized focus on re-entry?

MR. BLOMBERG: That's actually a very complex question because the first thought I have is it depends on what countermeasures you have available that would make that certification a valid certification. If you have no ability to fix the vehicle or to bring the crew back any other way, then it's kind of a moot point. If there are things you can do, if there are alternatives, then that has a lot of appeal as long as it doesn't get in the way of all of the other things that are necessary for safe mission operations. Re-entry is not just getting in and pushing a button and saying, "Let's go down." There's a lot of crew preparation. There's a lot of support needed from the ground; and as long as that review doesn't get in the way of those things or supplant any of them, I don't see where it would hurt. It might help.

ADM. GEHMAN: Mr. Blomberg, I was thinking here to myself that in support of one of your comments here when we were talking about re-entry -- having looked at re-entry things, checklists and things like that -- I was reminded that one of the things on the re-entry checklist is to put all the laptops away, which supports your argument that we've got to upgrade the computer systems because what we're doing is we're carrying a bunch of laptops up there because the computer systems won't handle it. Earlier we had this discussion about whether or not the Shuttle is a research-and-development or an operational vehicle and I think I heard you say -- and I'll give you a chance to comment -- it's closer to being an R&D vehicle than a transportation system.

MR. BLOMBERG: Well, I don't even think it's close. I mean, it is an experimental vehicle. Just the fact that it's flown over a hundred times doesn't change its nature. Every flight is an experiment. Every flight is gaining knowledge. It's not an airline, by any means.

ADM. GEHMAN: We may be using the terms loosely here as to whether it's an experimental vehicle or an engineering development model vehicle or something like that; but in any case, we are in agreement that this is an experimental vehicle. But it is being used in an operational sense.

MR. BLOMBERG: Well, that's true and I don't think those things -- I think that's a semantic issue more than a technical issue. It's being used for the repetitive support of the International Space Station and for flying humans into space on a regular basis, but that doesn't change the nature of the vehicle. That nature arises, for example, from things such as you've got multiple copies and they're not all identical by any means, that the technology that's being used in the vehicle is not widely-used technology, or much of that technology. It doesn't come from the nature of the mission.

ADM. GEHMAN: Of course, there's no law against using an engineering development model or an experimental vehicle in operational use. In the first Gulf War, the military's JSTARS was still an engineering development and was used. The Predator unmanned aerial vehicle was used in Bosnia that was still technically under engineering development. So there's no law that says you can't do that. But I'm still working on this context thing, and I want to get your views. I want to get this thing clarified. So it's an experimental vehicle and we're still learning about the environment in which it operates and particularly this Mach 24, 300,000 foot altitude environment which we know precious little about for a winged manned vehicle, but it is being used for operational purposes also. Now, the question I have relates to your comment about building another one. If we're in agreement on those two points, do you think it's reasonable for an experimental vehicle to have a 40-year life?

MR. BLOMBERG: I don't see anything that precludes it. I mean, I don't think we have any models to follow for that. This is a unique situation, probably one that we've never been in before; but given the care that went into the design of the vehicle and that has gone into its operation, I don't see anything that precludes that.

ADM. GEHMAN: Let me rephrase the question, then. Let's forget about NASA and forget about the Shuttle Program. Do you think the United States should evolve into manned flight into space by not evolving itself for 40 years?

MR. BLOMBERG: Well, Admiral, you know, if you ask me do I think that the United States made a poor decision perhaps 20 years ago in not spending the money to have a Shuttle replacement ongoing, I would say yes. But if you also ask me would the country be better served by not having human space flight until a Shuttle replacement is produced, I would vehemently say no. I mean, that human space flight is important, we are learning a great deal from it, we are accomplishing things in space, and the Shuttle is fully capable of supporting that at an acceptable, albeit not perfect, level of risk.

Now, would we have been better if we had Shuttle 2 now or some other vehicle? Probably. But we didn't make that decision. So right now we have to play the cards that we're dealt. The cards that we're dealt is the only human-rated vehicles that we know of on this planet are Soyuz and Shuttle, and Soyuz can't do the job. So it's gotta be Shuttle.

DR. OSHEROFF: Well, first let me say that your team won last night. I'm sure you're happy about that. I noticed that. I have no stake in that. Stanford did not make it that part.

I wanted to bring up a question. When my graduate students do something with a cryostat, which is actually a kind of extreme environment and things go wrong and they end up having to warm up and fix things, I always tell them that they learn far more from their failures than they do from their successes. I think that goes well beyond graduate students doing research projects, as well.

I think it is fair to say that we have some good ideas as to what led to the loss of the *Columbia* and her crew. We certainly don't know for sure and we're not willing to identify anything at this point; but assuming that we've done that, can you give me some ideas as to what the lessons are that we need to learn? I guess I'm particularly interested in the issues of risk management and risk abatement.

MR. BLOMBERG: Well, this is an area which I've examined quite thoroughly, not only for the Shuttle but particularly for various aircraft accidents that I've been involved in. The reality is that the sequence of events is that whenever you have a human vehicle, a vehicle that's going to transport humans, you do as much analysis as you can possibly do -- and I'm including testing in that -- to make it as safe as possible before you operate it. But as the vehicle gets more and more complex, it is absolutely impossible to check out every interaction and every type of failure and every situation that the vehicle will encounter. Therefore, in those places that you consider to be most risky, you build in redundancy, you do whatever you can, and you hope that your operational experience, the closed-loop feedback, will give you that additional experience, as you operate the vehicle, to upgrade it.

Mr. Wallace was talking about the airline industry. This goes on all the time, whether it's brakes or various components of aircraft that reports come back from operators saying, "We're having trouble with this." The manufacturer looks at it and says, "Oops, we missed that." We didn't miss it because of dereliction of duty. We missed it because it's a maybe a second or third order interaction, but now we can fix it. We've got this operational experience. Unfortunately, part of our operational experience in any vehicle is accidents. We hope it never gets to that, but it is part of the reality of operating, particularly in a high-risk environment. When there is an accident, we get a spin-off benefit; and the benefit is that we get the resources to focus in on the area that was involved in the accident and then a wider part of the vehicle. *Challenger* is a perfect example. There was a focus in on virtually every high-risk component of the vehicle, and a lot of improvements were put in.

I think that is the natural progression of things; and your students, when they destroy an experiment or have a problem with the laboratory, learn from that. You'd hope that they don't learn by someone getting injured or a high-cost destruction of property; but regardless, as long as we close the loop and as long as we didn't do anything intentional, deliberate, or uncaring -- we are fallible. I mean, I'm a human factors person, and I'm the first one to tell you that humans are perhaps the most fallible part of any system. We design the systems, we operate the system, we make the decisions to go, and so somewhere in whatever you're going to find for *Columbia*, humans failed.

The question that I would want to ask is: Did we fail through malice, did we fail through neglect, or did we fail through ignorance? If we failed through ignorance, let's learn from it, let's increase our vigilance, and make the system better, and keep that closed loop going. That's all we can do in any vehicle.

DR. OSHEROFF: I would suggest that there's another possibility and that is that the failure was through a faulty process which did not properly identify some of the risks and which would then have allowed NASA to take steps to minimize those risks.

MR. BLOMBERG: Absolutely. That is certainly a possibility. But if that's the possibility, I would speculate that that process failed because we didn't understand it, not because we short-circuited it or because anybody deliberately said, "Oh, it's okay. Let's go full speed ahead." That's part of the understanding. It's not only characterization of the materials and the software and so forth, it's characterization of how people and processes work. That's an integral part of it, and the whole Shuttle Program has been struggling with that now for years and doing a pretty good job of process control and understanding that processes are, in many cases, as important as products, as the hardware and software that result from them. So they've developed a process failure modes and effects analysis technique and some other things.

It's very likely that -- it's assured -- I mean, I am sure that whatever caused the accident escaped a process at some point. It had to have, because it flew. So at some point in the process, somebody missed it; and it may have been my panel. We may have been staring it in the face and missed it, but it wasn't for lack of trying, I'm convinced, on the part of all concerned, because, as I said in my opening remarks, I just have never seen a system more safety-conscious and people more dedicated to safety. That's not 100 percent assurance; it just says that their hearts are in the right place.

DR. OSHEROFF: Well, I fully agree with you, but I think that we really have to look at what processes may need improvement and I'm sure you agree with me on that.

MR. BLOMBERG: I do, Doctor, but with one variation. I think that the time to do that is after you've decided what the proximate cause was. The processes are in the root cause domain, and right now my understanding from your statement is you're still struggling with understanding the proximate cause. Once you understand that, then I think that's the time to step back and say how did that slip through all of the defenses.

DR. OSHEROFF: Well, let me suggest -- and I think that NASA's already suggested this -- inspection of the Shuttles in orbit, with the ability to repair at least the tiles, if not the RCC panels.

MR. BLOMBERG: Well, even if that doesn't turn out to be the cause of the accident, that may be a positive outcome of the investigation, saying here is a technique, is an ability that we had that we weren't making maximum use of. That's the kind of improvement that I was talking about that comes out of this intense scrutiny. But, again, I don't think that we're dealing with an escape here that anybody can be faulted for not having realized, because the operational experience just didn't point to it.

DR. OSHEROFF: I'm sorry, I have absolutely no intention of assigning fault to anyone. This is an extremely complicated vehicle and the process of certifying it for flight readiness is extremely complicated, but I think we have to set aside the issue of fault and, in fact, not identify that but recognize the processes that must be changed.

MR. BLOMBERG: I fully agree with you. I'm just saying I think it's a matter of timing, and I think that is done most effectively after you understand the causes and, you know, you have to work backwards from the effects and then say what processes were there that could have caught this and are reasonable to perform. I venture that you will find in some of your blind alleys, some of the theories that you've checked out that don't turn out to be the cause of this accident, you will still be able to back those into improved process because you've scrutinized those so much. That's a terrific benefit of the kind of investigation that you're doing. It's just a question of when to do it.

DR. OSHEROFF: So the idea of minimizing risk is certainly one that's very valuable.

MR. BLOMBERG: That has to be the overriding principle of the entire operation is risk management and minimizing risk and understanding the risk you're accepting. It's not only minimizing the risks but it's understanding the risk that you've accepted.

DR. OSHEROFF: Thank you.

DR. WIDNALL: I'd like to follow up a little bit on some of the words you've used. I didn't write them all down, but you said, you know, we know it wasn't due to malice. But then you had this rather large catch-all category called ignorance, and I guess I'm just not willing to allow so much to be in this category of ignorance. Being a poor engineer, I don't have a rich vocabulary in organizational theory; but it seems to me words like denial, organizational structure in the way the various levels work together, issues of unconscious trade-offs that various parts of an organization make, I think somehow that vocabulary has to get into any kind of framework which otherwise might be called ignorance. I mean, I think we really need to think deeply about how one organizes an effective, you know, as you mentioned earlier, large organization for the whole question of making good decisions in the safety area.

MR. BLOMBERG: Well, I agree with you completely; and probably the word "ignorance" was unfortunate. Being a poor engineer myself, I couldn't think of a better term. But I wrap in that the clear issues of things like we don't have the technical knowledge to understand how a material performs under certain circumstances because it's never been tested in that environment and we never looked at it because we never thought it was a problem, which is another form of what I'm saying, in quotes, is "ignorance."

My own concern is that, with the best of intentions, any organization -- and I think NASA and its contractors may have fallen into this -- when you're so goal-oriented and you're so budget-limited, you tend to put blinders on and you tend to look at -- in my experience here -- they tended to look at the next flight, let's look at getting this next flight off as well as we can. Maybe the old not seeing the forest for the trees comes into play. That's one of the reasons, for example, why we try to get engineers and managers in any organization to understand the end-to-end system so they understand where their portion fits in and maybe will see some of the interactions that go beyond just the performance of their subset. That clearly could have been a problem here.

The Space Shuttle people were under enormous stress, stress from one side of supporting the International Space Station and not being the weak link in the international effort to put a space station up and, on the other side, the very real knowledge that if they could not perform within the budget, there was a risk to the entire Program and, therefore, to their lives, to what they had dedicated themselves to. I'm absolutely convinced that nobody said, "Well, we've got to go ahead. I know we're increasing the risk; but if we don't do that, we could lose the whole Program." That I would be very sure of, knowing the people; but whether they inadvertently missed something because of their zeal and because of their innovative capabilities, remains to be seen.

Certainly they need relief. They're not going to be able to fly for another 20 years under the stress levels that they've been asked to fly under for the last seven or eight. I would liken it to a very taut rubber band. You can only pull that rubber band just so far, and eventually it's going to snap. These folks are being asked to continually pull rabbits out of hats, and you can't do that forever. I am convinced that if they knew they couldn't pull the rabbit out of the hat, they would stop the flight; but as you're saying, sometimes you think you've pulled the rabbit out of the hat and all your analyses say that, and you just don't have the tools to give you the proper insight.

DR. WIDNALL: Or you don't really want to know the rabbit is in the hat.

MR. BLOMBERG: Well, I think there's very little of that. I honestly do believe that the folks on both sides, NASA and the contractor, do want to know if the rabbit's still in the hat. They understand the implications of failure. They are very dedicated to the crews and to keeping everybody safe. So I think if there's uncertainty, they err on the side of conservatism; but sometimes zeal can say that you're certain when perhaps you should have said you're uncertain.

ADM. GEHMAN: Mr. Blomberg, on behalf of the panel, we want to thank you very much for your help here today. You've been looking at this for over 20 years, and your views are very useful to us. We appreciate your very frank answers. We appreciate your willingness to dialogue with us as we attempt to bring our level of knowledge up to yours. Your views are very helpful to us, will make a big difference in the report, and we want to thank you for your contribution. So thank you very much. We'll take about a ten-minute break here while we seat the next panel. (Recess taken)

ADM. GEHMAN: All right. Ladies and gentlemen, we're ready to resume here. We're privileged to have join us today a panel. Mr. Gary Grant is the Systems Engineer in the Thermal Management Group for Boeing; and Mr. Dan Bell is in the TPS, Subsystem Manager for Boeing. I'll invite you to make a statement and give us a briefing or whatever you want to do; but before we begin, let me ask you to affirm that the information you will provide to the Board today will be accurate and complete, to the best of your current knowledge and belief.

THE WITNESSES: I affirm that.

ADM. GEHMAN: Thank you very much. Would you introduce yourselves. Tell us your background and what your current position is.

DAN BELL and GARY GRANT testified as follows:

MR. BELL: My name is Dan Bell. I am the TPS Subsystem Manager for the Boeing Company. I've got 15 years of experience in TPS, TPS installations, materials. Prior to becoming the TPS Subsystem Manager, I was the Manager in the Thermal Management Systems Group in the Huntington Beach facility, also Boeing.

MR. GRANT: My name is Gary Grant. I'm also in the Thermal Protection System. I have 14 years experience, primarily in the operational and turn-around area and requirements. I'm an active member of the LASS subsystem, and I'm acting as an Assistant Subsystem Manager in that capacity.

ADM. GEHMAN: Thank you very much. We're delighted to have you join us today, and we invite you to make a presentation or a statement.

MR. BELL: I think we're here to give you guys a presentation.

ADM. GEHMAN: Go ahead.

MR. BELL: I want to bring up the charts.

Next slide, please. We're here to kind of bring the Board and give an overview of our TPS and RCC systems. In this presentation we're going to talk in some detail about the Reinforced Carbon-Carbon system, the leading edge of the vehicle, and some other components, what we call our high-temperature reusable insulation. I think you all know them better as these are the black tiles on our vehicle. Our low-temperature reusable surface insulation, these are the white tiles. AFRSI or FIB -- each of those are kind of interchangeable names -- those are our quilted soft goods that we use primarily on the upper surface of the vehicle. We have FRSI, flexible reusable surface insulation. These components are a needled felt material used on the upper surface of the vehicle, more durable than our AFRSI material. Then we're going to go into some penetrations and seals, those locations on our vehicle where we have areas that need to be closed out with different thermal barriers and sealing systems.

Next slide, please. Just to demonstrate on a very high level where the RCC and these different components exist. RCC makes up the leading edge components. The nose cap and what's not shown here on the lower surface. We also have the chin panel and what we call the aero head, and that's the forward attach point for the vehicle itself.
Next slide. When we talk about our high-temperature reusable surface insulation tile, those are the black tiles, the upper surface tiles that are shown here. Most have seen the lower surface -- and we'll get into that -- but the entire lower surface of the vehicle is covered by those components.

Next slide, please. Our LRSI tile. As you can see, right around the forward windows and on the forward edge of the OMS pods themselves, we have our low-temperature surface insulation tiles. Next slide. Our AFRSI blankets or FIB blankets that we have cover a large acreage of the upper surface. These components are lower maintenance than are LRSI tiles, and that's the primary reason those were selected over the tile system for that upper surface.

If we go to the next slide. This fills in the puzzle with our FRSI system. This is a felt system, very durable and very maintenance friendly, having workers in and around that vehicle. The penetration seals and thermal barriers, we're going to get to on some later charts.

Next slide, please. When we talk about the environments that our vehicles are exposed to, the first thing everybody asks is what kind of temperature, thermal environments we're exposed to. What's shown here are some data that came off a compilation of data taken from three flights early on in the Program. It shows you a variation in temperatures from the very forward edge of the vehicle, lower surface, ranging from 1900 degrees. Then we have some areas on the vehicle that we'll talk about a little later on that get upwards into the 2500-degree range. These isotherms vary across the vehicle. Our upper surface of the vehicle sees much lower temperatures, generally less than a thousand degrees, and varies, depending upon the location, to as low as 300 or 350 degrees at the top of the payload bay doors.

Next slide. Now, when we go through a re-entry cycle, what we wanted to demonstrate here is the change in pressure; and pressure is an important part of the equation on re-entry. Starting from the time of re-entry, you can see how the pressure actually increases as you get further in the atmosphere, as one would expect. This was taken from a body point forward on the vehicle surface.

Next slide. I wanted to touch base in a little more detail on some heating and some very specific locations. These are some of the more extreme environments that our TPS, our tile systems see. A body point on the very forward edge of the nose landing gear door, Body Point 1024, sees a peak heating of about 2300 degrees Fahrenheit. On the door itself, the temperatures start to decrease as we move aft. We're still getting extremes around close to 2100 degrees there. We do have a very hot region in between the two elevons, the inner and outer elevon. In this region we get some additional heating that causes us to push that tile system upwards to 2500 degrees.

ADM. GEHMAN: There are two lines on each of those graphs. What do they mean?

MR. BELL: I don't think I have the background on this specific slide to answer your question correctly. So we'll get you that data. I do know that the lines that were listed there are the actual temps, though, that were measured at those body points.

Let me go to the next slide. The TPS system is very extremely part-count heavy. We have very high numbers of parts that we have to deal with on a daily basis. Our high-temperature reusable, our black tiles, what's listed on the first line there, is two different systems. One is our LI-900 system, which makes up the majority of the components, nine-pounds-per-cubic-foot tiles. Then our LI-2200 tiles make up a smaller subset of that, and we'll get to those locations on some later charts.

You can look there just with those systems alone. There's about 20,000 tiles on each vehicle. TUFI tiles, which we'll talk about, are our newer introduction to the vehicle; and we have about 306 of those installed on the vehicle. Those primarily take up the base heat shield and upper body flap section of the vehicle at this time.

FRCI tiles, which were an introduction sometime in mid-Program, we have almost 3,000 of those installed. Again, now getting to the upper surface, the upper surface tiles, our LRSI, about 700, actually 800 tiles with varying density of substrates. Then if we look at the amount of area occupied by our FIB or AFRSI blankets and then our FRSI, we're talking over 2,000 square feet for each one of those systems. It's a lot of parts to deal with.

Next page, please. I wanted to touch on how our system goes together, and it's primarily for our tiles. Well, let's start at the top of our system. The tiles are a substrate, which three of the components that we are currently using up there are listed. LI-900, LI-2200, two of the original substrates from day one on the Program, still occupy the majority of our substrate material. We have a material called FRCI 12 which was introduced at a later time. It's got some benefits from a strength standpoint. Then we have what's not listed up there, an AETB-8 material, an eight-pound-per-cubic-foot material that accommodates us the use of a TUFI coating on that surface.

These three substrates have the same coating, our RCG coating, reaction-cured glass coating, over the surface of that. We then take that substrate, the base of the material is densified, and we bond onto that what we call SIP. It's a strain isolation pad. That is bonded to the base of the tile with an RTV system, which is a silicone. It's a two-part silicone system, and that two-part silicone system is then bonded to the structure. We have multiple types of structure that we actually bond to.

One of the features of our design system, as you can see, is this component. This is what we call filler bar. Filler bar allows us to have a seal in between two adjacent tiles. So if you can imagine -- you kind of see in this gap here. If we had another tile that would sit into this hole here, this piece of filler bar would be covered by this tile and then its adjacent tile.

Next page, please. When we talked about the different types of substrates that we have on our vehicle -- and this is a little archaic as far as it's a demonstration of where those parts are located -- the nine pound material, our LI-900 material, as you can see, makes up the majority of our lower surface of the vehicle. It's our primary workhorse from an acreage standpoint. FRCI 12 -- and this is 102, so it has actually less FRCI 12 than do the other vehicles. We have instituted some locations where FRCI 12 has been installed for different reasons.

LI-2200 material is a higher density material that we use in LESS regions, generally around penetrations and a highly-loaded region. It's also used quite a bit around the nose of the vehicle itself. AETB-8 obviously isn't shown here because it's on the base heat shield and upper body flap of the vehicle.

Questions?

Next slide, please.

GEN. BARRY: One question. Could you tell us what percentage of the tiles on the bottom are original tiles?

MR. BELL: We have that data. It's a pretty substantial number. Most of our tiles. I believe the number is about 60 percent. We certainly can get you some accurate data, and I think we have those charts available and we'll make those available to you.

GEN. BARRY: Thank you.

DR. RIDE: Could I just ask how you chose the areas on 102 to put the FRCI tile on? You said that it's less than the other Orbiters.

MR. BELL: Sure. The FRCI 12 tiles were an introduction that occurred after the build of 102. From a design standpoint, those tiles give us some benefit because they have some added strength characteristics that allowed low-margin tiles to be upgraded and in some cases we went forward and upgraded specific areas of low-margin tiles that would benefit from that strength.

DR. RIDE: So it looked like the doors of the wheel wells, the inboard doors of both wheel wells?

MR. BELL: Actually this forward edge, there's a seam that exists under this edge. I don't think it's really driven by the fact that the doors are at that location. Yes. And there's some other very specific areas. FRCI allowed introduction of a stronger substrate that can accommodate relieving some of those low-margin areas that we've had to deal with for 102. We simply installed more of them on the other vehicles to deal with that, but there was still attrition mods where FRCI, on the books, that 102 would have had upgraded at points in time.

Next slide, please. This kind of gives you a feel. You know, you take a look at the bottom of our vehicle and you think that it's a nice, flat surface; but it's really not. We have various thicknesses of our tiles; and our tiles provide some contour to the vehicle, as well. You can see in some of our thinner areas we get down to less than an inch in thickness; and back on the very base of the body flap, we're talking about tile thicknesses approaching four inches in thickness. So a significant variation across the vehicle.

ADM. GEHMAN: And the reds are thicker?

MR. BELL: The red ones would be thinner, sir.

ADM. GEHMAN: Thinner. Then the blues and purples are thicker?

ADM. GEHMAN: I can't read the numbers over there.

MR. BELL: Next chart, please. Talking a little bit more detail about our lower-temperature systems that are used on the upper surface. I talked about AFRSI or FIB blanket. What we have is two glass fabrics: an outer OML fabric which is a quartz, astroquartz material; and S-class IML fabric on the lower surface; and that surrounds a six-pounds-per-cubic-foot-density batting. This is the insulating characteristics of the blankets themselves. Then just as you would stitch a blanket, we actually stitch, using quartz thread, through the entirety of the blanket itself to hold those together.

Now, a little bit different approach is our FRSI material. Our FRSI material is specifically fiber that is felted. This is a Nomex fiber. It is felted together and produces these sheets. Then we apply a silicone coating to the surface of that, and that is bonded then to the structure itself. We have vent holes, too, for obvious reasons. A little lower density. This material is very good around the workforce. Very durable. We actually walk on this material. This is the only TPS component that we actually can walk on.

You can see the difference in the materials is driven by -- we'd love to use this material everywhere, but we can't because of these temperature requirements. That's really what defines those locations where we can put those materials.

Next slide, please. A little more detail. I'm not sure we want to go into a whole lot of this. A couple of features. You know, how do we close out the edges of the blankets? Simply the fabric is wrapped around the edges and then the stitches that we talked about are provided all the way through the blanket itself. Another feature that is interesting about this design is the actual loop part of the stitches occurs at the very bottom of the blanket. That allows this bond line; or when we bond this blanket, these stitches and overlaps are included in that bond line. So if we ever lost -- let's say we broke a thread. We wouldn't be subjected to an unravelling situation where the blanket could unravel itself.

Next slide. When we talk about our tile systems, you'd be negligent to not include our gap-filler systems. In between our tiles, we have a gap. If that gap is deemed to be out of tolerance or specifically designed to be large, then we would come in and include what we call a pillow-type or pad-type gap-filler which, simply stated, it's batting with fabric wrapped around it, similar Nextel or quartz fabrics that we talked about for the AFRSI blankets themselves.

We include a strip of Inconel foil. This Inconel foil provides some stiffness that allows us to handle these parts and install them. We have some features that we include in specific areas for design purposes where we would add a piece of sleeving to the surface of that.

We get down to where we would have design cases. In some areas we want to protect that gap a little more. We actually build into our tile system this lip. This lip protects the gap-filler in that gap a little more, and then we come in with our gap-filler underneath it. And there's what we call a double lip and single lip type of installation. On the acreage portion of the vehicle, we utilize a lot of -- and you'll see a lot of these -- what we call RTV or ceramic-coated Ames gap-fillers. These Ames gap-fillers, you can think of them as almost like playing cards; and we can include up to six layers of these Ames gap-fillers to deal with either out-of-tolerance gaps or to deal with flow conditions that we've witnessed and inspected down the cavity itself. So we'll install those on an as-needed basis.

Next slide, please. The penetrations. Penetrations are a difficult thing to deal with. An ideal vehicle would have no penetrations on the lower surface of the vehicle. Obviously, for many reasons, we don't have that luxury. The major penetrations that we're talking about here are the Nose Landing Gear Door, a very critical area because it's very hot in that region, as well; the mains, which everybody has had a lot of attention on; the ET doors; body flap seals; and then elevon cove seals. On the upper surface, we have our rudder speed brake, we have around our thruster, the forward RCS thruster module around the hatch, and then around our hinge line. There are places that TPS needs to be included. It certainly doesn't get the attention that the big acreage stuff that you can see, but it is probably as or more critical than the other systems.

Let's go to the next slide and talk more detailed about that. There's a lot that goes into dealing with how we keep heat out of these locations where we have penetrations. The nose landing gear door, again, I touched on it being a very critical area. It's very critical because this is in a very hot area, and actually for this nose we have a triple-redundancy NR seal on the forward edge of the nose. There's an OML thermal barrier, what we call a primary thermal barrier, and then an IML thermal barrier; and that is backed up by a pressure seal that we have or an environmental seal, if you will, that seals the surface of the structure together, closing that door itself. This kind of shows the three barriers in place. The reason we have the redundancy here obviously is because of the extreme environments and heating.

Let's go to the next slide and talk Main Landing Gear Door thermal barrier. This shows a difference between an old and new design. It's a pretty good example of what the barrier is itself. We start with a Nextel sleeving and Nextel fabric wrapped around an Inconel spring tube. This Inconel spring tube supplies stiffness into the part that allows it to retain some compressibility. If it was just batting or other material, we wouldn't get a spring-back that we need to maintain our seal.

We used to come and bond in. Every time we had to replace a barrier, we actually had to bond in this barrier into place. Very, very time-consuming. Very labor-intensive. Difficult bonds to make in-situ. A redesign that occurred included a standoff, if you will, that had an attach plate; and this aluminum attach plate snaps into place. So now we have piece parts that we can apply much quicker to include into the design of the vehicle. Helped maintenance significantly.

When we start to talk about elevon cove and even the body flap cove, it's a very difficult area to deal with because it's a dynamic environment while the heating is occurring. We have moving parts here occurring that we have to protect. It all centers around what we call our hinge tube. We have a primary seal here and then a secondary curtain seal on the back side of that. The tiles are designed to protect the seal itself here and here, and then we have actually AFRSI blankets installed inside this cove to deal with any heating that might get through and into that panel itself. The rest of the components obviously have to move in situ with any movement around that part.

Next slide. I wanted to go into a little bit about damage history as far as our vehicle goes and what we typically have seen as far as impacts to our vehicle. We use this greater than one inch as kind of a criteria that we track our larger impacts. There is no significance about that size in particular. For the fleet average, we have about 30 impacts of that size every mission and with a total number of impacts, including the ones that are less than an inch, of about 144 per flight. The average tiles --

MR. WALLACE: Can I interrupt you with a question, Mr. Bell? This fleet average of 30 impacts, can you give an historical perspective on at the very beginning of the Program? I mean, was there an expectation that there would be a number of impacts?

MR. BELL: Obviously you're probably pre-dating me with that question, but I certainly can go back and know that the requirement for TPS is that there would be no impacts to that system. That's in our OVEI document and that still exists today. That has not changed. So early on in the system -- and I've gotten this from those that have preceded me -- early flights, they were even concerned about having cracks in tiles and obviously having to deal with those type of changes and evolving into where we are now.

MR. WALLACE: We've seen these sort of numbers, and they seem to be fairly level. I mean, while there are some extreme cases, the trend is fairly flat. I mean, can you tell me sort of from a standpoint of the TPS program is this something that has just sort of become -- and I know that you don't cause these impacts, you're the victim of these impacts.

MR. BELL: Sure.

MR. WALLACE: But you work with the other elements. Is this just like an ongoing effort to lower this number?

MR. BELL: From the TPS standpoint, we are primarily looking at these numbers and these numbers come out of our post-flight reports that we generate every flight if we see a movement in these numbers or these have been treated as our baseline. Now, what we really look for is anomalies, very large damages, or a case where you would have a significant number of damages that are out of the norm; and that drives us generally to go and pursue that further.

I think if we'll go to the next slide, what you'll see is -- next slide, please. If you look at these impacts, you know, there's a variation from flight to flight. You know, here's a significant case; and I've got another slide that will kind of point out those events. Generally, what we're using this data for is to point out, say, significant event or changes from that baseline that you kind of defined.

ADM. GEHMAN: This slide here is actually *Columbia*?

MR. BELL: This is 102.

ADM. GEHMAN: This is OV-102. This is *Columbia* minus her first five flights, which I guess were considered to be test flights.

MR. BELL: Test flights. And I'm not sure we had collected the data in the same fashion for those flights. That may be why it's missing from the slide. You'll notice that *Columbia* actually had a lower average of impacts than the fleet from a one-inch standpoint. The location of these impacts is pretty consistent. It doesn't really vary a whole lot from the vehicle itself. The TPS system is actually quite resilient. Even though it's quite easily damaged, it absorbs these type of impacts very well. It certainly is a maintenance issue, these sizes of impacts that we're talking about.

GEN. BARRY: Let me ask a question. We discussed a little yesterday about, of course, foam. Really the question came up: Can you design an External Tank that will not shed foam? I think most conclusions are that it's going to be very hard to do that. If you take that assumption and accept the fact that you are going to take some hits, you've already alluded to this design spec originally for the tiles was not to accept any hits.

MR. BELL: That's correct.

GEN. BARRY: Now, if you have history on where these things are traditionally hit, you've already just stated that they kind of reside in the same areas, for the most part. Are there any designs right now to strengthen the tiles so the specifications can be stated as having an ability to accept hits? I understand there's a BI-8 kind of tile. Can you talk a little bit more about that?

MR. BELL: Sure. It's kind of been an evolved process. We started out with our AETB-8 TUFI tiles, and I've got some charts that will actually show you. It's pretty dramatic what these tiles have done for us on the base heat shield as far as reducing impact damage. Again, let me emphasize that impact damage was being driven by a maintenance issue. It wasn't considered a safety issue back on the base heat shield that we were trying to fix, primarily driving towards that. The implementation from that was very positive.

Now, the issue with that substrate is that substrate, the AETB-8, does not have a thermal conductivity or it's not as good an insulator as the base system that we have on the rest of the vehicle. So we cannot go in and simply implement that material because then our thermal load to the structure would have issues from a gradient standpoint or a local thermal impact standpoint.

In 1999, we initiated an upgrades effort to go forward and try to create or design a system that would accommodate a tough coating that would have the ability to insulate where we needed to on the lower surface of the vehicle itself; and what you had mentioned, that BI-8, or in some cases it's called BRI-8, it's still in development. It's actually very close to being completed, and that's something that we'd like to have that tool in our bag if the Program deems that we need to go and do this type of replacement. It's not available today.

GEN. BARRY: The bottom line is the question that could be asked is: What will it take for the Orbiter tile, the TPS tile, to be able to accept hits?

MR. BELL: Well, I think you could approach this in two ways. Certainly we can imagine that these type of ascent hits, we can take those hits now, the typical hits that we have; and we've demonstrated that if we get the sizes of impacts that are typical, our system can deal with those very well. We do have still a maintenance issue that we would have to deal with. That, from a TPS standpoint, we would love to eliminate.

Now, if you're talking about substantially larger impacts than we are accustomed to seeing, then we have to do more homework even to evaluate whether this BRI material installed in these specific locations would provide us the benefit that I think that you're looking for.

GEN. BARRY: We've been told there are about 200 to 400 of the 22,000 tiles on the bottom that are "critical." Is there any attempt to beef those up? Maybe you could explain why those are identified as critical.

MR. BELL: If I could try to clarify, there's probably more like 18,000 tiles on the bottom of the vehicle that are "critical." I would hesitate to be the person that has to pick out one tile to leave off for the next launch. The tiles I think that keep coming up, these two to four hundred tiles, they're primarily the ones around the penetrations. Those are already beefed up per se because those are the higher-density materials. That's not to say that we aren't pursuing higher-density materials that can accommodate a stronger substrate, just like we are on the BRI-8. That work is in process, as well. But really to accommodate increased toughness of that lower surface system, it would be difficult to pick out a specific location. I can kind of take a step back and say which is the critical tile; the critical tile is going to be the tile that takes the impact. If you can figure out what is going to be the location of that next impact, I can fix or increase that durability or certainly approach the vehicle as a whole. But I don't think that you can simply say -- certainly we would make gains by any replacements. I hate to be very specific on one location as being critical.

DR. OSHEROFF: You talk about tile hits that are an inch. I assume that's in diameter at the surface. How deep are these typically?

MR. BELL: And I'm talking in generalities here. Lower-surface impacts are generally very low-angle impacts. So when you're talking about for most of the lower surface -- and I know with some of the work that we've had going on, if you start to look at the acreage impacts, you're talking about less than 10 degrees of angle of incident at various velocities. So generally the crater depth is very much driven by the length of the damage or the degree or mass that impacts it. So generally they're not very deep. We have seen some deeper ones. I would say, you know, a half inch would be deep. Most of the damages that are listed out on the vehicle are typically not very deep.

DR. OSHEROFF: And how deep are the deepest ones?

MR. BELL: That would be data that I would have to go back and pull for you. Certainly we have not just the foam impacts, we've had an impact, STS-27, where we lost half of a tile. A cavity in that one, I would say, would go full depth.

DR. OSHEROFF: Can you say a little bit -- I think it's pretty clear that most of the tiles are repaired rather than replaced. Could you describe a little bit that process, or are you going to do that?

MR. BELL: I wasn't planning on it, but I would be happy to. We really have three basic repairs. We have what we call a coating repair. We could think of it as a coating repair. The coating gets removed from the part. No depth to it really at all. We come in and apply an additional coating over the surface of it to preserve our erosion resistance and our emissivity for the next flight. It's a very benign repair.

Then we start to get into different depths or quantities of, how I can say this, volumes of damages that we are allowed to repair. Those are simply a ceramic slurry is mixed up and applied to the cavity, and what you end up with is a high-density putty. We call them our putty repairs in that surface. Our next level of repair is, if we exceed our putty level requirements as far as sizes that we can repair, we replace the tile.

DR. OSHEROFF: How difficult would it be to apply this putty in space, from a chemical point of view? Forget about gravity.

MR. BELL: Any application in space obviously has its challenges. I think that I would probably like to not answer that question since we have an entire team out there driving towards an on-orbit repair. Certainly the approaches that have been dealt with previously have not been along the lines of a ceramic putty repair like we're dealing with.

Let me approach this from a different question. I think I can answer your question without going somewhere where it's outside of my realm. The putty repairs that we're dealing with, if our damage is that we have returned from space with them, typically they're ascent damages. So those damages existed prior to the re-entry or thermal cycle. So we would really have no driver or no need to go and repair that prior to a re-entry case.

Now, if you're talking about going in and trying to repair a much larger volume, potentially even a full tile replacement, the ceramic system that we're talking about would be way too massive from a mass standpoint alone, I think, to accommodate that, as well as it would not necessarily stick to a fractured tile surface the way that we need to. Generally, we mechanically lock in those repairs, as well as we get some chemical attachment. So I don't think that would be a very good approach, sir.

DR. OSHEROFF: The point is that they are, in fact, working on how to do this. Is that correct?

MR. BELL: There is a flight techniques panel which includes 12 subteams, of which obviously TPS is a big part, that are pursuing this effort.

DR. RIDE: Just a question where you've got this particular slide up. You said that the patterns of debris hits tend to vary from flight to flight. I was wondering whether you had seen any patterns in the hits from certain types of debris. For example, I think these are the products that you guys put together, is that right, so you're probably pretty used to looking at these. I'm just curious whether, for example, foam coming off the bipods has certain patterns that you would recognize when you just went out and started counting these up and looking around the vehicle and putting together a chart like this. Where I'm going is that there are a lot of flights where we don't really have ascent imagery and we don't know where the foam came off. I'm wondering whether, just from your experience with the patterns here, one could go back and take a look at the drawings like this that you've made for each flight and kind of estimate where the foam came from.

MR. BELL: The effort that goes into putting this data together, there's actually a parallel effort that goes into it by an actual debris group. They actually build something very similar to this and they take specific sizes and they are looking for exactly what you're talking about. They're looking for anomalies that they can trace back to sources, and they do a better job than TPS by themselves to integrate those different pieces of data and try to bring that information forward.

You know, the bipod ramp is challenging from a transport standpoint and where it comes off within the launch and where it would actually impact the vehicle. The one piece of data that we have been able to go back in, we had a significant damage on STS-50; and that STS-50 damage was related back to the bipod. I believe, if I'm right, that damage occurred back here. It was, again, a very low angle of impact. We really don't know what the size of the foam debris was. All that we know is there was a relationship between when that came off and the damage that we had. The damage, I believe, was about 14 inches long, if I am pulling numbers out of the air here, if I remember correctly.

DR. RIDE: Thanks.

MR. BELL: Again, not very deep because the angle of incidence is very low.

ADM. GEHMAN: You've got total impacts and you've got impacts greater than an inch. If in any of those flights the OV-102 came back missing a tile, would it have been annotated on there or would that have made your chart somehow?

MR. BELL: We don't have that relationship here. The only tile that I know that we had lost from an impact case was half of a tile, and that was that STS-27 case. I know of no other losses of tiles due to impact. We've had significant damages; but if you're talking about loss of tiles, that is the case. Now, that case, I have to be very specific. That case was related back not to foam but SRB ablator, so a much more dynamic projectile than foam is.

ADM. GEHMAN: That was *Atlantis*.

MR. BELL: Yes. Correct. Would you pull up the next chart. I think it will kind of go down the path of what you're talking about. This is primarily to demonstrate that when we have something that is out of the norm as far as debris impacts, we normally go back or we have gone back and related that to a specific event that was significant. You can see the STS-27 flight, I believe, is in here somewhere and we're talking about those cone ablator and the SRB cork. I'm having almost as hard a time reading it as you.

ADM. GEHMAN: I think one of my colleagues here previously mentioned that even if you take out the spikes, that the trend is flat here. We're not getting any better at preventing damage to your TPS.

MR. BELL: That is correct. We have not seen any significant change in that. Next slide. This is a demonstration. We really didn't talk about TUFI tiles because it really isn't applied to the lower surface at this time. I wanted to show you what it did for us on the lower surface. On the case on the left, it's not as easy to see; but all of the tiles had been replaced except for this tile in the center. You can see the damages that occurred on that specific tile. On the right-hand side, these two tiles were replaced. And you can see the gray marks are previous damages. So these are repairs that we had done from flight to flight, all the gray in this photograph.

ADM. GEHMAN: These are your putty repairs?

MR. BELL: These are actually what we would call slurry repairs, sir, where we simply paint the slurry on to eliminate the erosion resistance. We don't really have an aero issue on the base heat shield of the vehicle. What's significant here that you can see is all the little white marks. Those are from a single flight. Those are damages that we would have had to repair from a single flight. The TUFI tiles have virtually eliminated our need to do repairs like that. So from an operations standpoint, it was significant for us.

GEN. BARRY: I understand also the TUFI tiles shrink. Is that correct?

MR. BELL: That's incorrect, sir.

GEN. BARRY: Incorrect.

MR. BELL: The TUFI tiles, if we were to put that TUFI coating on our existing substrates, those substrates, when we would fire them, cannot handle this type coating and those parts would shrink to something that would not be usable for us as a system.

GEN. BARRY: So with the coating, they do not shrink.

MR. BELL: Our AETB-8 substrate with the TUFI coating on it, it's a very stable material. Next slide. Okay. This is the point that, unless you have any more questions about TPS, I'll hand this over to Gary.

GEN. BARRY: Just one other question. Do you know of any systematic studies to identify critical damage scenarios? It really alludes to the fact that if you can trace where the hits have been and we can get some kind of data base, which we've asked for, to be able to say, okay, 80 percent of the hits have occurred on this part of the underside of the Orbiter, then we can take up maybe the issue of how you want to strengthen it even further to be able to accept hits. So we're really talking about damage scenarios. To your knowledge, is there any damage scenarios that have been done?

MR. BELL: I don't know of any, sir.

GEN. BARRY: We still have that question out. So we're looking for the answer.

DR. OSHEROFF: Can you tell me how much would it increase the weight of the Orbiter if you were to replace, let's say, the 500 -- I know you don't like the word "most critical" tiles -- with TUFI tiles? Roughly speaking, how much extra weight is it per tile?

MR. BELL: From a weight standpoint, these new tiles that we're talking about do not include a weight penalty.

DR. OSHEROFF: Really?

MR. BELL: Yes. We're actually closely approaching the weight. So if you ask me if it would be significant, I think it would be very insignificant.

GEN. BARRY: But there is a difference between LI-900 and TUFI tile.

MR. BELL: Well, the LI-900 is the substrate density, nine pounds per cubic foot. We have an RCT coating on that which applies mass to that system, and you get a weight. We started out with our AETB-8 or a BRI-8 material, which is eight pounds per cubic foot substrate. It's a lower-density substrate to start off, and we're adding our mass at the coating where we get the benefit out of the impact resistance. Does that make sense to you?

ADM. GEHMAN: Yes. So it's close to the LI-900.

MR. BELL: Very close. The new BRI-8 system is very close.

GEN. BARRY: Now, there's a difference in BRI-8 and TUFI. I guess that's the question.

MR. BELL: TUFI. You can think of TUFI as -- the Ames guys might get upset with me here -- but AETB-8 and TUFI coating is intended to be a system. That system was intended to be used as a single product and we kind of have gone away from that and looked at applying that TUFI, which we really refer to it primarily as a coating and not an article, and we're looking to apply that material to another substrate, per se, that allows us to utilize this in different locations.

DR. LOGSDON: Let me see if I can reconstruct and understand something you said early on in your presentation, which is that most of the damage to tiles that you've seen has happened on ascent and, since the vehicle has survived successfully re-entry, you do not treat these as flight safety issues but as maintenance issues. Is that a fair summary of what you said?

MR. BELL: These are ascent impacts that we have no control over fixing them on-orbit, from the standpoint of when these parts get back to Kennedy Space Center, whether that's through Edwards or landing directly, it is an operations issue to have to do and deal with the maintenance associated with that. We have a baseline of impacts that we have seen historically that fall into that category.

DR. LOGSDON: Even though you have a stated requirement of zero impacts, that's at this point kind of irrelevant to reality. The baseline is 30 or so inch impacts expected per mission and a judgment that that's acceptable.

MR. BELL: That judgment is probably not one that I should address. All I can tell you is what we've dealt with from a typical standpoint as far as operations go, and you've seen the numbers and that's demonstrated to have been, looks like, my interpretation, something that has been longstanding.

MR. WALLACE: As we've learned a lot about the Shuttle System, even the parts of it that may have originally seemed fairly simple and straightforward turn out to be very, very complex; and we talked this morning earlier, as witness, Mr. Blomberg, about incremental improvements. My question is if you were to design -- let's just assume that we're going to build a Space Shuttle again that's going to be essentially the same vehicle but we have a clean sheet of paper and today's technology to design the Thermal Protection System. Any general thoughts on what it might look like and if it might, in fact, be a lot simpler than what we have now? Either of you can answer that question.

MR. BELL: Let me take a stab first. The vehicles, as you see them, are in evolution. If you look at the vehicles and say that vehicle, that was the design originally -- there's been several iterations and changes to the vehicle through time. So as the TPS community, we continually make modifications and changes to improve both safety and operations.

Maintenance drivers, those changes occur continuously. And there are requests for changes on the books today that we will continue to pursue and you will see this vehicle evolve from what it is now. If we had to start from a clean slate, that allows us to do other things that we wouldn't necessarily have an opportunity to do, given our current configuration and some of our tiles.

Your specific question, I think, referenced the complexity of the design. Sitting here, thinking about the complexity of the design, I do not see any major changes unless you would start to approach the structural part of the vehicle and the way that the penetrations are originally designed that would benefit TPS necessarily. Certainly you would have to integrate TPS into your design up front so that we are not just the insulation system going over a door. You'd have to design and think differently how you would approach the seals.

Let me give you an example. Maybe Gary's a better one to look at this. The chin panel is an add-on to the vehicle. The chin panel is an RCC component that attaches up -- it lays against the nose cap of the vehicle. That was an add-on. Well, the interface between those two components has created a gap-filler that is just very maintenance-driven for us; and certainly if we had the opportunity to start over, we would design that out, design a different interface there. But, you know, what we've got now is an evolution of TPS that you see. Is that a good synopsis, Gary?

MR. GRANT: To take a step back from like what Dan's saying, I think if we were to do something different, we'd look at the most maintenance-intensive areas from a standpoint of refurbishment and from the interface end. It would require more than just a change in the Thermal Protection System. So inputs that we have may also drive changes in the way that the penetrations, doors, or seals would function. But the chin panel is a good example and it may be something that we talk about. But at the time that that interface was designed, there was talk of putting another seal which would basically bridge those two together. Unfortunately the maintenance, you know, downfall wasn't seen in the future; but that would be a good example of something that we could change without causing another change to the rest of the function of the Orbiter.

ADM. GEHMAN: Please go ahead, Gary.

MR. GRANT: Okay. So then we'll talk about the leading edge structural subsystem. Next slide, please. As Dan alluded to, although it is a subsystem unto itself, it is part of the overall Thermal Protection System of the Orbiter. In this first slide, we talk about some of the basic requirements. It was put in areas where you do have the higher temperatures. So we've got multi- and single-mission limits that were posed to the design element. Part of it also is not just, of course, for example, on the wing leading edge to provide a shape there but, of course, you have to protect the internal also. So internal insulation is part of the design requirements.

Of course, being that most of the parts other than the aero head are on leading edge areas, the aerodynamic shape's important. The air foil shape needed to be maintained for flight; and also on these leading edges where we have the highest heating, it's roughness- and waviness-critical. The system needed to be able to distribute loads amongst the system itself and to the structure, the supporting structure.

The impact resistance. The main component or actually the only component that was really designed to withstand a very adverse impact was the forward ET attach plate, which actually in the original design was tiles, and then they ended up doing a functional test of the explosive bolt and found tile damage and this actually ended up being somewhat of an afterthought retrofit. RCC was already in place and in development for the nose cap and the wing leading edge. When we talk about impact resistance, that's the one element of our subsystem that was designed to take a known or expected heavy load or shot.

GEN. BARRY: Do you know what the measurement of that is? I understood it was like .006 foot-pounds. Do you know that?

MR. GRANT: I think we might get to that. There's some slides that talk about the impact testing that was done in the development.

GEN. BARRY: Which was very small, by the way.

MR. GRANT: Yes. I guess the point is that impact resistance, you know, other than the forward ET, was more for foreseen handling damages and kind of rain impingement and bugs and things like that, as opposed to a real protective shield. Then the last thing is that the parts being new and really not much of a way to test, they had to be certified by analysis; and in that process it's found that they are limited life, which in the Orbiter, actually Space Transportation System, whatever element you're talking about, limited life or cycle means that it's not something that you can install and it's good for the hundred missions or 20,000 cycles it's going to see in its life.

ADM. GEHMAN: What does certification by analysis mean?

MR. GRANT: Well, these parts, you know, some of the portions were tested and rated at facilities and/or checks, but the actual parts themselves were not able to demonstrated on any other type of vehicle.

ADM. GEHMAN: All right.

DR. WIDNALL: I have a question. I don't know whether you're going to get to this later, but are you going to talk about things like the fatigue life of these panels and vibratory loads and things like that?

MR. GRANT: Yes. If we don't -- I mean, if the charts don't cover what you need. Then the final thing is that the parts need to be interchangeable.

Next slide, please. The LESS consists of more than the carbon. In the investigation and discussions, we've really focused on the RCC, Reinforced Carbon-Carbon parts themselves. In the system there's a nose cap that has three expansion seals and five TEE seals to make up the nose cap assembly. The wing leading edge is made up of 22 panel seals sets per side, or 44 per Orbiter. And as Dan mentioned, a chin panel was retrofitted on the panels. It was in an area where we ended up having a lot of tile and gap-filler rework, and this actually spans between the nose cap and the leading edge of the Nose Landing Gear Door and the forward External Tank Attachment Plates.

For the carbon to work, it has to have attachments, internal insulation to protect the structure that the parts are attached to, the attach fittings themselves; and then in all cases other than the External Tank aero head, we have to be able to access our fasteners. So we use reusable surface insulation tiles and gap-fillers to make access panels. In general, the basic design goal was to provide thermal structural capabilities for the areas that exceeded 2300 degrees.

Next slide, please. Let's talk about the RCC now. In general, the makeup of the Reinforced Carbon-Carbon, there's three breakdowns. So there's actually kind of two main ways of viewing it or two main entities. One is the actual load-carrying part itself, which is the carbon substrate. It's made up of a rayon fabric that's impregnated with great amounts of graphite and then there's a resonance used to help it lay up in a rigid fashion and then there's a very detailed three-stage process that's used to convert it to a carbon matrix. In and of itself, you could almost be complete with your parts right there except that we have an environment that is going to attack that substrate through oxidation. So that's where the silicone carbide coating and the TEOS and the other sealants come in. So the purpose for the silicon carbide coating is to protect the underlying impregnated carbon fabric.

This is actually not a coating that's applied. It's actually a transformation. It's accomplished by a dry pack in a powder that's made up of silicon carbide, silicon, and aluminum powder. Ideally, our coating is about 20 to 30 mils thick. Of course, it gets thicker when you get to some of the sharp edge and the bends, just due to the geometry, the way the shape is.

Unfortunately, during the cool-down, due to the difference in the thermal expansion between the carbon substrate and the newly converted silicone carbide coating, there's a difference in the thermal expansion coefficients and the silicon carbide contracts more during the cooling and we get craze cracks, if you will, which affects the substrates, potential oxidation. So the next element, that's added to help this. First the TEOS is applied, which leaches down through the craze cracks into the carbon areas to help form a harden or another way of protecting the carbon substrate. Then once this is completed, a Type A sealant, as we call it, a glass sealant is applied which helps to fill in some of the craze crack areas also and, again, give additional oxidation protection. The early design had just a single application of this coating, and it was discovered about the time 105 was being built that actually a second application of this coating would be beneficial for mission life. So some of the 105 and then subsequent spare parts have actually a double Type A coating.

DR. WIDNALL: What's the density of the material?

MR. GRANT: You know, I don't have the actual number. It's, on the order of tiles, you know, magnitudes greater. I don't know the actual number of the density.

DR. WIDNALL: I mean, it's got to be a heavier density than tiles.

MR. GRANT: Yes. By magnitudes. Down at the bottom right, you see a typical acreage is on the order of a quarter-inch thick. Then as you transition to lug areas where the parts are actually attached or some of the areas where you get the curves due to an actual geometry change, you actually get quite a bit thicker. In some of the lug areas, you're close to a half an inch thick.

Next slide, please. This is a good snapshot at all the parts installed on the vehicle. Of course, we have the nose cap and associated seals. Behind this, there's a row of access tiles; and it actually allows us, if we need to, to change gap-fillers behind this area. The chin panel, which actually here you can see just the edge of it, access panels located out on the edges and then actually you reach through the nose landing gear door, which you barely see here, to reach in to get the attachments and then you get a view of the chin panel and the seals, just on the edge.

Up on the right, we see the wing leading edge panel attached to the ship. This actually is a photo of a 103, and so its configuration is a one-piece spar fitting. In another slide that's coming up, you actually see the two-piece spar that 102 had. But you get a good look at the leading edge rib of an RCC panel there. You can actually see many of the insulators and some of pieces we're looking at. The Koropon-coated spar is shown there.

Then the forward ET, actually you can see the forward ET attach point. This is evidently a post-flight photo on the runway. This is what that installation looks like on the runway, and there's actually an aft plate and then a forward plate and then that interfaces with the nose landing gear door.

Next slide, please. Here's a little more detail of the system itself. The nose cap is actually somewhat of a self-contained unit. The nose cap actually has its own bulkhead, own structural bulkhead, which is the nose cap and the seals. Internal insulation of the conic blankets, which you see a cross-section of here. Of course, it's attached by way of Inconel fittings to the actual nose cap bulkhead, which then the whole assembly is put onto the Orbiter vehicle and attached to the forward fuselage structure. Interface panels which actually go all the way around and interface with the forward fuselage and then a bulkhead door which allows access into the nose cap and then the conic blanket internal insulation assemblies are actually broken down into four quadrants. And that's the way that you get those in and install them to the nose cap bulkhead itself. Next slide, please. Wing leading edge parts. You see here, sitting on the bench, a panel with attached T seals. This is a panel T seal set. You can see the attached lugs here. T seals are attached to the actual lug fittings on the panel, not directly to the ship.

As you can see, this is a cross-section. The purple is the RCC itself. Upper access panels that allow us to get to these attach points here. Upper panel. Lower access panel shown and installed here, which again allow us to get to the attach fittings.

The spar fittings -- and this picture here does show the 102 configuration. There's a separate upper and a lower spar fitting, and those are shown by red in the sketch here. Then once everything's installed and complete, before access panels are put on, the spar insulation in the different -- the earmuff insulators here -- again, this is 102 configuration -- actually go over and cover the spar fittings so that once everything's completed on the internal, all parts are protected from radiation.

DR. WIDNALL: Are you going to talk about any structural testing that was done on these RCC panels?

MR. GRANT: I think the slides that we'll talk about have some of the impact testing. I don't know --

DR. WIDNALL: Well, let me just ask a question. Are you surprised? The thing that surprised me about it is that in recovering the debris, we found half of the RCC panels. In other words, they broke at the center. Now, looking at that, I'm asking myself, if I grabbed ahold of that panel and, you know, pulled it out, where would it break? The rib is a little thinner in the center. I mean, do you have an understanding? When you saw that debris, did you say, uh-huh, or are you as confused as I am about why they broke where they broke?

MR. GRANT: I think the loads those parts saw -- you know, I don't think it's surprising that they broke there; and one of the things that we saw the parts, you know, broke at the lugs, too.

DR. WIDNALL: That's fine; but, I mean, really every single panel we have is broken at the leading edge.

MR. GRANT: Yes. You know, if you notice, we don't have any -- we have some T-seals or gap seals --

DR. WIDNALL: Right, but I'm talking about the big panels.

MR. GRANT: -- in somewhat good condition, but the panels themselves, I don't know that any of them --

DR. WIDNALL: Well, we have a lot of half-panels. Half.

MR. GRANT: Yes. You know, I don't have that specific information. I know there had to have been compressive and stress testing, and that's something that I could take an action and make sure you see that data.

DR. WIDNALL: I'd be interested in that.

DR. OSHEROFF: This is pursuant to Sheila's question. Certainly looking at the debris, it was my impression that a lot of these things had to have been broken. They didn't break upon striking the ground.

DR. WIDNALL: No, I don't think so.

DR. OSHEROFF: Well, part of it at least was still attached to the wing. That seems to be more -- because you could see that there would be spatter on one half and not on the other half.

MR. GRANT: Well, I've been somewhat involved in the reconstruction. One of the things that we tried to not do -- I mean, other than, like the doctor was saying, you know, of course, thoughts are running through your mind -- but we've specifically tried not to speculate on where did they find these parts -- you know, "Oh, my God, this is the one right here." We really tried to systematically place them; and, as you know, it's an important part of investigation to make sure we get the correct parts correctly located on the floor.

In general, observation-wise, I've personally seen very few parts that show a lot of ablation to the actual substrate. I mean, it's really impossible to speculate as to when they broke; but a lot of them, I'm not seeing a degradation of that, the carbon and the fabric substrate that you would see, you know, had it broken early in the re-entry attempt.

ADM. GEHMAN: Why don't you go ahead.

MR. GRANT: So I think this covers the basic assembly of the wing leading edge system. Next slide, please. The parts were procured to a spec that was developed through NASA and the vendor. Performance is that they should be structurally sound, maintain a positive margin of safety -- which, of course, the factor of safety baseline is 1.4 -- be able to withstand 100 missions with minimum refurbishment and replacement, be able to withstand rain impingement. Physically the system, the goal was 1699 pounds. Of course, you had to maintain and be able to have step-and-gap control adjustment, which is built into the design; and the surface roughness within any part had to be less than the figure shown there.

Impact resistance was really more of a goal from the standpoint of, you know, if you talked to the vendor today, their biggest concern is handling damages on these. So, in general, the goal there was to create some type of impact resistance if somebody dropped a nut or a wrench or some of the things that would happen in normal processing -- other than, like I mentioned before, providing a protective shield. The maintainability. The visual inspection would give you clues into any concerns you have with the parts. Part removal should be somewhat straightforward and simple and shouldn't take very long. Less than 15 minutes was used as a number. And again, that they should be interchangeable. Predicted temperatures that were presented to critical design review in March of '77 showed the maximum temperatures on the nose cap were around 2500 degrees and, wing leading edge, 2600. On the panels, the gap seals actually are a little bit hotter at 2800, close to 2900 degrees.

GEN. BARRY: Can you go back a slide, please? The 100 missions. My understanding is that certain panels are a lot lower than that, like Panel Number Nine on the lower part is only cleared to 50.

MR. GRANT: That's correct. So part of what's integrated here is that, you know, your spares or your extra parts on hand actually are necessary to help you achieve that. I mean, obviously the design for the Orbiter was 100 missions. So the reality of the RCC and the leading edge structural subsystem was that individual parts -- you know, one part, without being replaced, was not going to get you there.

GEN. BARRY: That's an appreciation for, I guess, an analysis that has been subsequent to the original design spec that you've concluded that, okay, for nine. Then it varies, too. I mean, 10, I think, is 63; and then it goes out and gets to 100 on the outer.

MR. GRANT: Yes.

GEN. BARRY: Let me ask you a question on mass loss. There is mass loss to these RCCs over time.

MR. GRANT: Yes.

GEN. BARRY: Okay. Can you talk a little about that and how we talk about ageing? I mean, there is an effect over time on these RCCs.

MR. GRANT: Yes. Well, actually the early mission lives on these parts was quite a bit lower than what you've seen in our current requirements. Initially the Type A sealant was not a part of the system; and then once the sealant was added and then the double Type A was added, we actually began to get the parts to where they were more robust. Then since then we've had to go through performance enhancements and different types of things where the capabilities of the Orbiter were expanded. So, you know, over time those things tended to jostle around the actual mission life itself. So initially the flight lives were actually a little lower than what you had -- I'm sorry, what was the question again?

GEN. BARRY: Well, I guess it really comes down to the fact --

MR. GRANT: Talking about mass loss.

GEN. BARRY: The RCC's a quarter of an inch thick.

MR. GRANT: Yes.

GEN. BARRY: I mean, if you add the Type A sealant, the TEOS, of course, the substrate, the silicone carbide. Now we introduce mass loss of about .003 pounds per square foot, right? The thing is how do you measure this ageing, you know, for the mass loss?

MR. GRANT: Well, what we've done over time -- the biggest thing that we have that really corroborates some of the assessments on that is destructive tests that we've done, and we're able to take a look at that. Mass loss, of course, is related to oxidation; and in looking at that, one of the things that came out of some of those early destructive tests was the sealant refurbishment which we have instituted. I think it was around the 1992 time frame where every other LMDP on the wing leading edge panels that are in the areas where you have the highest convective mass loss get their sealant refurbished, in a sense, really kind of reset those parts in the way of having a higher resistance to the convective mass loss.

I think one of the backup charts I have shows, if you never do a seal quantity refurbishment, how that mass loss increases with time. As you do it, it actually brings that number back down, not quite to the design but a lot to something that's manageable. And our every other LMDP effectivity that we have on that's actually a little bit conservative by a few flights.

So I guess the destructive tests and the evaluations on the parts that we've had -- and most of the models that were used to predict the actual life, you know, using the extrapolation of the mass loss, were very conservative. The trajectories that were used for those -- like the initial flight lives are using abort trajectory, which, of course -- you know, and they basically ran a hundred abort trajectories. So that's very conservative to what we're actually flying, which is normal mission with a re-entry.

ADM. GEHMAN: At KSC, do you do any acceptance testings of the parts from the vendors to see if they meet these criteria?

MR. GRANT: Well, they have this procurement spec that's something that they are held to and that we are, too. The actual receiving and inspection is something that's done in the logistics area. So from an engineering standpoint, you know, we would do our normal maintenance inspections before parts are installed; but we don't actually make a decision to accept the part. Obviously if we saw something that concerned us that may have --

ADM. GEHMAN: Obviously. So to pursue the business of the acceptance inspection, we've got to pursue that someplace else. Dr. Logsdon had a question.

DR. LOGSDON: Just a quick question. A couple of weeks ago, ten days ago, there was some suggestion in some press accounts of the primer from the launch tower having an oxidation effect on the RCC. Did you, in fact, see evidence of that?

MR. GRANT: Specifically on *Columbia*?

DR. LOGSDON: No. In general.

MR. GRANT: Yes. I guess that's something that we eventually would have gotten to. You're referring to the pinholes. When we first saw the phenomenon, we obviously didn't know what we were dealing with; but through quite a bit of time, study, tracking these, we were very concerned about them. We spent hours and hours looking at them, mapping them, measuring them, just dissecting them every which way you could. We were trying to narrow in on there was a point in time -- and I believe it was STS-50 -- where from that point on we found them, we found them only on certain parts, and we somewhat -- you know, trying to find a cause for this, we ended up finding out that there was a change to the way that the pad was being refurbished. And I think some of the other members of the Board that have been in contact with us have quite extensive reports on this. But we found there was a zinc-based primer that was being used and then an overcoat -- and I'm not familiar with the materials on it. But at that time the change was to not apply that overcoat. And the zinc was one of the elements that we were finding in the glass deposits that were coming out of these pinholes, so to speak.

So basically what happens, the zinc does break down the matrix of the silicone carbide and actually gives us a little path down; and the nature of it, too, is that it follows the paths of the craze cracks and imperfections. So it's not necessarily a straight hole down in, once we actually took some destructive tests. But the zinc was the key to us actually linking it to what had changed on the pad. But that fell into line with all the other findings that we didn't really see them on the nose cap or chin panels and it was in the wing leading edge in certain areas that were covered by the rain protection.

DR. LOGSDON: Have you done anything about that?

MR. GRANT: The procedure to the pad or --

DR. LOGSDON: No, the --

MR. GRANT: To the parts.

DR. LOGSDON: Have we covered the primer?

MR. GRANT: So, I mean, are you talking about to our parts?

DR. LOGSDON: The launch pad.

MR. GRANT: Yes. At that time -- and I don't have the information -- but we were able to get in touch with the facilities crew and basically the procedure was changed again. And as we continued to analyze and track the parts, the pinholes, they actually formed somewhat of a glass coating down the actual path that leads to the substrate, which actually gives, itself, some protection to oxidation. So we've done quite a bit of studying and set criteria for the size of the pinholes that are acceptable and the cycle time that we review them.

DR. LOGSDON: My question was: Have you now painted the launch pad to cover the primer?

MR. GRANT: Yes. Once that was identified positively as a source, that was immediately taken care of.

DR. OSHEROFF: Can you estimate how much mass loss occurred as a result of oxidation due to the pinholes?

MR. GRANT: The actual oxidation is preferential to the silicone carbide and carbon substrate interface. So what we've found in the few that we've seen that actually go down to the substrate, because that pinhole actually forms a glass coating around it, what we're concerned about is the oxidation that would actually separate the coating from the substrate itself. So because of that glass lining, so to speak, it somewhat protects those edges from actually getting the attack that we're concerned about.

ADM. GEHMAN: I didn't hear an answer to the question there. Have you ever measured the mass loss?

MR. GRANT: Well, what we've seen is that we are not getting any additional attacking of the interface of the silicone carbide to the carbon, based on the pinholes. That's one of the reasons why we go and do a sealant refurbishment, which somewhat fills in that pinhole temporarily; but once the zinc is present in that matrix, it's impossible to get it actually removed.

GEN. BARRY: But right now you're doing that visually.

MR. GRANT: Yes.

GEN. BARRY: The Board is very interested, of course, in further NDI, you know, to get verification on what that mass loss would be, and not just do it on a visual indication, to be able to look down there and see if there are voids underneath those pinholes to see if, in fact, there has been, in the admiral's term, the termites that have dug holes underneath there.

ADM. GEHMAN: Why don't we go ahead. We're a little bit over time here.

MR. GRANT: Next slide, please. This shows a predicted trajectory temperature pressure curve for a space station mission, one showing wing leading edge Panel Nine, which is our highest-temperature wing leading edge panel, and the nose cap, which itself is a very high-temperature part.

Next slide, please. The design allowables for impact resistance. A test was performed. LTV is the vendor for the parts. Tests were done with a spherical steel ball; and for a typical 19-ply acreage area, it was found that the threshold for not seeing cracks or damage to the coating or substrate was 1.4 foot pounds, which is approximately the 16-inch pound design goal that they had.

There were hypervelocity impacts that was done in '77. They used nylon cylinders and glass spheres and, again, the lower-energy impacts produced some front face damage, the higher energy produced a front and back face damage, and the glass spheres only produced front face damage. Next slide, please. Ice impact tests were performed at Southwest Research and, as you can see, again, as the energy goes up, you start to get cracks into the coating and then, finally, at a high enough energy, the specimens were actually destroyed. The low-velocity impact tests are probably the most consistent or useful data, and these are actually things that are used when we have concerns on damage that may create a hole in a panel or whatever. But the results here, again you see, as you get the higher energy, you get damage to the front and rear face. And with the lower energy, you get some damage to the coating on the outside.

Next slide, please. A low-velocity impact test was performed on a right-hand Panel Number 10, which was actually a panel that we did sustain a couple of impact damages while we were on orbit. Once it was removed, there were tests done by Rockwell and NASA on this. The Rockwell tests used a BB and a lead bullet. The idea there was to kind of demonstrate the effect of the hardness of whatever the projectile was. The lead, of course, is soft, did not produce any damage; and the BB actually saw front and rear damage to this panel.

Next slide, please. Some of the issues that we've had over the years. This panel that I alluded to, the 104, STS-45, we actually sustained two damages on the upper surface of this panel. An evaluation was done to determine, you know, the micrometeoroid orbital debris effects. Concern, of course, is that a potential such damage could actually create a hole in the RCC panel which would very quickly compromise your wing spar. Of course, the burn-through would be a potential loss of crew and vehicle.

The resolution was that a study was done to enhance the internal insulation and to provide a little more margin there, where if you actually had a hole that was a quarter inch or smaller, although the hole would grow during re-entry, your cavity heating would increase, but the actual spar itself would remain protected by a more robust internal. Inside the Inconel foil, there's actually some fabric, high-temperature glass fabric layers that were added to essentially just give you enough margin to return safely. That's one of the things that came out of that actual event.

Next slide, please. I think we have some pictures of it. Actual pictures of the OML or the outer mold line and then the underlying damage. On the left you see what's called Impact One here. It's about two inches by an inch and a half wide. And then an associated back face damage, which you can barely see some of the cracking that happened on the back face. On this side, this edge is a little stronger since it's close to an actual rib of the panel. Damage wasn't very big on the front; but then the actual back face, some of the coating actually was dislodged there.

Next slide, please. This demonstrates some of the pieces that were used for the testing. Of course, with a quarter-inch hole, we're trying to provide -- just a little bit extra protection here. So some specimens were created with a hole of such size. You see what the hole grew to and you see what the actual -- this is the material that's used that covers the internal insulators. And this has the Nextel fabric around it and you can see at the end of the test, you actually still had some protection there. Next slide, please.

That's it.

ADM. GEHMAN: In one of your first viewgraphs up there where you showed the cross-section of the RCC wing leading edge panel, you referred to the matrix and then the way the outer few mils are treated to provide -- your viewgraph said that the carbon is there for the strength and the outer piece is there for the protection. We mentioned oxidation, but is it correct to characterize the outer treatment also as the major part of the thermal protection also?

MR. GRANT: No. Again, the --

ADM. GEHMAN: The whole thing is for the thermal protection.

MR. GRANT: To protect for oxidation, yes.

ADM. GEHMAN: For what?

MR. GRANT: Well, the primary elements that actually provide the thermal protection to the Orbiter are the internal components that protect the wing leading edge spar from the radiation of the parts. The parts themselves, since they can sustain temperatures up to, you know, 3,000 degrees, the parts themselves, you know, that coating is not the primary protection for the actual RCC.

ADM. GEHMAN: Unlike the tiles, which are nearly perfect radiators, the RCC is not.

MR. GRANT: Yes.

ADM. GEHMAN: It's just supposed to take the heat and stay structurally intact.

MR. GRANT: Yes. That's correct.

DR. WIDNALL: You actually didn't say very much about the requirement, the fatigue requirement and how that was tested, what the requirement actually was in terms of vibration levels or whatever. I recognize you don't have that on slides, but I'd be very interested to see the kinds of requirements that were set for basically the fatigue life of the panels in that environment.

MR. GRANT: Okay. You know, the details on the type of cycle testing, obviously the parts were designed to withstand the thermal, vibroacoustic, all the stress. So all those environments, you know, were things that the parts were designed for; and I'd have to get that.

DR. WIDNALL: I'd be interested in knowing what that was.

ADM. GEHMAN: Anybody else? All right. Gentlemen, thank you very much. Your depth of knowledge on this is very impressive; and we appreciate you bearing with us as we work our way through this. I know you want to get to the bottom of this as much as we do, and we thank you for dialoguing with us and being patient with us as we work our way through this. You've been very helpful.

Thank you very much.

Okay. We are finished. We're going to have a press conference right here in 30 minutes.

(Hearing concluded at 12:28 p.m.)

APPENDIX H.8

April 23, 2003
Houston, Texas

***COLUMBIA* ACCIDENT INVESTIGATION BOARD PUBLIC HEARING**
WEDNESDAY, APRIL 23, 2003

9:00 a.m.
Hilton Hotel
3000 NASA Road 1
Houston, Texas

BOARD MEMBERS PRESENT:

Admiral Hal Gehman
Rear Admiral Stephen Turcotte
Major General John Barry
Dr. John Logsdon
Dr. Jim Hallock
Dr. Sheila Widnall

WITNESSES TESTIFYING:

Dr. Milton Silveira
Mr. George Jeffs
Mr. Owen Morris
Mr. Aaron Cohen
Mr. Robert F. Thompson

ADM. GEHMAN: Good morning. The Columbia Accident Investigation Board public hearing is in session. Today and this afternoon, we're going to deal with various types of risks. We're going to listen to a number of experts and talk about their view of risk management and risk mitigation and how risk is looked at from about five different angles, particularly as it applies to manned space flight and the Shuttle Program.

This morning we're going to look at risk as it applies to the original design and construction of the STS. Later this afternoon, we're going to look at risk from the point of view of experts on aging aircraft. We have a couple of experts going to testify and talk to us about how risk migrates over a period of time as aircraft are used. Then later in the day, we'll have Professor Diane Vaughan who will talk about organizations and how organizations deal with risky enterprises.

For this morning, the Board is very fortunate to have a wonderful panel with years and years, maybe decades and decades of experience in this particular enterprise, the STS system. The *Columbia* Accident Investigation Board would like to thank the NASA Alumni League for organizing this panel -- and a very special thanks to Norm Chaffee, the president of the Johnson Space Center chapter of the league -- for helping us to arrange this panel that we have in front of us.

What I'm going to ask, Panel Members, is if you would, first of all, go right down the row in some order or another and introduce yourselves and including in your introduction, if you would, say a word or two about the official position you had when you were involved in either the Johnson Space Center or the STS or Shuttle Program when you were actively engaged in running it. Then when you're finished with that, I would invite you all to make any kind of an opening statement that you would like to make; and then we'll proceed into questions.

So if I could ask you to start at one end or another there, and maybe with Aaron there, and introduce yourself, including a little background of your involvement in the Space Transportation System.

AARON COHEN, ROBERT THOMPSON, GEORGE JEFFS, OWEN MORRIS and MILTON SILVEIRA testified as follows:

MR. COHEN: Good morning. Thank you. My name is Aaron Cohen and I was the first NASA Space Shuttle Orbiter Project Manager from 1972 to 1982. This period of time encompassed the design, development, and the first four flights of *Columbia*. I retired as the Johnson Space Center Director in 1993 and I taught at the Texas A&M University from 1993 until 2001. I am now Professor Emeritus of Engineering at Texas A&M.

During this period of 1972 to 1982, there were many design challenges on the various subsystems and the integration of the subsystems into the basic vehicle. This included the structure system, the life support system, the environmental control system, the Thermal Protection System, which were the tiles and the carbon material, the thermal seals, the avionics system, the auxilliary propulsion system, the hydraulic system, and the many mechanical systems such as doors, actuators, and tires.

I would like to say that we have a very good documentation of this activity, and it was prepared in 1993. It was a compilation of papers presented at a conference held at the Johnson Space Center in June 28th to 30th of 1993. This documents the design challenges of all the Shuttle systems. The papers were prepared by the NASA and contractors' subsystem managers, and the subsystem managers were the backbone of the Shuttle design.

This is my introduction statement. I will be happy to answer your questions in the hopes that we will be able to return the Shuttle soon to safe flight.

ADM. GEHMAN: Thank you very much.

Mr. Thompson.

MR. THOMPSON: Okay. My name is Bob Thompson. My principal reason for being here today, I was the Shuttle Program Manager from 1970 to 1981. That encompasses a time that we started into what we called Phase B, the very early design activities on the Shuttle; and I remained the Program Manager through the first Orbiter flight, at which time I retired and went to work in industry.

I'll be happy to answer any questions. I think certainly the subject of risk management, I think we all recognize that any vehicle that can fly to and from earth orbit is going to be a risky vehicle by definition. So you're going to have to deal with risk. I don't care how you design it. Of course, the way you determine that you want to design it really sets in the family problems you're going to have to deal with; and it's very important in the early design phase to pick the set of problems you're going to want to have to live with. I think we were extremely conscious of that when we picked the configuration that we picked, and we knew we had a lot of problems to deal with. As long as we continue to fly the Shuttle, we'll have to have problems to deal with. So I'll be happy to answer any of your questions as we go on through the morning.

MR. JEFFS: I'm George Jeffs. I've spent since the Sixties in the space business, most of it with NASA, a lot of it with the Air Force also. I was at one time the Chief Engineer of the Apollo Program, the Program Manager of the Apollo Program. I was the Apollo Program Manager and the Shuttle Program Manager at the same time for a while. I ran the space division that also had the global positioning satellites. The Rocketdyne division reported to me. The energy activities reported to me at Rockwell. I ended up running that part of Rockwell that was sold to Boeing.

I've enjoyed working on the space program with the NASA because we have thought alike. We have been after the basic cause of problems rather than Band-Aiding problems. We've left no rock unturned to try and get the right answer to these things, mutually. We may have missed a few, but they were unknown to us or we would have fixed them. All those years I have spent in the middle but between NASA and industry and making those teams work because the teams are just as important a part of making these big programs happen as the hardware itself. I find myself again in the middle here, with NASA fine people on both sides of me, a thorn amongst roses; but at any rate, I will try and also answer any of the questions you might have that we may recall the answers to. We're all very proud of the hardware and its performance. Some of the best memories that I have are the astronauts telling us, after flights, what beautiful hardware it was to operate. Thank you.

ADM. GEHMAN: Thank you, sir.

MR. MORRIS: My name is Owen Morris. I was with NASA throughout the Apollo Program and worked on the Space Shuttle from 1972 to 1980. Initially I worked with Aaron as his assistant Orbiter manager, and then later I was in charge of systems integration at the Level 2 of the program. I worked with Bob Thompson there from late 1972 to 1980, retired in 1980, and then formed a company of my own for the next 15 years, working on conceptual design. I'm very happy to be here and look forward to answering your questions.

ADM. GEHMAN: Thank you, sir.

DR. SILVEIRA: Hi. I'm Milton Silveira. I first became involved with the Shuttle in March of '69, before we landed on the moon. I was involved in Phase A studies; but even prior to that, I was involved in the design of the systems, support systems on Mercury, Gemini, and Apollo. I went through the Phase B studies; and when we started into the hardware studies, I moved from running a Shuttle office in engineering and development over to become Aaron's deputy as Orbiter Project Manager.

I was involved with the Shuttle up until about '80, when I moved to headquarters to become NASA Chief Engineer. I retired from NASA in '87, after 36 years with NASA.

I currently serve as a technical adviser to Lieutenant General Ron Kadish in the Missile Defense Agency. I'm glad to be here and hope we can help you.

ADM. GEHMAN: Thank you very much. Did you all get to make any opening statements that you would like to make before? Okay. That's fine.

Okay. What we'll do is start a round of questioning here and I'll go first and then I'll open to any one of my panel members.

I'll address my question -- and all of us will follow this procedure. We'll address our question to somebody, but I hope that any of you who wants to piggyback on the reply or elaborate or anything will please feel free. We would love to have two or three answers to the same question because you all approach this thing from slightly different angles. Some of you were more intermittently involved with systems and some of you were more Project Manager and integration related. So I'll start the first question.

Mr. Thompson -- and others, too -- I notice that in addition to being involved in the STS system in the Seventies, which was in the program design definition phase, that you had previous experience in Gemini and Apollo also. Could you in any way contrast the engineering development, the Project Managership, the rules under which you operated of those two systems? Is it possible to draw for us any differences or similarities between those two systems? And then I would invite anybody else that would like to comment on that.

MR. THOMPSON: Well, I would give you a broad, general, off-the-top answer. I think the processes and procedures and the management approaches and techniques were better in Shuttle than they were in either of the two programs previously, mainly because we in government and we in industry had matured a good deal by working through those programs. For example, all through Mercury, Gemini, Apollo, Skylab, we kept a "Lessons Learned" document. 8086 or something. I can't remember the number. I think it was the 8086 document, and we made the 8086 document an applicable document on the Shuttle Program.

Let me pick a specific example. We lost a main propulsion test article during the Shuttle development period because we used the wrong weld wire in a critical weld joint. That wrong weld wire came about because the vendor had mixed two metals on the weld wire reel. We had learned in an earlier program that, in any critical welds, you ought to test the weld wire you're actually using before you make the critical weld. We missed that early in the Shuttle Program. We came back and corrected it, but that lesson learned came out of the previous programs and fed on into the later programs.

So that's just one of many, many, many examples I could cite and I think, frankly, both the government management team and the contractor management team was more experienced and probably was able to take on the Shuttle design and development job and in many respects the Shuttle design and development job was considerably more difficult than Mercury and Gemini and probably more difficult than any single element of the Apollo Program. So I think I would say that we were better prepared to manage and develop a critical risk program in Shuttle than we were previously.

MR. COHEN: I'd like to add my comment. It's almost the same as Bob's but maybe a little different emphasis. I was on the Apollo Program. I wound up being the manager of the command and service module on Apollo. The heritage we had from Apollo was a very strong subsystem manager concept, both at the government and at the contractor. It turned out to be a very, very good system. Our subsystem managers, in all honesty, were not peak ticketed, so to speak, to the program office. They actually worked for the head of the engineering directorate, which was Max Faget at the time, but the subsystem managers essentially did do their daily work for the project office and there was a very good check and balance. They had a very good relationship with their counterparts at Rockwell or at Grumman or in the Apollo Program, but in the Shuttle Program at Rockwell.

There was just a very good check and balance in the system. I felt very comfortable with that because if there was a disagreement, the subsystem manager could always go to Max and Max could then go to Chris, who was the Center Director, or Bob, and we could resolve the issue. So I felt that that was a heritage from the Apollo Program that made it very good.

MR. THOMPSON: While we're on this subject, let me make another point that I would like to call to the Board's attention. At the time we were moving into Phase B on the Space Shuttle Program, we still had not decided what configuration to build. So the Phase B management was still led out of Washington with almost identical management roles at Johnson Space Center and the Marshall Space Center because it had not developed exactly what vehicle we were going to build. Once we got to the end of Phase B and it became apparent the vehicle we were going to build, we went into a somewhat new management structure for NASA, which set up a Program Manager at what we called Level 2.

If you aren't aware of it you need to understand what Level 1 was in Shuttle, what Level 2 was, and what Level 3 was. The agency, NASA, and within the manned space flight, decided to set up a Level 2 Program Manager having agency-wide responsibility for the design, development of the vehicle but to locate that individual institutionally at the Johnson Space Center so that he could take advantage of all the institutional resources. But he did not have any program per se responsibility to Center Director. He had, of course, a desire to keep the Center Director informed, but he did not responsibly report to the Center Director. He reported directly to Level 1 in Washington; but in working in Houston, then you had to work across two other centers to work the other project elements.

In addition to the subsystem managers that were set up within the project elements, one of the key things that I feel that we set up to manage across the Program were what I call ten key technical panels. We picked a key NASA individual to chair those panels, and we made those ten key technical panels all report into Owen Morris' office that was part of my Level 2 program office. Those key technical panels then had membership put on those panels of experts all around the country at other NASA centers, within contractors, within universities; and those technical panels worked specific technical issues that cut across the total vehicle. They reported in to Owen and then any issues came from there to my control board and I had the responsibility to sign off or approve or implement the things that came out of that integration process.

If that process has been allowed to weaken, I would be very concerned because that's the heart and soul of working issues across the vehicle of a technical nature. For example, if insulation is coming off the Tank, the Tank Project Manager cannot approve that. He cannot allow that to happen. That violates a systems-level spec. He has to come to the Program Manager at Level 2 and ask the Program Manager to approve a bunch of insulation coming off the Tank. If the system isn't working that way and if the Problem Report and Corrective Action procedure is not working and if the program is not bringing the collective intelligence to deal with those kind of problems that you do if you work through the system properly, then you've got a problem in the program and you need to fix it.

ADM. GEHMAN: Let me follow up on that. I don't want to hog the microphone here. So I'll let my panel get a word in here edgewise. For me to understand the chain of command, did any of you work for the Chief Engineer at JSC?

DR. SILVEIRA: For the Chief Engineer at JSC? In reality, although he did not have that title, Max Faget, who ran engineering and development, was basically our chef engineer; and, yes, I was on his staff during the Apollo Program.

ADM. GEHMAN: During the Apollo Program. What about the STS?

DR. SILVEIRA: During the Shuttle Program, we started out that same way, yes, sir, until I became Aaron's deputy. Yes, sir.

ADM. GEHMAN: To get to Mr. Thompson's point then, as I understand this -- and I'm beyond my level of expertise here. If you were trying to resolve an engineering program -- of course, that's all you did for ten years was resolve engineering problems -- but the engineering section or the engineering division, would you describe for me the checks and balances between a fix, an engineering solution that Mr. Faget had responsibility for, versus either the Shuttle Integration Office or the Shuttle Program Manager?

DR. SILVEIRA: Well, probably our biggest disputes were always between operations and engineering as to what operations wanted and what engineering was capable of doing. I think, in general, the thing is, you know, we as a team had been working all through the Apollo Program together and I think as a team we realized that we were all friends, we knew each other, we knew who to go to, and we knew how to resolve any issues we had. And we usually, you know, came to a compatible solution as a result, without having to be dictated to as far as what approach we ought to use.

ADM. GEHMAN: The point I'm trying to get at -- and thank you for that answer. The point I'm trying to get at is: Would it be incorrect for me to characterize Mr. Max Faget's role as being essentially an equal to the Program Manager?

DR. SILVEIRA: Yes, sir.

ADM. GEHMAN: That is correct.

MR. THOMPSON: I don't understand why you would use the word "equal." No, Max Faget could not make a within-the-program decision.

ADM. GEHMAN: I understand that.

MR. THOMPSON: He could come to me and make his wishes known. He could come to my control board and argue until we got to midnight, pro and con. If he did not like what I did, he could go to the Center Director, who could go to my boss in Washington and straighten me out; but when it came time to decide who made the decision, there was no doubt who made the decision and who was responsible for it.

DR. SILVEIRA: But there were few decisions that went that far.

MR. JEFFS: You need to put this in the right perspective, too. The majority of people worked for the contractor. We had 40,000 people on Apollo. We worked for these guys, but those guys worked for us. On Shuttle we had up to 20,000 people. So you've got a whole engineering structure, both in the contractors' level and the different contractors with the subcontractors. So those technical issues were being massaged with great care, and they were being interfaced with the NASA so that we had a team working. But the drawings came out of the contractor. The detailed decisions on how to do things on change control within the contract were done with the contractor. So you've got to look at both these things together to see who's making the decisions and how they're made.

MR. THOMPSON: And you have to really be a little more specific. Ask us any detail you want and we can tell you how that would be managed and handled. For example, if it was a stress-level issue down in designing what an allowable stress somewhere internal to a wing, you'd have to go deep into the contractor organization and check that work to really find out whether it was pro or con. And the subsystem managers in the government actually checked that work, not number by number, but looked at the procedures used, looked at the decisions made, looked at the allowables and the materials and this sort of thing. But now if you ask who's responsible for not having an abort system on the vehicle, you have to ask me that question. You cannot ask George Jeff or you cannot ask Milt Silveira that question.

MR. JEFFS: But if you would ask who, why it didn't work, then you can ask George Jeffs. (Laughter)

MR. THOMPSON: Well, if it didn't work, it's a combination of the government and the contractors.

MR. MORRIS: Yeah, I think, getting back to how decisions were made, we probably ought to talk about the Change Board that Bob Thompson chaired. That board was made up of all of the element managers. The Orbiter was Aaron Cohen. The Tank, the Boosters, the Engine. Reliability. Max Faget sat in on that board. He was a bona fide member of the Board. Operations was a member of the Board. And there was no significant decision made that that board did not understand. Now, as one of the Program Managers in Apollo once said, you know, "The Board is here and this is a democratic organization but I have 51 percent of the vote."

MR. THOMPSON: But there was never a significant decision made in the Shuttle Program that Max Faget didn't have plenty of opportunity to sit in my board while we were discussing it, make his wishes known as many times as he wanted to, and he knew exactly why I made the decision I made. Whether I agreed with him or not, he knew why and he knew and by the next day I had signed off on the decision and written up why it was made.

MR. COHEN: Let me hitchhike on one more thing. The Orbiter also had a Change Control Board, and on that board we had Rockwell sit in on the Board, we had a contractor sit in on the Board, and we had each directorate, like Gene Krantz from Flight Operations, George Eddie from Flight Crew, Max, and R&QA and so forth. So we also had a board. Now, if it went outside our envelope boundary, then we would take it to Level 2; but if it was inside, then we make the decision.

MR. THOMPSON: And you can say the same thing for the other project elements -- the Tank or the Engine or the SRBs.

MR. JEFFS: As Bob says, the other elements, whether it's the SSMEs or the Orbiters, these are engineering focus operations. The engineering is the head of the snake. So engineering had a key voice in almost every decision that was made down the line on these programs. And a free voice.

DR. SILVEIRA: And I think, importantly, the heritage of the organization, most of us came out of the Langley Research Center and we moved to the Manned Spacecraft Center when it came down to Houston. So we had a heritage of working together. We knew each other, and we respected each other. Once we arrived at a decision, everybody supported it. There was no hassling afterwards. We were sort of really, in looking at a lot of organizations today, we were sort of unique in that regard, in being able to work together and make decisions together.

MR. THOMPSON: You never strive for 100 percent agreement. If you get 100 percent agreement, there's something wrong.

ADM. GEHMAN: Right, you're missing something.

MR. JEFFS: I'd like to add one more thing I mentioned earlier, and that is the issue of organization and developing organizations. I was fortunate to have, with the Apollo Program, a source of great depth of capability of people, experienced people. They came from the aircraft areas. They came from P-51s. They came from SMJs. They came from across the Board on how to build aircraft. A great base.

That base was trimmed and kind of honed during the Apollo Program. That same base fortunately was maintained on the Shuttle Program. Trimmed and maintained. So we had not only the same kind of people but the same people, the same procedures had been smoothed. The knowledge of what each element could do and couldn't do within the organization and between ourselves and NASA was understood. That doesn't exist to the same extent, as I see it, in these different companies today, probably because a lot of those people are gone and you can't put everything in the database. You've got to have with the people. So there you go.

MR. THOMPSON: George just read part of his proposal for the contract.

DR. LOGSDON: I want to go back to the period of '69 through January of '72. At the policy level, the decision whether to approve the Shuttle was being debated; and you folks at the engineering and management level were getting, I think, changing signals of what kind of Shuttle was going to be politically acceptable. I guess the question is, Bob, you said you started as Shuttle Program Manager in '70 and, Milt, you said you were involved in the Phase A studies. Phase A studies produced a particular concept, a fully-reusable straight-wing Shuttle. So first question: Did that first design have the large payload bay, the 15 by 65 payload bay?

MR. THOMPSON: The answer to that is yes; and the answer to what came out of Phase A, what came out of Phase A, those of us that were given the responsibility to go implement the Program felt that that was a very dumb way to go about it. The two-stage fully-reusable system, as we looked at it in detail about going to build it, a lot of people argued that politics made us change it; that is absolutely not correct. We changed from that vehicle because we found, as we dug into it, that was not a very smart way to go about the job, for many, many reasons. I could spend half a day here explaining it all to you, but the concept that politically we wanted to build a two-stage fully-reusable vehicle but couldn't afford it, that is not correct. The vehicle we built is the vehicle that the NASA people that came into the program starting in Phase B that had the responsibility for building it, we built the vehicle that we wanted to build, not the one that the politicians told us we had to build.

DR. LOGSDON: Fair enough. In 1970, a new set of requirements, I believe, appeared in terms of what was required to get Department of Defense support for the Program -- with additional cross-range, I guess, being the most important of those new requirements. Tell me if I'm wrong, that that had a link to shift from a straight-wing to a delta-wing configuration.

MR. THOMPSON: You want me to answer that?

DR. SILVEIRA: Let me make some comments on that, John.

Of course, you know, a few of us got cleared on what the Air Force programs were; and once we understood what the Air Force requirements were, then we understood how that affected the design and changed over to meet those requirements.

MR. THOMPSON: I'm not sure I would agree with that. I think the myth that the straight-wing two-stage fully-reusable Orbiter was a good system to build is strictly a myth. You don't want any wing on the Orbiter while you launch it, and the only benefit of the straight wing is in the terminal approach and landing phase. The fact that what Max was proposing was to hold that straight-wing vehicle up above the stall level all the way down to 10,000 feet above the runway, then whip it over and land it on the runway and to carry those straight wings all the way to orbit and back, and to have a fly-back booster, that whole system crumbled when you began to look at it.

NASA did not put cross-range in the vehicle because the Air Force forced us to. NASA put cross-range in the vehicle because we thought that was the right way to build the vehicle and it just happened to give the Air Force some capability they wanted. But we wanted it for abort capability during the launch and we wanted to start flying the vehicle right at entry. We didn't want to keep the thing above stall all the way down to landing area and then flip it around. So the myth that the Air Force made us do something we didn't want to do is absolutely a myth.

DR. LOGSDON: So the implications of that design for thermal protection came along with the NASA engineering decisions.

MR. THOMPSON: We got the same thermal protection the way we fly the Shuttle that we were going to get with the straight wing. The straight wing was not any benefit thermally at all.

I guess it's awfully interesting to me, look back over 20, 25 years, the myths that have grown up and where they have come from. But I'll go on the record today saying NASA built exactly the vehicle it wanted to build.

DR. LOGSDON: I guess the final thing I'd like to talk about a little bit is the cost estimates for development and operation that were provided, again, to the political level of decision-making. OMB gave you a budget ceiling, I believe, in May of '71 that said you had to build the system with a five billion-dollar development cost; and the ultimate presentation, at least to the White House level, said you could do that, or 5.5 billion, with an operating cost of $118 a pound. I'm curious where those numbers came from, particularly the operating cost.

MR. THOMPSON: Well, I'm not going to answer just the operating cost; I'm going to answer the whole question.

DR. LOGSDON: Good.

MR. THOMPSON: Again, one of the big myths on the Shuttle is that it was way over budget. That's an absolute myth. In December of '71, when Jim Fletcher and George Low went to San Clemente to present the final recommendation to President Nixon, we prepared a letter that George and Jim took with them, a one-page letter. That letter said that we felt we could build the configuration that you now know as the Shuttle for a total cost of $5.15 billion in the purchasing power of the 1971 dollar but that it would take another billion dollars of contingency funding over and above that to handle the contingencies that always develop in a program like this. So you need to budget 6.15 billion in the purchasing power of the '71 dollar and that we could build it and fly it by 1979 if everything went perfectly, but the $1 billion and 18 months ought to be planned in the program because that's probably what will really happen and we'll probably fly it in early '81. That was in the document.

Jim Fletcher and George Low went to San Clemente, had a little model of the Shuttle. President Nixon approved it. He came back into the agency at NASA. Bill Lilly, who was the Comptroller of the agency at that time, took that letter and started his negotiations with OMB. When he finally got around to getting it through the OMB cycle, they took the letter and said we'll take the 5.15 billion, but we won't give you the one billion because we never budget contingencies. We'll hold you to the 1979 launch date because we never launch budget contingencies there, and we'll put it in the '73 budget at those numbers.

So we lost two years of inflation in that little maneuver in OMB. I went back and talked to Bill Lilly. He said, "Shut up. You got your program. Go on about your business." So we did. During those years of the Shuttle development, inflation got as high as, what, 20 percent, 18 to 20 percent some years. We would usually get maybe two thirds of that out of the Congress. Also, the Shuttle was picked as a program to be monitored by OMB and they actually put five or six people out of the OMB into my office level here at the Johnson Space Center and they monitored for several or probably two years exactly where all the spending was to try to keep an accountability in the Program.

One of the fellows who worked for me in the financial area, named Hum Mandell, kept a very accurate level of the spending in the Shuttle Program. When we finished the program, his record showed that the Orbiter actually under-ran our original budget, including the one billion dollar contingency and the 18-month schedule. Our schedule was right on. The other elements of the program were slightly over. The total cost of the program, when you account for inflation, account for the under-commitment of the '71 to '73, you account for the deliberate schedule that OMB asked to us do with their funding. He came to me after the first flight and says, "Here. We can prove you met your cost and schedule goals." I called John Yardley in Washington and John says, "Hell, why don't you put it in a filing cabinet. No one's interested in that." So we put it in a filing cabinet. Hum took it and got a Ph.D. thesis on it at the University of Colorado. So you can get his thesis and read it if you're really interested in the true funding.

Now, one more thing. I remember being called on television at the time, not knowing that Jules Bergman was going to be on. After they introduced me, Jules Bergman says, "Hey, Mr. Thompson, you said you could build this thing for $5 billion. You've already spent 8.5 billion. That's a terrible overrun. What the hell you going to do about it?" Inflation doesn't mean a thing to the people who write in the papers, and it's a pretty complex job to keep up with the true cost of a development program like the Shuttle. In fact, after three years, OMB quit and went home. So the myth that the Shuttle was way over budget is another myth.

DR. LOGSDON: Bob, you didn't answer the question about operating costs.

MR. THOMPSON: All right. Operating costs. (Laughter) I had a better answer for development costs.

At the time we were selling the program at the start of Phase B, the people in Washington, Charlie Donlan, some of them got a company called Mathematica to come in and do an analysis of operating costs. Mathematica sat down and attempted to do some work on operating costs, and they discovered something. They discovered the more you flew, the cheaper it got per flight. (Laughter) Fabulous.

So they added as many flights as they could. They got up to, what, 40 to 50 flights a year. Hell, anyone reasonably knew you weren't going to fly 50 times a year. The most capability we ever put in the program is when we built the facilities for the Tank at Michoud, we left growth capability to where you could get up to 24 flights a year by producing Tanks, if you really wanted to get that high. We never thought you'd ever get above 10 or 12 flights a year. So when you want to say could you fly it for X million dollars, some of the charts of the document I sent you last night look ridiculous in today's world. Go back 30 years to purchasing power of the '71 dollar and those costs per flight were not the cost of ownership, they were only the costs between vehicle design that were critical to the design, because that's what we were trying to make a decision on. If they didn't matter -- you have to have a control center over here whether you've got a two-stage fully-reusable vehicle or a stage-and-a-half vehicle. So we didn't try to throw the cost of ownership into that. It would have made it look much bigger. So that's where those very low cost-per-flight numbers came from. They were never real.

Let me make one other comment. In my judgment -- and no one can either agree with this or disapprove it -- in my judgment, it would have cost more per flight to operate the two-stage fully-reusable system than the one we built, even though the cost analysis didn't show that. When you get two complex vehicles like that and all one vehicle does is help you get up to staging velocity -- and the staging velocity is 12,000 feet per second -- when you build a booster that does nothing but fly up to 12,000 feet per second, you've built something wrong. I think that's what the two-stage fully-reusable system was; and I think, had the agency tried to build it, we wouldn't have a Shuttle Program today. My feelings.

ADM. TURCOTTE: You've largely described what could be in today's, I guess, modern management vernacular as a matrix organization as it existed back in the Sixties and Seventies, et cetera. You also described some complex relationships between both contractors and the different Center Directors and the Program Manager, element managers, subsystem managers, et cetera.

MR. THOMPSON: There were no complications on the program management channels. They were very clear.

ADM. TURCOTTE: Okay. Could you explain the difference, as you see the organization today, in its relationships, its matrix structure today, and compare and contrast it to the Sixties, Seventies, and up to, say, the middle Eighties.

MR. THOMPSON: I could not, because I'm not in detail familiar with what they're doing today.

MR. COHEN: I don't think I can either. I knew that question was going to be asked, but I really don't know enough about what they're doing today. I understand the system very well. You described it as a matrixed system. It was. It may appear to be complicated, but it was really very well defined. I mean, the people, when they came to work every day, they knew what they had to do; and both at the contractor and at NASA, they knew what they had to do and they knew what their role was.

MR. THOMPSON: I want to try and make another comment. A lot of the people at NASA had come from working in a research center back at Langley, through Mercury, Gemini, Apollo, Skylab; and when we got to Shuttle and set up the matrix organization for Shuttle, it was clear to me then and it's clear to me now that the primary responsibility for integrating that program was the government's responsibility. So when we wrote the RFP for the contract that Rockwell ultimately won, we asked for them to build us an Orbiter and to provide major systems engineering support. We did not say you're responsible for systems engineering across the Program and we didn't say you're responsible for integrating the program, because they had no contract leverage over any other part of the program. They had no responsibility for the Tank or the Booster Rocket and so forth, no direct responsibility. So it was the government's responsibility to integrate the program.

Now, we used all of the hardware development contractors in a very heavy support role. A lot of the ICDs were actually prepared on assignment by Rockwell in Downey, but those ICDs came into Owen's office for review. They went across the total program for review and came to me for signature, and I had the full control of those ICDs.

Aaron couldn't change anything that impacted the Tank. The Tank couldn't change anything that impacted the Orbiter without coming back to me at the systems level. So it was no doubt but what the government had the program management and the programs systems engineering integration responsibility, but we plugged the contractors in in a way to use their talent as effectively as we could.

GEN. BARRY: I've really got two questions, if I may. One has to do with history, and one has to do with design. On the history element, could you please give us maybe a characterization of what I'm going to say here -- and correct me if I'm wrong in any of it. It has to do with compromises.

Now, after, of course, when Apollo was coming to the end and Jim Fletcher was Administrator, there were plans, originally, to put stations on the moon. Then that was backed off by the administration and there was a space station design with a Shuttle. Then that was given up in place of the Space Shuttle as we know it today, which was a bit of a compromise to try to put a space station capability payload to orbit, get down to hopefully $1,000 per pound eventually at some future point, depending on how many times you flew per year. The historical question I'd like to ask is: What compromises were made on the structure development on the Shuttle in that time period? Then I'll ask my design question here.

MR. THOMPSON: I hate to keep hogging the thing here, but you're asking history and I guess I'm the oldest person here. To answer your question, I've got to take you to 1968 or '69 -- I can't remember which year -- and the Space Council. Do you know what the Space Council is?

GEN. BARRY: The Vice President.

MR. THOMPSON: In 1969, driven by the fact that the government works on five-year budget plans, it was then incumbent on NASA to put some dollars into the out years for where they wanted to go post Apollo. So the nation then came to a fork in the road or what are you going to do with manned space flight, in 1969, because you could see the end of the Apollo Program. We had already decided what to do with the residual hardware in what became known as the Skylab Program. If something wasn't done, we were going to go out of the manned space flight business. That simple.

So the Vice President at the time, Spiro Agnew -- and this thing never really got advertised very much maybe because of that -- in any event, he chaired the Space Council and they worked for about six months and they looked at where this nation should go post Apollo, so-called post-Apollo planning. I'm sure those are in the records and you can go back and get them.

That Space Council looked finally at four major options. They looked at a manned Mars expedition, they looked at a follow-on lunar program, they looked at a low earth orbital infrastructure program, and they looked at getting out of the business. They looked at those four things.

They made the decision to have a low earth orbital infrastructure program. It wasn't we'll build a Shuttle or we'll build a space station, you know. We will have a low earth orbital infrastructure program. It never got announced like Kennedy announced the Lunar Program, but that decision was made by the President on the advice of the Space Council.

Now, up until that time there had been a lot of debate in this country about whether space station should be a great, big, artificial-gravity rotating wheel launched on Nova-class boosters or whether it was to be a zero-G station built on orbit in modular form with something like the Space Shuttle. The desire for a zero-gravity, modular space station prevailed at that time. It was a commonsense, logical thing to do; but before you can go that way, you obviously have to have something called a Space Shuttle. You have to have a truck and a personnel carrier and a work machine to go up there and do that work.

Also, at the time the President was giving the head of NASA instructions to come down off the 3.5 percent spending that we had peaked at in Apollo, down to about one percent spending for the agency. As Jim Fletcher looked under his one percent spending -- with Apollo ongoing, with Skylab ongoing -- he felt that he couldn't have but one billion dollar annual funding expended on low earth orbital infrastructure development.

We then undertook obviously to build the Shuttle first and then the modular, zero-gravity space station second; and the low earth orbital infrastructure gave the nation a capability to operate from the surface of the earth up to 600 nautical miles, operating Shuttles and space stations and interim upper stages that would take payloads from that low earth orbital up to geosynchronous orbit. As the thing evolved, we started with the Shuttle; and the requirements for the Shuttle were driven 99 percent by what we wanted to do to support the space station. It also happened to give the Air Force the kind of payload volume and the kind of capability they wanted, although they really wanted to be at higher orbits for their work.

So the Air Force came in and said we will plan to use the Shuttle and we will also take on the task of building the interim upper stage, which was part of the low earth orbital infrastructure. So NASA embarked on the Shuttle. It wasn't necessary to commit to a space station at that time because the Shuttle had to be built and operational before you commit to space station, and the President at that time, Nixon, had other things on his mind. He didn't get up and make a great, big speech about low earth orbital infrastructure.

So now a lot of myths have grown up about we stumbled between space station and the Orbiter and we wanted to do an Orbiter this way and an Orbiter that way. That's not the way it happened at all. It was pretty orderly planning. It was a decision to go to the low earth orbital infrastructure. Let's have a Shuttle, then let's have a modular zero-gravity space station.

Once the *Challenger* accident occurred, the Air Force got off of the ship and stuck with their original vehicles, which I think was probably the right decision for them all along because the nature of their missions don't fit the Shuttle quite that well but they could have done some of their work. But they actually developed the interim upper stage and they built a bunch of launch facilities at the West Coast that we ultimately phased out.

GEN. BARRY: Let me ask the following question based on a historical perspective. Can you give us an understanding of the design specifications for the Orbiter to take debris hits? When you finally settled on the design after going through these ramifications of alternatives and finally settled on, as we know, the Space Shuttle system to be today, our question from the Board repeatedly is: Was the Space Shuttle designed to accept debris hits from foam, either at the RCC or at the belly with the tiles?

MR. THOMPSON: The answer to that is no. The spec for the Tank is that nothing would come off the Tank forward of the 2058 ring frame and it was never designed to withstand a three pound mass hitting at 700 feet per second. That was never considered to be a design requirement.

MR. COHEN: You've got to recognize, when we first started flights, we were concerned about ice coming off the Tank. That really was our big concern, was ice going to come off the Tank, because we knew ice would do very serious damage.

MR. THOMPSON: But usually ice under insulation was our principal concern where you would get a crack in the insulation, you had cryo-pumping under there, you'd get ice formed up under it, and a chunk of ice and insulation come off. We must have had -- Owen, you can estimate – 15, we had so many meetings on trying to make sure we didn't have ice, we called them the ice follies meetings.

MR. COHEN: And we still have an ice team today that goes out and inspects the vehicle before every flight.

MR. THOMPSON: I don't know what they're doing today. It was my understanding -- and you can correct me, Owen -- I was pretty sure we did ultrasonic testing on the Tank foam insulation, looking for any voids. We carefully did visual inspection. We put together a very comprehensive ice team that walked up and down the vehicle just before liftoff. We put the beanie cap on top of the Tank to capture the cold exhaust gas to make sure no frost or ice built up there. We even talked one time about building a great, big damn building around the whole thing and environmentally control it, but we decided that really probably wasn't necessary.

We paid an awful lot of attention to making sure nothing came off, because we knew if we fractured the carbon-carbon on the leading edge of the Orbiter, it was a lost day. We could take a fair amount of damage on the silica tiles and still be all right, but it was a maintenance problem. So we worked very hard to make sure we did not have any foreign object debris.

DR. SILVEIRA: You have to understand the exterior of the vehicle of the Orbiter is glass. I mean, the coating on the tile is a silicate glass, and you have to treat it like that. So, yeah, impacts are not allowed.

MR. JEFFS: Let me hitchhike on that briefly, too. That is that it's kind of incongruous, when you look at the overall picture, the RCC panels are -- the bottom line, for example -- the rear of the panels is not completely true. There's a little waviness in it which is just due to the way it comes off the tool and spring-back and so on; but when the tiles are matched to it, the tiles are delicately matched to mix those interfaces all the way along. With a graphite epoxy, the coefficients of expansion are such that you can maintain those shapes just right. Then we stand back and think, gee, there we go to great pains to kind of hand-tailor all of this stuff and then all of a sudden we're hitting it with debris. It just is two different worlds.

MR. THOMPSON: Well, let me comment. The silica tiles that are on the Orbiter behind the carbon-carbon, in the damage testing and the testing we did on that during the program, in most cases the type of damage you would expect to get on those is not the kind of damage that kills you. Most of the time when you hit those tiles hard with something, they were fragile enough that you knocked the outer layer off but the inner layer where it's been densified against the two glue joints and the strain isolation plate, just a portion of the silica, the two glue joints and the strain isolation plate gives you enough thermal protection to make an entry. So people have gotten locked up on the fragile nature of the silica tiles. The silica tiles are fragile to damage, but they're actually pretty forgiving. You can take a lot of damage right there. You cannot take any damage that knocks a hole in the carbon-carbon leading edges.

MR. JEFFS: Well, let me add one thing to that. That is that they're a robust system from what they're designed to do, and that's to take the heat loads. They are a little delicate here and there when it comes to like the coatings because the coatings are part of the radiating heat transfer. So the coatings are meant to be there, and it's also pretty critical on the front edges of that system so that you don't trip the boundary layer. You certainly don't want to trip the boundary layer on the front end of that thing.

So, as Bob says, those tiles along the interface to the RCCs are also densified. So they're a higher density than the tiles further aft. So they're stronger. You do that, taking with it the higher thermal conductivity through the thing, and still maintain the bond line temperatures. So they are more rugged and they will, as he says, give you assurance you're going to get through even if you have some missing, but you don't want to do that and you don't want to nick them on that front end.

MR. COHEN: We were concerned early in the program whether you could damage a tile and that tile damage at the bond line and that the heating then would cause what we call an unzippering effect where you actually damage the bond line and a lot of tiles would come off. That would be the case we were concerned about. But as Bob said, the tile is actually pretty forgiving with reasonable types of hits. But you can't take large hits that really cause you damage that would destroy the boundary layer.

MR. THOMPSON: Let me take you back on this and tell one story. We were doing some thermal testing of the silica tiles in a thermal wind tunnel out at Ames. We heated the air stream with some carbon heating elements. And there was a test panel with several silica tiles put on it that would be put downstream and then you would hit it with this heat pulse in the aerodynamic wind tunnel there. We ran the tests on the silica tiles. Lockheed, which was the subsystem manager for the silica tiles, ran these tests out at Ames, and the heating elements, the copper heating elements in the tunnel failed and they put a whole bunch of carbon shotgun-like particles in the air-stream. They actually blew off probably 70 percent of the silica tiles, just like you would shoot it with a shotgun. They brought that to my office to show me what happened on that. I said, "Well, okay, that's fine but what happened to the temperature of the aluminum behind it for the re-entry heating pulse?"

They said, "Well, instead of 200 that we were looking for, it got up to 3 or 4 hundred degrees, but it didn't structurally fail."

I said, "Hell, that's the best test I've seen in a long time."

MR. JEFFS: Just a couple of notes on it. When you look at that wing after flight, it's fascinating to see where the transitions occur. You can see from the heating patterns under the bottom wing. You can see how far back that transition is. So you're laminar a long way back, which is very reassuring. Even if you had a nick along the front edge locally, it doesn't necessarily transition the boundary layer throughout the total wing. It could be just in the local air of the wing, and it would be probably be survivable. So we weren't really concerned with the zipper effect. Fletcher was really worried about that, but we didn't think that would occur.

MR. THOMPSON: Well, you don't want to leave the impression that if you trip the boundary layer, you would lose the vehicle.

MR. JEFFS: No, but I didn't say that. I said you could locally trip it and you could have higher heat transfer coefficients in that region but you're not going to necessarily lose the wing in those circumstances.

MR. COHEN: Let me ask you a question. You may be more familiar. Have you gone back and looked at Volume 10 now? Do they have a requirement in there for the size of debris?

GEN. BARRY: Volume 10.

MR. COHEN: Volume 10 would be the design specification --

DR. SILVEIRA: That's a Level 2.

MR. COHEN: Do they have a criterion in there?

GEN. BARRY: They do have a criterion, and it's like .006 foot pounds per hit. It's very, very small. It's almost minuscule to the point where it can't take hits, just like Dr. Silveira mentioned. So that's the puzzling aspect because, in reality, as you trace the hits on the Orbiter from the very beginning, from the very first mission, they've averaged, you know, as high as 700 on STS-27 to 300 on STS-87 and almost every Orbiter has averaged about 50 to 100 hits. So it's interesting to see that the design specification really was not to allow for any hits, although the reality has been it's been pretty durable for most of that; but the design specification is contrary to the reality.

MR. JEFFS: Weren't the majority of those coming off the runway?

DR. WIDNALL: What runway?

MR. JEFFS: Landing the thing. You get a lot on the runway. That runway is coarse.

MR. THOMPSON: Here again, Aaron was talking about a document that was called the 07700 series of documents. Those are the Level 2 documents that I controlled to put the specs across the program. Volume 10 was one of those specs, and that was where the 2058 ring frame came from. In any practical problem, it would be nice to meet all of your specs. In the real world, though, you know, I will sit here and let you shoot at me with a pop gun that's got a little cork in it that won't come halfway over here all you want to; but if you pick up a .45 and shoot at me, I'm going to get the hell out of here. So you've got to have some judgment when you're operating a vehicle of this nature of what you're willing to live with and what you aren't willing to live with. And that's hard to write in a specific spec and it's hard to live in an ideal spec world because you run into practical problems like popcorning of insulation.

MR. JEFFS: Let me say one more thing. I might have left the wrong impression here, too. That is, you know, first off with the RCC. We were always concerned about the RCC and the loads on the RCC. We spent extra money and extra time to go to the woven cloth, for example. We didn't go to the single filament stuff to take advantage of the load direction and all this jazz. We really went overboard to make that as strong as possible.

We went through the whole litany with McDonald on the problems they were having on trying to make a graphite tail for the F-15 or F-18. I don't know which one it was. They had a lot of problems with it relative to how you weave in the middle interfacing elements of the carbon-carbon. You can't just drill holes in carbon-carbon. So you've got to weave in the interfacing metal elements in order to attach it to the air frame. So they had special techniques that they had gone to to wrap it in like you tape-wrap a swollen ankle or something like that, to really get those pieces in there right. Went through all that stuff with them. So we really had a rugged RCC. That RCC, the Q alphas are, I don't know, 900 to 1100 something like that, pounds per foot. So they're taking a pretty damn good load up in that front end. So they're not wussies, that's for sure.

MR. THOMPSON: Well, they are strong; but they're still a ceramic. What you don't do is hit a ceramic with a real sharp, high-energy, low-time blow. Anything going 700 feet per second, even if it's a soft piece of insulation, if you look at the force-time curve that we put onto that insulation, we didn't do a dead-chicken test on it. We knew well you could knock it off if you hit it with enough potential energy, or kinetic energy.

MR. JEFFS: You guys mentioned the holes have been mentioned on the RCC. When I looked at the first flight back, up at Edwards, I was looking at boundary layer transitions pattern and stuff. I noticed on the underside of the wing that I could see occasionally a few holes. They looked almost like a circular hole. Completely circular. Almost like a hole that would be popped out of your porridge when a steam bubble come up out of a porridge, you know. I couldn't figure what those things were. I thought maybe we might have trapped water in the zip or something and we had gotten over the boiling temperature of water, which is like 160 or something like that at the altitude, and that we were building ourselves a little steam engine there and that might be accounting for the tiles occasionally popping off, which we couldn't figure out why they would occasionally come off. But we ran some tests and they ran some tests lately at Langley and they haven't verified that that's any condition at all. I noticed you said there some round holes on the RCC, or somebody was saying that there were some holes. We just don't know what the nature of those holes are. We had never seen those before. We didn't see any of those at testing.

GEN. HESS: One of the issues that's often discussed in the back rooms of the Board is this thing about whether or not the Shuttle is an operational vehicle. We wonder if y'all could share your opinions on that versus being an R&D vehicle.

MR. JEFFS: I've got a lot of heartburn that I can share with you on that. You know Beggs wanted to declare the Shuttle operational after about five or six flights. That was one of the reasons for the SPC. It was one of the reasons for the Shuttle processing contract being given at the Cape. Our arguments or my arguments were that we were still learning about the machine and we still had a number of things to really sweat out before we completely understood it and all the characteristics and, therefore, the development contractor should be maintained strongly in that act.

MR. THOMPSON: George, you need to ask him what an operational vehicle is. Define it. A vehicle that flies to earth orbit will never be operational in a sense a 747 is operational, if that's your definition of an operational vehicle.

MR. JEFFS: So we were as operational as we ever had a space machine, I guess, because we had flown it that many times.

MR. THOMPSON: But it will always be a risky endeavor.

MR. JEFFS: Well, we're still learning about these machines. It's a machine that doesn't have the same wear and tear as an aircraft. I mean, we're not landing it ten times a day or what have you. It does take heavy loads on launch. It takes thermal loads on re-entry. So it's different. It doesn't do much on orbit. It's pretty easy for it on orbit. But it is not a hard-driven machine from an operational point of view, and it's more like a helicopter.

MR. THOMPSON: You're still hitting it with four million pounds of thrust.

MR. JEFFS: Well, you only do it every once in a while. You only do it twice a year rather than ten times a day. I wanted to add one more thing to it, though. That is, further, it's like a helicopter, and even more so, in that when you get it to the ground, you can do anything you want to it. You can re-examine it. You can change, add to the tiles, fix the tile problems and so on. So you're rebuilding the machine between flights.

MR. COHEN: No matter what you say, the hardware, the process, whatever, needs to take -- you need to have tender, loving care of it.

MR. THOMPSON: You need a development mentality organization managing it.

MR. COHEN: It's a hostile environment you go into and return to.

MR. JEFFS: With all respect to Beggs, though, he wanted to -- the other side of that argument, the flip side obviously, is that if you're the development contractor, you're continually making changes to it. So stop making changes, guys, to make it better all the time. That's where Beggs was coming from.

MR. THOMPSON: I've heard that all my life: "Don't make changes." If it's about to break, you better change it.

MR. JEFFS: You've got to have those kind of eyes looking at it so they can see ahead of time before it's about to break.

ADM. GEHMAN: I'd like to ask Mr. Morris and Mr. Silveira if you'd comment on this, whether it's an operational or a developmental vehicle.

MR. MORRIS: Well, I would go back to Bob's question. How do you define operational? I think, in my experience, any high-performance aircraft is continually being inspected, is continually being modified. They're being updated with glass cockpits and other things that are systems upgrades. But any high-performance vehicle is continually being modified. I think the Shuttle, although I haven't been involved with it for many years now, has been modified more than most operational aircraft, things you call operational; but I don't think there's a difference in the amount of changes made. I don't think there's any difference in the philosophy of the way you manage the Program or operate the vehicle. I think a high-performance vehicle, be it in space or in the air, continues to be something you are developing and you're learning more about as you operate it.

MR. THOMPSON: I think it's also somewhat delusionary to think you can start with a new sheet of paper and build a new vehicle and it won't have any problems and it will be easy to operate and it will be cheap to operate and everything will be fine. That's always what you come out of Phase A with; but once you build it -- and particularly if it's going to sit on the surface of the earth and then accelerate to 18,000 miles an hour, stand re-entry heating, land on a runway -- you're going to have to give it a lot of attention.

MR. JEFFS: As you say in the aircraft business, it's operational on condition. It's an on-conditional airplane, but you've got to have the right eyes looking at it to know when that on-condition time occurs.

ADM. GEHMAN: Mr. Silveira, you want to comment on that?

DR. SILVEIRA: You know, like with any vehicle, you have to continue to scrutinize the results of every flight. You know, we had many thousand hours on 737s when we had to go back and modify the actuator and the rudders because it didn't really work the way we thought it did on that. I think that's the thing you have to continually do with any aircraft.

Now, as the aircraft gets more mature, of course, you can back off some on the scrutiny; but where the Shuttles have actually very, very limited amount of flight time, then you've really got to pay a lot of attention to it. You say: Are they operational? To a certain extent, yes, but you still need an awful lot of engineering scrutiny to examine what the results were of the last flight.

MR. THOMPSON: You have to also recognize that a rocket engine, you're essentially building a very hot fire in a cardboard box; and you have to do it very carefully. If you get a little bit off on your cooling paths and so forth, you burn up your box.

MR. JEFFS: We've come a long way. We didn't really know that much about the regen system with the SSMEs. As a matter of fact, we had a lot of trouble going through the gates to get the engine started. The guy I worked for at the time that ran Rockwell used to say, "How in the world are you ever going to get three engines started at the same time if you can't start one?" That was a very good question. We've come a long way and we've learned a lot about the engines. Where we found shortfalls -- or not shortfalls -- but marginal conditions and we were operating with low margins, those are things that have been worked on. Changed. Addressed. The pumps and so on.

MR. THOMPSON: And the digital controller.

MR. JEFFS: And that's the kind of whole process that should go right along with the evolution of the whole system. Someday it will be even more on-condition in total, but it will still have those things in it that we learn from the operation of a system like this in space, which is new. We don't have the aircraft background that we had.

DR. HALLOCK: You mentioned Volume 10. I've had some many sleepless nights looking at it, trying to understand what was going on, and looking at this evolution over time. You also mentioned that one of the criteria you had was that you didn't want to have any strikes, foam strikes, is the way we were talking about it at that time. But how about the ambient environment itself? I mean, things like what you might expect in that when you get up into orbit, such as space debris and micrometeorites and other types of things that could also cause damage to the craft?

MR. THOMPSON: I would comment that we did not know enough about the orbital environment to practically say what kind of impacts you should take from orbit. So, frankly, we did not spend a lot of time trying to design the Orbiter to take hits while on orbit from unidentified objects.

MR. COHEN: We did have a criteria -- and I believe I'm right -- the criteria in the Orbiter that you could have a penetration or an opening of a half an inch or so diameter and have makeup volume, makeup gas.

MR. THOMPSON: You're talking about the environmental control system.

MR. COHEN: Yeah, the environmental control system. So the crew could get their suits on and do a de-orbit. But that was not for space debris. That was just for a penetration.

MR. JEFFS: We did have the specs on particle size impingement on windows and what have you. So the windows are all designed for that.

MR. THOMPSON: For a certain particle size. But you could certainly get above that.

MR. COHEN: As Bob said, I don't recall orbital debris being discussed very much.

MR. THOMPSON: I don't think you would really know enough today to put a good spec on a system flying in earth orbit.

MR. JEFFS: We had some data from Apollo that we used.

MR. THOMPSON: It's going to have to be a judgment call for someone.

DR. HALLOCK: One of the things you hear a lot of discussions going on at this point is: Is there someway that one could make a repair on orbit? Were those kinds of issues addressed back in those times?

MR. THOMPSON: They were discussed. They were never addressed in a serious way.

MR. JEFFS: Well, we were pretty serious about trying to figure out how the heck you might replace a tile. There's a young lady in the bowels of NASA named Bonnie Dunbar -- or Donnie Bunbar or whatever they called Bonnie -- and she's a Ph.D. in ceramics. She was right in the middle of the tile operations. She worked for us a while up at Palmdale. We often discussed how in the heck if we look at the detailed process of what the guys had to go through just to get a tile on and how you would do that with gloves, you know, in an EVA situation. And it's not easy. I'll tell you, it's not easy. You know, you've got to pull-test it and you've got to do lots of things with it to verify that you've got -- you might take some shortcuts if you just had to make a repair in orbit, I suppose. I suppose it's doable, but it's very tough. Now, how you replace an RCC panel? That's something else.

MR. THOMPSON: First of all, I noticed in the paper a lot of conversation about looking at the Shuttle while on orbit. We did look at the Shuttle while on orbit for the first Shuttle flight, using the Air Force resources. It was more from a we would just like to know ahead of time whether we've got some potential problem in front of us, not because we had any ability to go inside and do very much about it.

MR. COHEN: Those things are documented. I don't recall. But the real issue is going EVA and trying to get to the various parts of the vehicle. Even if you had a kit, it's very difficult. With the space station there, it may be another thing.

MR. THOMPSON: You could do some things like that. It's a matter of whether that's a good expenditure of your resources with the probability of what you can really do that's practical.

GEN. HESS: I'm kind of curious if you would characterize for me the role of the safety organization in the structure that you had back in the Sixties and Seventies in terms of how it integrated itself with the system development.

MR. COHEN: Let me say a little bit from the Orbiter point of view on the changes. In our Change Board and my daily meetings, SR&QA had a person sit in on every one of our meetings; and I think that was the same thing at Rockwell, also, from the Orbiter point of view. Somebody was there. Again, very much as the engineer was a check and balance, SR&QA was a check and balance because in that case I believe Marty Raines was the head of SR&QA and he reported to Chris Kraft. So again, if SR&QA had an issue with what we were doing, just as engineering or operations, there was a check and balance at my level.

MR. THOMPSON: Well, I think I'd comment this way. Within the Program, there was a very active Safety, Reliability & Quality Assurance presence and activity. We did all the usual failure mode and effects analysis. We did all the development of critical items list. I signed off on probably several hundred critical items, recognizing if that item failed, we'd lose the vehicle. Safety was spread throughout. Safety, Reliability & Quality Assurance was spread throughout the entire Program.

We looked very carefully at whether we wanted to do what we called the nines business, whether we wanted to attempt to do statistical quality assurance kind of things. In looking at the spectrum across the Shuttle systems, the part of the system where the nines kind of approach made sense in avionics and things like that was a relatively small part of the overall system. So we did not go into a formal statistical qualification program where we could get nines that had some meaning to tell us which part of the system was relatively good and which part wasn't. We tried that on Apollo and gave up on it, more or less. A lot of consideration was given to what we called the formal or statistical safety and quality analysis, and we decided it was not worthwhile to try to lay that on the Program.

How you put the statistical number to an O-ring failing is pretty hard to come by; and if you have a lot of garbage in, you get a lot of garbage out. So I think you have to be very careful. If you're building television sets by the thousands and taking data on this resistor and that resistor and it tells you which resistor is causing your televisions to quit, it probably has some value; but when you look at most of the systems on the Shuttle, you cannot do the kind of numerical numbers of tests to give you, under a properly controlled condition, any kind of valid input data. And once the people get those nines, they really maneuver them, whether they have any real meaning or not.

Owen, you may want to comment on this.

MR. MORRIS: You know, if you take this and go to the structures, which is really kind of where we're interested today, we did use fracture mechanics, fracture analysis. We did have margins in the vehicle; and that's the way, again, aircraft are designed. Structure has to be qualified to the level of the margin, and then it has a reliability of one in your nines approach.

MR. JEFFS: Structure is tough, but we also have redundant load paths. So if we had one failure, we had a second path in order to take the load.

MR. THOMPSON: In some parts of the system.

MR. JEFFS: Wherever we could.

MR. THOMPSON: For example, we went to safety factor of two on the Solid Rocket Boosters. Typically the Air Force in their ballistic programs were using either 1.25 or 1.4. We went to a safety factors of two on these SRBs in the amount of insulation we put in, in the structure, design allowables, and so forth, which is relatively high for these kinds of systems; but we did it because we didn't have a backup for the SRB. If the SRB failed, you lost a system and we knew that. We didn't get there by nines; we got there by safety factors, as best we could.

MR. COHEN: Design philosophy, at least. Margin in the design, whether it be electronics or it be structures, is important. Redundancy and margin. I would say margins first and then redundancy. If the redundancy adds to the margin, then it's good. If the redundancy doesn't have margin, then it's not very good. So that's what we really looked for was margin in your design, the deterministic type of analysis rather than probabilistic analysis.

MR. JEFFS: The tiles in the design was considered for 100 missions with a factor of four. So a factor of four was on top of that 100 or so. That was considered in the design. The Orbiters were built by MCRs. The MCR is a Master Change Records. I signed every Master Change Record, and I looked for lots of things in those MCRs and one of then was safety. But we had organizations that were tuned and they came out of the Apollo Program. They were looking for the what-ifs. They were looking for failure modes and how to recover from failure modes. So therefore, in the design, how do you put something in when you don't have those failure modes? So we had a very sensitive organization to that; and that was partially schooled into them from interfacing with the Mission Control, for example, in the Apollo stuff, on how to respond and react to in-flight emergencies. So a lot of that basic background was in the fundamental design as best we could put.

MR. THOMPSON: We haven't mentioned sneak circuits. We did all the typical sneak circuit analysis work. We did all of the kinds of things we had learned to do in the previous programs to prevent the rocket going off when you hooked the battery up and that sort of thing.

MR. JEFFS: All the golden chute relays and everything.

ADM. GEHMAN: All right. We have a lot more questions and we're going to go on for at least another 90 minutes, but we're going to take about a 10 minute break here so we can all pay attention and be in comfort while we're doing this.

(Recess taken)

ADM. GEHMAN: All right. Ladies and gentlemen, we're ready to resume.

Gentlemen, thank you very much for your very forthcoming answers to our questions. We appreciate it.

Dr. Widnall, if you're ready, go ahead.

DR. WIDNALL: I'm going to ask an engineering question. Given that at that period of time that composite materials were sort of new -- in fact, not to make a pun of it, they sort of were at the leading edge -- I sort of would like to understand what kind of testing was done on the RCC panels. For example, was there a lot of fatigue testing done? Did you have in-flight unsteady pressure loads data that you could use for fatigue testing? Did you cycle the panels through a vibratory environment followed by heating and ultraviolet or whatever-else-is-up-there environment? Did you rip them apart? Did you impact them with small pellets? What kind of testing was done on the RCC? It's clearly an important issue for the design of the vehicle.

MR. JEFFS: Let me tell you what little I know, and a lot of things I don't know the details of. First off, the RCC panels, I'm sure, in the process, were subject to all the rigors of qualification of everything else on the program; and that included structural testing of all major elements. So the RCC panel was certainly a major element. The interface of the RCC panel to the wing structure itself was kind of a critical area. The whole issue of water in graphite epoxy and how it might play in the game. The whole issue of the specs re salt water, et cetera. Now, whether they vibrated the panels or not, I don't know, and I don't have the documentation to identify it, but I would be very surprised if there weren't detailed documentation of the structural testing of those panels and the load interfaces to the wing. I don't remember anything in the way of impacting those panels with high-velocity particles or something like that. I don't remember that, but the rest of it I do recall that there was some of those.

DR. WIDNALL: What about testing to destruction? I think one of the issues that we are amused by is that the RCC panels seem to have broken right along the center line of the leading edge. So were the panel destruct-tested by putting loads on them to see where, in fact, they would break?

MR. COHEN: Testing we did on the panels. On the RCC panels.

MR. JEFFS: I'm surprised that it would break in that area.

DR. WIDNALL: I know. I was surprised. I have no explanation for this.

MR. JEFFS: As I said, that cloth is woven cloth.

DR. WIDNALL: No, right along the leading edge, they broke. I have no explanation for that, but I wondered whether structural tests had been done.

DR. SILVEIRA: I don't recall.

DR. WIDNALL: I know they're very expensive panels. So obviously...

MR. JEFFS: Yeah, what we could test, we tested; and we tested to know what kind of margins we had. We tested them certainly up to yield; and whether we went to ultimate on those panels, I don't know. But I'm sure that the Boeing guys would have that in their files.

ADM. GEHMAN: Anyone else want to make a comment?

DR. SILVEIRA: Don Curry was subsystem manager on the RCC, would be familiar with what testing we did. But as I recall, we took a number of panels to destruction. I don't remember seeing a failure like that, at least in the stuff that he showed me.

MR. JEFFS: We had material we could work with. You know, there was a long process that they went through at Vaught to develop the panels because the panels were pyrolyzed, as you know, and you build them on this tool that has to go in the oven with the panel, and then we would get spring-back. So they went through a lot of steps before they got the right spring-back in those panels. So they had panels to work with; and Vaught, in general, did a very good job on those panels overall. So I'm sure that they tested those.

MR. COHEN: I'll refer to this document.

DR. WIDNALL: Thanks a lot, Aaron.

MR. COHEN: It does talk about -- this is the Space Shuttle technical conference and Don Curry --

DR. WIDNALL: I would love to get a copy of that.

MR. COHEN: It does talk about the early design challenges, the leading edge. Of course, one of the big issues was the coating, the coating and the degradation of the coating and how the panels degraded with the degradation of the coating. Now, it doesn't go into a tremendous amount of detail in here, but it does give you an overall view. This was written by Don Curry, and Don Curry is the subsystem manager. I don't have the data in front of me, but I'm almost sure we did take the panels to do some structural testing on the panels. I don't have it here but --

DR. SILVEIRA: The RCC was really a big technical challenge, as far as building the panels. You know, when we started doing it, John Yardley made a comment to me one day. He said, "If I ever hear about delamination, it's going to be your job." Well, LTV actually did, I think, a superior job in putting it together. They really did. You had to pack the panels in carbon retorched to form and the like and there were very, very few quality problems that we experienced during the development of the panels.

MR. COHEN: They did Eddy current testing and sonic testing of the panels in the manufacturing process.

MR. THOMPSON: There was never any thought, though, that those panels would withstand a 20,000 foot pound kinetic energy strike. They were not designed for that. The whole intent was to not let it happen. You could not set out and design -- I wouldn't know how to design the leading edge of that wing to take a 20,000 foot pound kinetic energy strike.

DR. SILVEIRA: Not many airplanes are designed that way.

MR. THOMPSON: I think we may have had to abandon the program, had that been a requirement.

GEN. BARRY: I'd like to address the issue of the design of the Space Shuttle itself insofar as lifespan is concerned. Right now in our readings, of course, the original design was to fly 100 times in 10 years. So that's ten times a year per Shuttle. Here we are at 2003. We know the *Columbia* was on its 28th flight, not 100, and certainly not within 10 years. So we've entered an era that the Board has pretty well identified as an era of reusable vehicles in an aging space platform in a R&D or development based environment. So let's say aging spacecraft in an R&D environment, for practical purposes. I'd like to get your perspective on how long you anticipated in the original design on how long the Shuttle would last, in light of the fact that NASA has announced now that the Shuttle will fly until 2020. Can I get a perspective on lifespan for the Space Shuttle?

MR. THOMPSON: Let me comment. Then I'd like to have some of the other people talk. We debated a lot about what kind of a number to put in the spec for that. Frankly, we could never find very much that was sensitive to that number in the kind of application we were talking about for Shuttle.

You know, 100 times would be a minor load for an airplane or airplane structure or fuselage and so forth. We put it in there to help ferret out any problems that people might come back and say, "Hey, it won't go 100 times." I don't remember anyone coming back and saying that was a constraint for anything.

I would think, with reasonable attention and oversight and proper upgrading of subsystems and replacement of subsystems as appropriate, I don't see any reason why the Shuttle couldn't last many, many years. You know we have B-52s out there flying after 30 or 40 years. We've got some T-38s out at Ellington that have got how many years on them. So that 100 number we put in there was never much of a driver to us on the Program. We didn't quite understand what we were trying to control with it in the first place very thoroughly, and it was more put in there to see if it drove anything out. And I don't ever remember anyone coming and asking for an option on the 100-cycle lifetime.

Owen, you may want to add more to this.

MR. MORRIS: I don't think, in my memory at least, that we ever really addressed any issue that said we have to have five more pounds or we have to do something to be able to reach 100 missions. I keep going back to aircraft; but, again, if you look at T-38s, yeah, they're still flying. They're flying okay. Now, they've had some wing problems. There have been cracks. The cracks are carefully monitored on a per-flight basis or every 10 flights, whatever the spec is on that, and you continue to operate. You know, I think you can do the Shuttle the same way.

MR. JEFFS: Let me say a couple of things about it. What we did on both Apollo and Shuttle, we did have age life critical item identification. So we identified all the items that we knew about in the system that were age life critical. For example, all the rings, the N204 and all those seals were on that age life list. There are all the pyros. The pyros were also bootstrapped so that you fire pyros every six years from the same lot to see that, in fact, you still had life in that pyro which could change.

I think the specs for the review of the Orbiter after every so many years, there are certain items called out to look at specifically in those; and some of those were kind of age-related in the thinking when they went into that review spec. It's kind of like the 3,000-hour turbine engine or something like that. They're in that overhaul spec requirement.

I think the rest of it, as you say, it was a development item. We didn't know everything, too, that might have some characteristics re aging. So a lot of that is as required as we go through and look at the spacecraft. Certainly, you know, I think about this oft-times at night because I own and fly helicopters a long way and what I do in those helicopters is far less than what we do on that Shuttle in the way of looking at it very carefully to see what is aging as we go through the process, particularly on the Thermal Protection System.

MR. COHEN: The real issue on extending the life would be the obsolescence of the subsystems, the replacement of parts, and the computers and this type of thing. Of course, we did upgrade the cockpit; and really obsolescence of hardware and replacement of hardware is probably one of the biggest issues, I would think.

MR. JEFFS: Let me say another thing. One thing that worried me was the screed. The screed worried me on the wing. I was worried about screed from the point of view of were we introducing something here that could, in fact, be sort of a zipper kind of effect. So I specifically went after that through the years; and the guys convinced me that there was no aging identifiable, that we had a true, solid bond in the screed on that wing. So that's one of the kinds of things you look at from an aging point of view.

ADM. GEHMAN: If I could follow up on that, some things age by how many times they've been used, like cycling an aircraft, but then there's also some things that chronologically age. Carbon-reinforced panels and things like that age by stress, but they also age chronologically. If you had an RCC panel and you left it out in the breezes of the Atlantic Ocean and you never flew it, it would deteriorate. But wiring ages and wiring insulation ages. And you mentioned seals and things like that. They obviously age. But there are a number of critical items on the Shuttle which, when you get to the 20th anniversary and you're thinking about flying it another 20 years, even if they've been properly maintained, it does occur to us that there are a number of critical systems that have to be looked at very, very carefully. Wiring comes to my mind. Wiring insulation.

MR. THOMPSON: Then again, you still have to ask yourself am I safer to continue to do that or do I embark on building a new vehicle, which one puts me into more risk. Frankly, the vehicle you have experience on, if you're looking at it at that level and watching those kinds of things, you may be safer sticking with the B-52.

MR. JEFFS: Let me say something about wiring. After the Apollo fire, we redesigned the Apollo; and the wiring in that Apollo was superb. I mean, it's better than any airplane I've ever seen, by far. That same wiring, all those wiring specs and so on, were carried over into the Orbiter. So it's not just a matter of redundancy in the wiring and separate routing of the wiring; it's the detailed quality of the wiring itself and the combing of the wiring and the ties of the wiring and the curvatures and everything else that are all carried over directly into that Shuttle. So there may be wiring problems there in the insulation, for example, in certain areas and it should be looked at, but in general you're starting out with a wiring set that is far superior to most of those that you're normally familiar with.

ADM. GEHMAN: Let me ask a question.

MR. JEFFS: May I say one more thing there?

ADM. GEHMAN: Absolutely.

MR. JEFFS: On the panels, the RCC panels. We were always worried about water in the RCC panels because, you know, graphite epoxy is sensitive to water. You get water in it and you're going to lose properties of the graphite epoxy -- and it is graphite epoxy, after all. So it always worried me that we should take a special look at those panels, and I think the guys were doing that. For example, in the *Columbia* I think those had just gone through a recycling back at the plant, as I understood it. I was always worried in that hashed-up field that we've got between those bodies that we might get some occasional buffeting on those panels and might be working the RCC panels at the interface to the structure itself. I don't know whether that's true or not. There's no way to tell, you know; but it is one of those kind of things that would contribute to aging in that you get a lot of cycles on that joint.

ADM. GEHMAN: That's a line that we're curious about. For example, the RCC is a pretty tough piece of structure but one wonders, after it's been heated to 2000 degrees two dozen times or three dozen times, what are the changes in its properties. That's one of the things we would like to look at.

MR. JEFFS: You've got some RCC panels back, didn't you?

ADM. GEHMAN: Oh, yes.

MR. JEFFS: They went through kind of an unusual environment, but you might get some information along those lines.

ADM. GEHMAN: We're going to do things like shoot foam at them and things like that at 700 feet per second.

Let me change the subject here a little bit and go back to the original design here again, the Seventies again, and talk about weight. Weight was one of the issues that you all wrestled with in order that you could get enough payload up to make it worthwhile. The history of the program shows a lot of concern about weight -- the weight of the vehicle, the weight of the payload, and a number of steps which were taken to lighten the vehicle and to thereby increase what it could carry.

Certainly, as a layman, one of the things that struck my attention was the decision to stop painting the ET because you could save 375 pounds worth of paint. So you get the impression that the concerns about the weight of the vehicle as it developed and the weight of the payload it could deliver into orbit was always on your mind as you were watching weight at all times. Could you describe the history of that process and, am I correct, was this a big concern that you were watching all the time?

MR. THOMPSON: Well, let me comment on that. Anyone who designs a vehicle to go to orbit will have to be careful about weight. Getting 99 percent of the weight to orbit isn't acceptable. So one of the things we struggled with was how to, first of all, select the weight targets and how to allocate the weight among the elements, what kind of weight to hold in reserve at the Level 2 or the Program Manager's level, and how to manage weight over the lifetime of the program like this.

As we got underway in the development program, we intentionally phased the startup of different elements based on several considerations; but weight affected some of this. We started the rocket engines for the Orbiter first because we felt that was the most difficult development cycle. Several months or almost a year later, we started the Orbiter development; and, of course, all during that time we were doing the systems engineering level things, doing the wind tunnel tests of the total system, doing the overall early design things that begins to see how much a design, as it matures, might meet the weight target you put in it to start with.

We deliberately delayed the start of the External Tank until we were pretty far along on the Engine and the Orbiter so that we could then size the Tank, because the amount of propellant and the ISP of the propellant tells you what you can take to orbit. We then started the SRBs last, and we actually left some growth. If you look at the SRBs today, unless someone's done something I haven't heard about, there's about two feet on the front end of the SRBs where you could add more SRB propellant if you really had to. Now, you only get a one for eight gain on the SRBs; but there was still that kind of consideration as we got into weight.

Now, once you have gotten into the program well enough to where you then can have pretty good confidence on your allocations to the different project elements, you still keep a certain amount of weight reserve at Level 2. Then if one of the element managers begins to complain that he's got a problem he'd like to fix but there's a weight constraint -- I can remember in one of our ice follies tests the Tank Project Manager wanted me to give him relief from ice forming on the LOX line because it was going to take too much weight to fix it and a little bit of ice isn't going to hurt you. I said, "No, you cannot have any ice on the LOX line and I'll give you 500 pounds to go fix it." And he went and fixed it.

Now, did weight make us do anything dumb? I don't think so. Did we have to manage weight from day one? Absolutely. The 65,000 pounds, 100 nautical miles due east, when we got to the point where we had to trade a little bit off late in the development program, we did; but then we got it back. Fairly early in the program, we went to the fusion-bonded titanium thrust structure in the Orbiter because we picked up a good block of weight and we thought it was a good thing to do, not because we were in so much trouble we had to do it. But we had to do it -- I mean, we did it to pick up that weight.

As far as I know, they quit painting the Tank after I left the Program. Painting the Tank gives you a little bit of advantage to the external surface, but the number that I remembered was 700 pounds of paint on the Tank. As far as I know, they quit painting the Tank more to save money and it wasn't really necessary rather than that they were in any kind of critical weight bind.

We put moderately tight but reasonable weight targets, and I cannot excuse a single dumb thing we did on weight.

Owen, you maybe want to comment on it at a systems level.

MR. MORRIS: Actually I think you're right, Bob. We did have a weight margin all the way through. As I remember, the Tank decision to take the paint off the Tank -- and this was after I left, but I was associated with it peripherally a little bit -- I think at the same time we quit machining the Tank after we sprayed it. Initially there was a machine job; you actually machined the foam. This left a much more porous surface. At the time that it was decided not to machine it anymore, you then had a hard finish on the outside of the foam; and the paint was no longer needed. And the Tank guys at that time, I think, had some weight problem and that was a good trade-off to trade that.

MR. THOMPSON: I do remember one time in discussing with J. Bob Thompson, the Engine Program Manager, some concerns he was having. I asked him specifically. I said, "J. R., if I give you another 1,000 pounds of weight, is there anything you want to do differently?"

He said, "No, I don't want another 1,000 pounds of weight. I don't need it. I don't want it."

MR. JEFFS: Let me add a couple of things. One of the reasons that the aircraft falls through as far it does on landing is the short forward landing gear. One of the reasons for that is to make sure that the weight was minimum of that landing gear. So we looked for saving weight everyplace we could on this machine. It's characteristic of all the space programs, as Bob said. On the MCRs that I talked about, which are thousands of them, every one of them has a place on it for how much weight this change adds to the system and which drawings carry them. So it was pervasive, and it was designed that way to be sensitive of the weight.

MR. COHEN: From day one in the Orbiter project, we were concerned about weight and we had a weight problem, but as Bob said and George said, I don't recall doing anything that was irresponsible because of weight.

Of course, that heritage came from the Apollo Program. You talk about a weight program. Owen was the aluminum module Program Manager, and we didn't get off the lunar surface unless we get to some real fancy footwork on reducing the weight of the lunar module. On the command module we had to take weight out because of the parachute hang weights. So we had weight problems on every program, but I don't think it caused us to do anything that was irresponsible.

MR. JEFFS: As far as Bob's comment on the weight side, the element of the system that has worried a lot of us from the beginning the most is, of course, the engines, the SSMEs. We're always been concerned that that was probably the place that if we ever had any problems, that's where we might have them. Of course, we had years of development of engines at the bottom of flame pits and so on, as we went through that development, to understand how sensitive and how critical that element was.

One day Sam Phillips and I were sitting together at a meeting at Rocketdyne and they were talking about the weights on every individual component of the engine. We thought that was the right thing to do as far as the requirements were concerned; but we thought, gosh, if we had to allocate the weights, we would probably add a little bit more to the engine side somewhere here, guys. But that's the only area of weight allocation that I could see. We didn't have any problems with embracing that concept on the Orbiter itself.

ADM. GEHMAN: Thank you. Another design parameter that historians have written about is the requirement for reusability. For example, as you are well aware, re-entry vehicles prior to this had had, for example, ablator-type coatings on them which were, of course, gone when they came back but --

MR. JEFFS: Not true. They weren't gone. Some of it was gone.

ADM. GEHMAN: They were used.

MR. JEFFS: They were used. I spent a lot of time trying to convince NASA to shave off those ablators to fly again. They were over-thick.

ADM. GEHMAN: They were well used when they came back. But the reusability parameters drove a number of things. Well, I'll let you describe for me what kinds of things it drove, but the history tells us that it drove such things as TPS systems which could be taken apart in little sections so you only had to rework little sections at a time and things like that. I don't know if that was driven by reusability or not. You can correct me on that. Again, going back in your experience, how was the reusability requirement characterized in your decision-making and your engineering design work?

MR. THOMPSON: Well, again, let me start off. At the systems level when we got into the early Phase A part of the program, full reusability was leveled on the program as a program requirement, under a perception that that would make it a more cost-effective program, particularly in the cost-per-flight regime. Of course, that was coming into a space business where staging and expendability had been a fundamental part of flying to space. One of the reasons the early system could go to space was because you could stage. You'd go part of the way and throw off weight. That even helped them explore the South Pole when they went down there.

So we accepted reusability during Phase A and came up, as I talked earlier, with the two-stage fully-reusable vehicle. But as we got into Phase B and particularly began to look at the details, when you've quit cartooning and gotten down to the specifics of designing and building and basing your reputation on something, then you begin to ask the question, does it really make sense to do it that way? I used to make a kind of simplistic argument that if expendability didn't make sense, there wouldn't be any Dixie cups around. You know, everyone would wash their cups and reuse them.

So there are systems that are more cost effective if you throw part of the system away. Particularly as we looked at putting the cryogenic propellants inside these vehicles and you had to think about insulating those Tanks, making a good thermos bottle inside that Tank and accommodating a minus 430-degree liquid that's going to shrink that Tank. I've got to shrink that Tank six or eight inches and it's still part of my structure.

Putting cryogenic Tankage within the aerodynamic envelope of the vehicle is an extremely difficult job. I don't think we've even done it to this day. So it began to make a lot of sense, at least to me and lots of others when we got into Phase B, to look at throwing part of the system away. The first thing we did was take the LOX out of the Orbiter and then we took the hydrogen out of the Orbiter and then we looked at, well, if we did that, we got the Orbiter down to a size where we didn't need this kind of booster and this kind of booster had a hell of a lot of complexity to it and maybe if we want to meet the national funding level, this is a better way to go than that way and might even be better if we had all the money in the world.

So reusability had a significant impact at the broad systems level and the fact that we put the propellant in an External Tank and threw it away, in my opinion, was probably the best -- and I would even defend today -- the best overall systems level decision we made. I think even if you were starting a system today with today's technology, you might come to the same conclusion.

Now, reusability, once we decided to partially reuse the

Boosters by fishing them out of the ocean and cleaning them out and so forth, brought some concerns to us, particularly as it affected the gimbaling of the nozzles on the SRBs. You have to worry about the APU and the gimbaling systems and so forth after you parachute them into the ocean. So that reusability was a concern; but the fact that you got them and looked at the O-rings and things of that nature were some pluses.

Reusability on the Orbiter? I never remember the fact that we were going to use the Orbiter over and over gave us any unique set of problems that we could have avoided by throwing something away. Throwing the Tank away, I think, was a great thing. Partially reusing the SRBs made a lot of sense; and reusing the Orbiter, particularly with the three expensive engines in the back end, made a hell of a lot of sense.

MR. COHEN: Well, if your question is, if we didn't have reusability on the Orbiter whether we could have come up with a different Thermal Protection System. I think that's where you were going with it. I don't know the answer to that, but I do know that if you had tried to use something like an ablator, it would be very, very heavy. You know, just to give you an example, if I recall correctly, the Apollo ablator was something like 100 pounds per cubic foot and the tile is something like 9 pounds per cubic foot, 20 pounds per cubic foot. So if you tried to use an ablator on the Orbiter, although we have ablators now that are much lighter, you would probably never get off the pad. But I don't think that you would have come up with a different Thermal Protection System.

MR. JEFFS: The whole beauty of the system is the reusability. I mean, you get the spacecraft back. That's the first time we got a spacecraft back really to speak of, unless you got some pieces of it back on parachute or something for other reasons. It's the first time we got the engines back. Usually the engine guys bury their sins in the Atlantic Ocean out there. That's what ELVs are. We don't do that; we get it back.

If you try to minimize cost to orbit, you get your airplane back, get your hardware back. So these guys got as much of the hardware back as they possibly could; and the Orbiter, bless its heart, is the most beautiful example of reusability. That whole reusability was facilitated by that radiated heat shield to get it back. And getting the engine back was an added bonus. So you want to get your avionics back which are expensive, your engines back which are expensive --

MR. COHEN: Fuel cells.

MR. JEFFS: -- your air frame back. And the heat shield makes that possible.

MR. THOMPSON: But had we made you put all of that cryogenic propellant internal to the Orbiter, you'd have had a hell of a bunch of different problems.

MR. JEFFS: Much more difficult.

ADM. GEHMAN: Thank you. But tell me something. I mean, I understand what you're saying, the fact that we have this wonderful reusable machine is a work of art and a work of engineering. It's an engineering feat. But you are trading some things. For example, you are lifting three 8,000-pound engines into orbit for no good reason other than reusability.

MR. THOMPSON: You've got to go to orbit with three 8,000-pound engines, no matter what you do. You can't get there without those engines. Now, you can throw them away or you can bring them back. Now, the Orbiter has to have some capability to bring 8,000-pound engines that wouldn't be there; but you've got to go to orbit with those engines.

ADM. GEHMAN: Just as you have to have the ET to supply the engines with fuel.

MR. COHEN: Right.

ADM. GEHMAN: The ET doesn't go to orbit.

MR. THOMPSON: Well, it goes, for all practical purposes, within a foot per second to orbit. Then you use the OMS to kick it on into it. We did that so we could put it in the Indian Ocean where it didn't bother people.

ADM. GEHMAN: I'm not in any way diminishing the engineering feat of building the Orbiter, but there are design trades that were made in here. For example, if you decided you wanted to reuse the engines or for some reason it was a requirement of the system that the engines be part of the reusable cycle, you now are in the position of having to lift the engines and bring the engines back. It makes the mass of the Orbiter higher on re-entry by 10 percent or something like that.

MR. JEFFS: That's the price of a two-way airplane.

ADM. GEHMAN: That's correct. I assumed that this was all debated and there were people that had positions on both sides.

MR. THOMPSON: It's still being debated.

DR. SILVEIRA: I think involved in that, of course, was the operational cost of the Shuttle in itself and then what you want to do is to return the high-dollar cost components like the engine and the avionics and the like. So as a result, you place the main engines in the Orbiter. You know, no doubt reusability shaped the Thermal Protection System because the two that we really gave serious thought to were high-temperature metals as well as surface insulator. Surface insulator, we thought, was a considerable weight saving.

When we started the program, we actually took on three major developments. One was the main engine, which was the only thing that made Shuttle possible. The other thing was a TPS, which was a major development. You know, we ended up with 6-inch tiles because the guys kept coming to me after tests and said, "Milt, the 12-inch ones keep cracking in half," and I said, "Well, why don't we make them 6 inches." That's what we settled on. I mean, simple as that. Then, of course, the other was the integrated avionics which, you know, is very complicated because, again, when you decided to take the engines to orbit, this gave an airplane with a very aft CG and as a result you had to go to a control-configured system to be able to fly it back.

MR. THOMPSON: Well, you would have had to do that anyway, Milt.

DR. SILVEIRA: Not necessarily. I think you could have flown it back without it if you had a proper CG on the airplane.

GEN. BARRY: I'd like to address another topic, if I may. Another topic would be managing risk, if I could get your perspective on this. We have clearly a system of systems integration element here with the STS. We are trying to address, as a board, providing substantive recommendations that might allow the Shuttle System Program to be strengthened. So, in light of the way you managed risk at the beginning of the Program, I'd like to maybe call on that knowledge base to just comment on a few things.

I know from the readings -- and, of course, my experience at NASA during the *Challenger* when Milt and I were there at NASA Headquarters -- that with the CIL listing, you clearly had a focus -- and you've already brought it up a number of times -- that a concern was with the SSME. Then we have a failure on a simpler, less-complex part of the Shuttle; and that is, of course, the O-ring on the Solid Rocket Booster.

Now, we jump 17 years later and you look at the CIL list again and, lo and behold, at the top of the CIL list is a clear focus on the SSME and we have a problem with, of course, the tragedy on *Columbia* and it is part of the simpler part of this system of systems. It's foam on the External Tank as the leading candidate, as the Board has been working here and trying to determine what the cause.

So the question that we have really got is: How do you manage risk in a system of systems, complex environment that certainly we have here, when you clearly have a good focus on some of the complex elements -- and the SSME is a case in point -- but we miss listening to the material that is talking to us, insofar as an O-ring in one case and maybe some foam in this case?

MR. THOMPSON: Let me start with that and then y'all jump in. What you say certainly was the emphasis on -- if you had asked me when we started this program what would be the first thing that would fail that would cause us to lose a system, I would have probably talked to you about a failure in the Liquid Engines in the Orbiter, number one. I might have talked to you about some failure on the Thermal Protection System. I would have been a long time probably before I got down to an O-ring on the SRB; but independent of that, any flight anomaly should be put on a PRACA, Problem Report and Corrective Action list. And the discipline in the system ought to be such that that PRACA is properly evaluated, in the sense that it's very clear whether it's a life-threatening issue or is not a life-threatening issue and who can sign off on that PRACA.

Now, the O-ring, I could argue whether that would be something that the SRB project could handle alone because you could argue that's internal; but when it's squirting hot gas toward the Tank, it's not internal. It's a Level 2 PRACA. Both of those items should have been entered on a Problem Report and Corrective Action. It should have been listed as something that could destroy the system and it should have come to the Level 2 Program Manager for full discussion and full disposition and full willingness to accept it on the next flight. And at the Flight Readiness Review, the Program Manager should have signed off on both of those PRACAs, saying, "I understand what the failure is, I understand the consequences of it, and I'm willing to fly." Now, if the system's working, that's the way you manage risk; and you should manage it whether it's an O-ring or TPS or a turbine blade in a Main Engine. It should be no difference.

MR. JEFFS: Let me make a suggestion here. I spent some time on this broad area of management review operation with Sheila and others on the Deltas. I think it gets down to the depth of what was stated here by Bob, and that's attention to detail and to every last detail. Every last detail. It's hard to just wrap your arms around something and corral that whole thing.

One thing that I have found useful in the past and suggest on big programs to look at where some of these details need further scrutiny are the MRs. The MRs are Material Reviews. They are identifying little voices that you should listen to. In the space business or in airplanes or anything, you've got to listen to the little voices because that may be the last thing you hear.

MR. THOMPSON: And you have to hear the little voices.

MR. JEFFS: Yeah. You've got to hear them, and you've got to do something about them. What I suggested doing with the MRs is what I call -- it's kind of a parallel to what Krantz and NASA and others have done down here on the what-if processes pre-flight -- and that's to review each MR. If I have an accident, I'm going to go look at the MRs among other things, first thing anyhow. So look at the MRs and do a pre-accident investigation. Just like it was an accident. Go through all those MRs. They are at least an identifier of where some of those voices are listening to be heard.

So how to answer your question any further than that, I don't know. It's get to the details and get to the right details, and that means you have to look at all the details.

MR. THOMPSON: But these two items that have caused the accidents in Shuttle are clearly Problem Reporting and Corrective Action items. Clearly. And if the PRACA system is working, if they're properly identified and they're brought to the right level and the right people discuss it and they make a decision, right or wrong, that's the way the system works. You've got to get them discussed with the right information and the right people and make the right decisions.

ADM. GEHMAN: Let me follow up on that. I think we all kind of agree with that. But some management arrangements migrate over the years. For example, the experience base of you and your team having wrestled with Gemini and Apollo issues, when you had to make engineering decisions or engineering evaluations in the Shuttle Program, you all came with a rich history of being able to sense when you were operating too near the edge of margins and you had the dirty-fingernail basis for understanding that you really did have to give that guy a 500 pound budget, you had to increase his weight budget and he really did need that 500 pounds to do that.

Over the years, management styles have changed. Management organizations have changed. A number of things have happened. For example, the role of the U.S. government person has migrated up and been filled in behind by contractors such that we don't have government people -- not that they're any better than contractors, but they have a different reward system. The experience level of these managers didn't get the same experience that you had because they didn't have all of these projects to experiment on and grow up in and they just don't have this rich background that you all have. They're just as smart and just as dedicated, but they just don't have the same background that you all have.

You have such managerial twists as this Max Faget and his engineering department has been morphed over the years now to where the programs have to pay his bills or he loses his employees. In other words, he's not independently funded anymore. That's a gross exaggeration; they are, but not to the extent that they were independent back in your days. There are a whole number of managerial trends that have taken place, driven by style and budgets and things like that.

So now we get to this meeting in which we're going to properly process an IFA or properly process a waiver or properly process some kind of a PRACA or something like that, but the machinery has changed now. The mechanisms have all changed. Based on good principles, based on first principles that you all have indicated, how do we balance this thing so that these good, proper sign-offs can be made by people who are qualified and understand the system, when the things are not the same as they were in your day and they can't be made the same? I mean, we can't go back and find people with the same kind of experience you had. It's not possible because NASA doesn't have, you know, four or five different space exploration projects going on in sequence in which to build the people with your experience. So somehow we've got to replace that.

What I've heard from you and what I've written down are what I would call first principles, and the first principles are you have to have knowledgeable people with experience and they have to have the authority and they have to have the richness of engineering horsepower behind them in order to make this case. And there has to be some checks and balances. Three or four of you have indicated checks and balances, not single-point failures in the management system.

Could you give me your views on today how you accomplish the things that you've said, when the dynamics of the management system have changed so much?

MR. THOMPSON: Well, you've asked kind of a complicated question for some discussion there. Let me comment this way. I think clearly, over whatever period of time you want to talk about, you have to maintain the internal procedural disciplines. You have to maintain the PRACA system and you have to maintain the forcing function that that puts in the Program because that's a discipline that makes you look at anything that's off-nominal whether it's in the worrisome engine or in the not-so-worrisome SRB. So you have to deal with PRACA. You have to deal with it in a formalized way through a Flight Readiness Review or whatever technique you want to use. So you have to maintain those systems.

Then you have to maintain enough high-quality well-trained people to make good judgments with those decisions. Neither one of these accidents that we've had on Shuttle require Ph.D.s in physics to understand. In fact, they barely exceed high school physics to understand. Erosion rates on an O-ring when there should be no erosion is an obvious thing. Kinetic energy of a 2.5 or three pound hunk of tile when it's traveling 700 feet per second, that's high school physics. There should not be anyone in a key management position in a Shuttle Program who doesn't understand those things in considerably more depth than it would take to make a good decision on them.

Now, why those things didn't happen is the kernel of your question. It appears to me that the agency needs to, number one, make damn sure that the procedures that bring the Problem Report and Corrective Action to the right discussion forum and then the right people are dealing with them in a timely manner.

Now, having said all that, there may still be some actions that occur in the Shuttle that those systems don't catch; but there's certainly no excuse not to have those systems in place and have reasonably good people dealing with them.

MR. COHEN: I think George Jeffs probably said it the best and the simplest. I think the people involved need to pay attention to detail, need to bring issues forward, that they need to pay attention to detail.

MR. THOMPSON: And they need to understand them. It's one thing to pay attention; it's something else to know what's going on.

MR. COHEN: I'll tell you a story, if I may. We were getting ready to go to the moon on Apollo 11. The initial measurement unit on the lunar module was perfect, no drift rate. All of a sudden it started drifting high but not out of spec. We, the Draper Labs or the MIT Instrumentation Lab and the subsystem managers, all went to George Low and told him he did not have to change the IMU out on the lunar module. Very risky. The lunar module was made out of Reynolds wrap almost. And George Low looked at us. He said, “You may be right, but I’m going to change it out.” It was telling a message. It was telling a message that it was drifting -- not out of spec but it started doing something different. I’ll remember that as long as I live as a thing that you need to think about.

MR. JEFFS: Well, you’ve got to make sure that you get people in the right places that qualify in three categories. One, they’ve got to be intelligent. They’ve got to be dynamic, and they’ve got to care. They’ve got to care. If you lose any one of those three, you’ve got a miss. So you’ve got to make sure at least the leadership has those qualities. That’s for the near term.

For the longer term, though, it’s a bigger problem because we in industry are losing our capabilities in these areas and our backgrounds; and you in government are doing the same darn thing. I don’t know what the answer to it is. Apollo was a stretch. Apollo stretched us technically, and it brought to bear a lot of interest and a lot of people in science and engineering. In the broader sense, we probably need something like that in the future to be able to attract our young people to science and engineering.

DR. LOGSDON: This is really kind of a follow-on to the discussion we were just having. I mean, the five of you represent the first generation of people that learned how to do things in space in this country. As Bob Thompson has said, putting people in orbit and getting them back safely is one of the hardest things that humans do. Most difficult. Most challenging. You are all here under the auspices of the NASA Alumni League, which should indicate that you have continued some involvement with the agency. Are you willing to give us your impressions of the NASA of 2003 as an organization? Is it up to the job that faces it? And if not, what sort of things you’ve suggested in the past few minutes are needed to fix it?

MR. THOMPSON: John, I would personally dodge that question because I left NASA 20 years ago. I do not think that manned space flight is beyond the technical capability of this nation by any stretch of the imagination. I think the young generation, in many respects, is smarter than we are by far, better trained. So I think that what we’re talking about here is easily achievable. There’s no reason the NASA today can’t function well and operate the Shuttle safely, whatever that means, and take on whatever future things you want to do in manned space flight. So I haven’t lost faith in the agency.

Now, I do think you have to be extremely careful when you draw the interface between government and industry. I’ve been on both sides of those fences. The people on both sides are just as honest, just as dedicated; but they’re driven by different things. If you’re in industry, you’ve got a different set of constraints on you if you run the program than you are when you’re in the government. I think the NASA of today ought to be very careful in drawing back so far and saying that contractor’s responsible, when he really doesn’t have the ability to be responsible if he doesn’t control the subs or doesn’t control the associates or he’s not in a position to make all the right kind of balance judgments, don’t put the muscle on him. I mean, don’t put the monkey on his back if he doesn’t have the muscle. So my only comment is I don’t believe NASA is serving itself well if it pulls back too far in feeling an overall technical management responsibility for ongoing programs.

MR. COHEN: I cannot answer your question directly either because I’ve been away from years. But I have had the opportunity since I’ve been gone to teach at Texas A&M. Seniors. I can guarantee you that those young men and women that are coming through the class, I would hate to compete with them. They are truly outstanding. Many of them, whether they get their advanced degrees and go to MIT or whether they go to Purdue or whatever, most of them want to go to work for NASA or their contractors.

So good students are very interested in the space program and a lot of my students did come to work at the Johnson Space Center and other space centers. So, you know, I think the people are there and the people are good. I mean, the students today, as you know, are just outstanding.

DR. SILVEIRA: John, if I may. You know, there’s no doubt in my mind that the kids today are better educated than we are. I have two kids that work in the Program, and they’re both smarter than I am. The thing I get paid for, at least, is to try to go out and find out what’s going on in industry that we don’t get the product we used to get out of them.

I think some of it comes about because we have started to train a lot of paper engineers rather than hardware engineers. Kids are not looking at the hardware enough to really understand what’s going on and, anytime there’s a little discrepancy in it, really get to understand what is happening. The hardware’s trying to tell us something, and we don’t carry it to a point where we really go and understand it and fix it.

You know, recently we had a PDR of one of our programs, you know, and the contractor was proud: “We have spent 3,000 man-years on documentation.” I can’t imagine a program demanding that kind of paper to keep it going. I think the thing we need to do is to get kids out from behind the computers and get them to go out and walk the factory floor and really see what hardware’s all about.

MR. JEFFS: I’ll say three things from the industry side. I won’t try and reorganize the NASA. That takes a little longer. But I think that, as Bob mentioned, we march to different drummers, in a way; but when I ran the space and energy operations for Rockwell, I was also a corporate vice-president of Rockwell. So I had a lot of pressure that didn’t have a thing to do with the space program, but it didn’t keep me from applying the right kind of people on the problems at the right time in the right way. And I think these guys will all attest that they didn't see anything in the results of what happened with the industry on their hardware that was influenced in any negative way by profit motives or otherwise in getting those problems solved.

Number two, there are a lot of smart people out there in industry. They can be assigned. There are talents available to the people that run these companies. I think it takes their focus also to get the right kind of people in the right place at the right time on the space program and to look at their priorities.

The third thing is that one of the things that made Apollo and Shuttle happen was an excellent working relationship between industry and government. That working relationship was criticized in many ways by being too close and what have you; but I assure you, when it came to solving the technical problems, it wasn't. I also assure you when it came to getting any money out of these guys, it also didn't manifest itself in the way of excess profit. So I think that encouraging the good working relationship on mutual utilization of each other's capabilities is an excellent additive to making these big programs happen properly and on time.

MR. MORRIS: I'd like to follow up on that just a little bit. I think one of the things that over the last 10, 20 years has happened in this process of NASA going up and being backed by contractors is a lack of sufficient check and balance. The one thing we had in the Apollo Program, in the Shuttle Program, during the design phase, was parallelism between the government and the contractor. Both were very good, but they also were checks and balances. When you turn all the responsibility either to the government or all the responsibility to the contractor, you lose some of that check and balance.

I think the process that you have to look at things like the O-ring or like the foam, you need to make sure the process you have asks the second question, not what did that cause on the last flight but what else could it affect. I think in both cases the second question was not asked properly. I think that's the thing that can be fixed with a system. The system that assures the right checks and balances and the right questions are asked.

DR. WIDNALL: Not including the space program, what are the other major scientific and technical challenges faced by our nation that have the power to motivate our young people?

MR. THOMPSON: I think, frankly, the Defense Department is one of the greatest motivators of our young people. I think maintaining a very strong and very active military or defense capability or offense capability, either way you want to talk about it, is a very important contribution to our society. We in NASA often take a lot of credit for technology advancement. I'm not so sure in the same number of years the technology advancement wasn't stimulated more by the Defense Department than NASA. The fact that you have to solve the kinds of problems that the military solves on a routine basis drives technology certainly as much as the space program. Obviously medical research. So I could list eight or ten things, but certainly we benefitted to a great extent in the NASA space program by what was going on in the Defense Department in similar activities -- be it rocket science, be it structures, be it flight control systems.

For example, at the same time we were putting the control-configured flight control system on the Shuttle, DOD was doing the same thing with the F-16. And we visited their research laboratories and they visited ours. We took some things, learned from them. They took some things and learned from us. Both systems are working today, 35 years later, quite well. So I would like to see us maintain an extremely strong national defense capability, if for no other reason, to drive the kind of thing you're asking about.

MR. COHEN: I think in my observation, being in academia for a while, is that there is a lack of funds for students that want to get their advanced degrees, to go on to get their Master's degrees and Ph.D.s. I think that could be a big stimulus to producing more graduate students and actually enhance our engineering capability in this country.

MR. JEFFS: They had a session not too long ago that George Abby pulled together at Rice that addressed the subject in part; and it seemed to me that to attract the young people, it's going to have to take something that has duration long time. Most of the military programs, albeit some of them are changing now, are lesser duration. It needs something that people can address and assign their life to, youngsters, and enthusiastically do that. I think that the NASA has that within its grasp if they better structured and articulated the total space program, the unmanned systems and the manned systems. And I think manned systems have to be an element because they have the aura. They have the thing that brings the young people into it more than the unmanned programs do. But the unmanned programs and the manned programs go together. So a better articulation of the total program. The targeting of something like a Mars stretch or something such as that, like the Rumsfeld approach, get out in front of the pitch, go out --

DR. WIDNALL: George, I specifically ruled out the space program.

MR. JEFFS: Oh, you did.

DR. WIDNALL: Yeah, I did. I really wanted to talk more comprehensively about our whole society, science and technology and our young people. I think obviously I think we all understand the power of space.

DR. SILVEIRA: As you know, the President has charged Missile Defense Agency with a deployment capability into '04, beginning of '05. That's a pretty big technical challenge.

ADM. GEHMAN: Let me ask a question that I think is related. Once again, going back to your experience in

Gemini and Apollo and Spacelab. These programs were not exactly heel-and-toe programs. There was a little overlap among those programs and people migrated and people learned and people worked their way up through the process.

In your judgment, what's a generation in a space vehicle? In other words, how long do you think that we should stay with a space vehicle and how big a leap do you need to make to have its replacement come along? Is 20 years, 25 years, 40 years, a generation? And should we have a replacement program already have been started? What's the time frame here and what are the indications or the characteristics of when it's time to say that's a generation? You've all heard of Moore's law that a generation in computing power is 18 months. Well, what's a generation in a space vehicle?

MR. THOMPSON: Let me make a jump at that because I've thought about this a little bit in my own career. In my working career, I spent the first 11 years in basic research at a research laboratory and, frankly, I was beginning to not get burned out but I was ready for a change. The space program came along. I got in the space program; and we did Mercury in about four years, as I recall, from the time we started talking about it until we had finished it. Before we finished that, we took on Gemini; and we finished that in maybe five. Let me just pick a number. Five or six years. Before we finished that, we had Apollo. We did Apollo in ten years. We then bootstrapped Skylab in there for three or four years, using the residual Apollo hardware. So during that 20 years, you know, I never spent more than ten years in any one focused area -- sometimes as few as four, sometimes as many as ten.

When we took on the Shuttle, Skylab and Apollo/Soyuz were the only things in town, and we had a gap of activity of three or four years, five years where we didn't fly anything from Soyuz until we flew the first Shuttle. But that ten years was a very strong development cycle. So for people at least like myself, there was an interesting activity every four to ten years that lasted anywhere from four to ten years. So you could jump from one to the other and grow as you jumped.

Now, if the country does not take on those kind of programs and you say stick with the Shuttle for 50 years, then you have to find some way, internal to that, to keep people excited. Maybe you do it somewhat like the military does, by rotating them every three years or rotating them every --

MR. JEFFS: Two months.

MR. THOMPSON: Again, the military found out in the R&D program it didn't want to rotate them as much because they lost the technical competence. So if it's not possible for the nation to throw an exciting new program out there every five years, then you have to look for some other motivation below there. I would say ten years in any one kind of an assignment is probably enough for most people and they need to go do something either more complex or something different. But that's just a wild guess.

MR. JEFFS: These programs cost a lot of money; and therefore when you start them, you better darn well make sure you've figured out what you want to do with them and what you're trying to do with the programs. That's kind of item number one.

The other thing is that these programs are often paced not by money and talent but they're also paced by technology. So there's no point in taking off on a single stage to orbit if you don't have an engine that can perform that kind of mission. So we go charging off and we all get together and say, "Let's go single stage to orbit." Then say, "Well, that's great but how do we get there? Oars?"

So therefore you've got to look at the technology base as it permits you to make decisions for the next generation. So I think, like Bob, it seems like it's five years, Gemini; 10, 15 on Apollo; 15, 20, maybe 25 on Shuttle. The next one is going to be longer than that. But it's going to have to have the technology behind it that enables you to commit that kind of funding and that duration of lifetime of people to it.

MR. COHEN: I think there are things you can do. In fact, things have been thought of that you can do is to in some way combine the talents of the human exploration program and the robotic program for Mars exploration and bring the human element of the program involved in that. I think those are things I think you could do.

I mean, one time we looked at a Mars sample return mission, JSC working hand in hand with JPL to do a Mars sample return. It never did come to fruition, but I think things like that would really create the interest and keep the people sharp and keep people very interested.

DR. SILVEIRA: When you consider that the Shuttle is a first-generation vehicle, first of its kind, you would think -- and I know a lot of the mistakes we made in the design initially that we have found out as a result of flying the vehicle. You would think within a 20-year time period that we would be coming up with a better design, seeing it's going to take another ten years to build a vehicle. I think it's far overdue that we should be into a second-generation vehicle similar to Shuttle.

MR. JEFFS: If you know what you want to do with it.

ADM. GEHMAN: My question, at least what I had in mind, was more along the programmatic and technology angle than it is the human resource angle. I appreciate what you say, and I agree with what you say. You've got to challenge people if want to keep good people working on these things. But it does seem to me that a generation in space vehicles -- I mean, I can't put a number on it, but I can tell you that it's not zero and I can also tell you that it's not 40. A generation is someplace in between there; and if it's some number less than 40 and it takes seven, eight, nine, ten years to produce this thing, I'm wondering how urgent it is that we get on with this.

MR. JEFFS: You know, I would like to add one thing to the previous statement. There are lots of opportunities that can be identified; and some of them have some very interesting possibilities, I think. I would commend the agencies and others from initiating the nuclear engine programs. I think this is a whole new avenue that's going to open up a lot of possibilities. I think that the idea of coming up with some engine that will essentially be unto itself, a turbojet or engine, a rocket and the whole schmeer in one swoop is an excellent kind of focus if there's feasibility basis behind it.

Those are the kinds of things that will offer the opportunity to identify these kinds of program. If I were going to try and build a new Orbiter today, I would do a few things differently, but I don't think the machine would be a heck of a lot different than before. It might have titanium in it instead of aluminum, for example. It might have a different, more rugged tile system, even though the one we've got is adequate. There might be a lot of things that we could do with it that would make it a better racehorse, but it would be in the thoroughbreds instead of the claimers or something. You know, it's not going to be that big of a step forward. But those other kinds of things like the engines and so on, nuclear engines and so on, those are the things that are going to offer the opportunities for us.

ADM. GEHMAN: Thank you for that. Assuming that if we could cast off to the side, for example -- this is argumentative, so you just have to make an assumption with me here -- if we could cast off to the side that the next step that we make in space has to be a leap -- I mean, why can't it be a tiny step? You know, aircraft developed by evolution. We didn't go from the Wright flyer to the 747. We went in many, many, many evolutionary steps.

So I hear this all the time that, well, you've got to stay with the Shuttle because the next giant leap is not there in front of us. I don't find that to be completely compelling. The President has already said that man is going to continue his journey in and out of space. Is there any reason why we can't do that journey in an evolutionary way, that we have to have some big, giant leap in technology to do it?

MR. JEFFS: No, but it has to be enticing enough for the new generation of people coming along to want to dedicate their lives to it. We're already losing our capabilities now on the one we've got. It's not sexy enough. It's not exciting enough.

MR. THOMPSON: Well, let me argue with that a little bit. I tried to allude to this before. When Nixon made the decision, the so-called low earth orbital infrastructure decision that I spoke about earlier, there was no big national-level discussion of it or national-level announcement of it or national-level description of it. So a lot of attention was not drawn to it. Part of the reason, politically, you were proposing to do something that was considerably less expenditure, less effort, less glamorous than the Apollo Program. So compared to what Kennedy did with the Apollo Program, announcing a low earth orbital infrastructure wasn't nearly that sexy, so to speak. Plus, the personality of the man, he wasn't that interested in space. So he didn't make a big to-do about it.

There is plenty about what we're doing today and what we will do in the next 10, 15 years that should excite a lot of capable people to work on it, even though it's not exploring Mars. I frankly think it will be a long time before you can convince any Congress to spend the money to embark on a properly thought-out Mars exploration mission because it's going to be extremely costly and there's going to be a hell of an argument about whether it's worth that cost as compared to putting the cost somewhere else.

So I think what is needed is a little more attention to explaining. For example, the space station, I think, is a very exciting program. The thought somewhere in the future of direct solar conversion to electrical energy with a solar power station in orbit. The kinds of things you can do in a low earth orbit with Shuttle and space station-type vehicles could be made into a very exciting program.

Part of the problem is that people want to throw that aside and go to Mars for some reason, and we've got to put the defense in that because I think where the nation's going to spend its money for the next several years in manned space flight is going to be in low earth orbit and we'd better start explaining the beauty of it and I don't think you're going to have any trouble getting plenty of people to work on it, good people, if you'll talk about it and explain it properly.

MR. JEFFS: The only addition to that is that Apollo dragged with it a lot of technology. A lot of technology came out of Apollo. A lot of new businesses came out of Apollo. It was a stretch and it was an exciting kind of thing. And if you don't have a stretch, you're not good to drag the technology. And I think that dragging the technology, forcing it into the forefront is the thing that's best not only for the space program but for the nation.

MR. COHEN: In order to do what you say, though, I think some group or some body, some body of people need to establish the need for doing it, what is the need, what are you really trying to accomplish, before you can really move forward to the next step, I think.

ADM. GEHMAN: Let me close this by asking the last question, which is a complicated one. My understanding of the glorious history of space exploration in which you all play an important role is that over the years the role of the NASA engineer has migrated in a sense. You read in popular literature that in the original program that Werner von Braun was accused of wasting money because when he received components from contractors, he had his engineers take them apart and put them back together again. I don't even know if that's true or not. In any case, those engineers, even though they didn't build this thing, they now got dirty-fingernails experience; and as you went through the Gemini Program and Apollo Program, a lot of that was in-house work. There was a certain amount of basic research and basic engineering that was done in-house and some of it was done by contractors and some of it that was done by contractors was checked by in-house engineers. Then as we migrate away, more and more of this work is being done by contractors and less and less of this work is being done by NASA employees.

So my two-part question. Is this management by subs -- let me get to my bottom line. Then I'll ask the question.

One of the possible outcomes of this Board's work may be some comment about some kind of a system qualification or a system recertification that if you were to really fly these Orbiters from one decade, two decades, into their third decade that, just like a 747 or something, if you're going to extend the service life of it, you ought to do some kind of a system qualification or system certification. Well, if there's nobody at NASA that has that hands-on engineering experience, then you've got to have contractors do it.

Now, does that get us into a boxed canyon here? Does that trouble you, or would you think that the style that you all grew up on in which NASA engineers also had hands-on engineering experience by some way is either critical or not critical? A lot of people have said it's not necessary to do that. How do you feel about that? Particularly in light of a possible outcome where it's possible that we might have to in some way formally recertify the three remaining Orbiters or requalify, do we have to do it system by system and who does it?

MR. COHEN: Well, I know when I was Center Director of the Johnson Space Center I always liked to have at least one or two projects, in-house projects where the engineering talent at the Johnson Space Center was doing the work. I think that was carried on. I think they went pretty far with one of the crew rescue vehicles they were designing here at the Johnson Space Center. They went pretty far with that. So I think in-house NASA projects or in-house projects at NASA that they can actually, as Milt said, get their hands dirty on is very worthwhile; and I think it does teach them an awful lot. Now, that takes money, it takes emphasis, but I think some type of steady, continuing having of in-house projects, I think, is very important. That would answer, I think, part of your question.

MR. JEFFS: I'd like to make sure the picture is not painted in some strange fashion here. The NASA guys are the guys that set the requirements and check the product as it meets the requirements. Industry is the one that puts the product together. The drawings are all prepared by industry and all the specs are prepared, all the list of materials, everything is built and tested, all the tools are made by industry. Industry does the job.

Now, if you're going to recertify the vehicle, industry, with NASA's overview, would be the one that puts together the details of what that recertification process should constitute and consist of. So it's not like NASA is doing all the job. NASA is a supervisor and an overviewer. Industry is the one that does the job.

I'd also like to say that you made some comment earlier about testing and checking. On occasion we've had to check NASA tests. Every once in a while NASA runs some pretty strange tests, too. So we've had to straighten that out. So it's both sides.

ADM. GEHMAN: It is both sides, but it is healthy.

MR. THOMPSON: You do, though, need to have -- what George says is exactly correct. Nowhere in our manned space flight experience, except extremely early in the Mercury Program, did NASA sit down and do the drawings and build in NASA shops a spacecraft. The first spacecraft we flew in Mercury, we actually designed with civil servants in the Langley Research Center. We built it in the Langley Research Center shops with civil service people and we took it down with the support of the Air Force and launched it on an Air Force rocket at the Cape and got our early Mercury data off of a thing called Big Joe. From that point onward, the people who do the drawings, the people who do the detailed internal stress analysis, the people who do the certification, formal certifications at all level, that is industry's job. That's what you contract with them.

My point I would like to make is you need to contract with them in such a way that they can bring their talents to the program effectively, but you have to leave the government in a proper control mode in that contracting format. If you contract in such a way that it isolates the government from some feeling of responsibility or some feeling to need what's going on or some reason to make critical decisions, then you've backed the government out too far. For example, if you take all of the contractors working on Shuttle and assign them under one integration contractor and give him all those contracts to run, that's fine; but you haven't gone down to one contractor. You've gone from 80 to 81 contractors, and you then have to back the government off to let that contractor assume a certain level. Otherwise, you might as well stick with the government and 80 contractors if you're going to still penetrate to where you are. But you also have to set up the contracting channels properly and the responsibilities properly.

I personally favor something much more like we had in Shuttle where, for example, no contractor in Shuttle had the leverage over the other contractors. Rockwell could not go tell Martin to do anything from the Orbiter to the Tank. It had to come through a government channel to get something done, and the government then was in a very knowledgeable and in a very controlled position to do it that way. It puts a responsibility upon the government that you've got to be prepared to fulfill, but I think it keeps you involved in a much more meaningful way.

Typically, in my judgment, in the earlier years, NASA penetrated the program probably a notch lower than the military DOD typically penetrated their programs. The NASA that I knew did not need the aerospace support to the same level that the Air Force needed aerospace support on the ballistic missile program. Either way, you can make it work; but you ought to decide which way you're doing it and make sure you make it perfectly clear. And I would very much like to see NASA retain a capability to penetrate the programs relatively deeply.

MR. JEFFS: I'd like to make a comment on one other statement that you made. That was about hands-on. I think hands-on is a fundamental need for the engineers on both sides of the fence, both the NASA and industry. One of the classic examples was to take thermodynamics people down and show them the hardware that they were actually influencing, changing, and controlling the configuration of. It's a revelation to those. You find the aerodynamics guys, aerothermal guys, thermo guys and so on tend to get remote from the program and work with just paper. Get them out and show them the hardware and it gives you a better project, a better person that's working on it engineering-wise, and he has greater accountability and responsibility for it. So that's true on both sides.

MR. MORRIS: I'd like to build on that a little bit, if I could. I think NASA in particular needs to be very careful that they retain smart management. I think, to do that, they have to come up through the ranks with a few dirty fingernails, maybe even greasy fingers. One of the things that really upset me was the cancellation of the X-38 project, the recovery vehicle that Aaron was talking about. This was a chance for the people working for NASA to actually understand how you go make something happen. By doing that, they then become much smarter managers.

I think at the time NASA pulled away from management in detail -- and there were a lot of good reasons to do that -- there was then at the same time a promise made that research and development internal would be increased, and increased materially. I don't think that's happened. Therefore I think the NASA personnel have lost out both ways over a period of time. They no longer are managing in detail and they are not backing up, in research and prototype development, the experience level within the organization that they really need.

ADM. GEHMAN: Well, thank you very much, Mr. Silveira, Mr. Morris, Mr. Jeffs, Mr. Thompson, Mr. Cohen. We thank you very much for joining us here today. We thank you very much for your open and candid discussions of all these issues.

As you can see, the Board has a fairly wide aperture about what we are going to write in our report. They include such matters as you have discussed with us today; and your background knowledge is still valuable, still of great benefit to the nation. I thank you very much for agreeing to contribute it here in such an open forum. We really appreciate it very much, and we wish you all the best of luck. Thanks very much.

We will reconvene at 1:00.

(Luncheon recess, 12:14 p.m.)

COLUMBIA ACCIDENT INVESTIGATION BOARD PUBLIC HEARING
WEDNESDAY, APRIL 23, 2003

1:00 p.m.
Hilton Hotel
3000 NASA Road 1
Houston, Texas

BOARD MEMBERS PRESENT:

Admiral Hal Gehman
Rear Admiral Stephen Turcotte
Major General John Barry
Dr. John Logsdon
Dr. Sheila Widnall
Mr. G. Scott Hubbard
Mr. Steven Wallace

WITNESSES TESTIFYING:

Dr. Jean Gebman
Mr. Robert P. Ernst
Dr. Diane Vaughan

ADM. GEHMAN: Good afternoon. The afternoon session of the Columbia Accident Investigation Board public hearing is in session. This afternoon we're going to hear from two experts on the subject of aircraft aging, which is another risk element in the Shuttle program which wasn't originally foreseen -- at least I don't think it was. The Shuttles were originally designed to last 10 years and now we're passing 20 and headed toward 30. And the Shuttle vehicle then is facing issues which need to be looked at to determine whether or not the shuttle can operate safely. We're very pleased to have you two gentlemen join us.

Dr. Jean Gebman is a senior engineer at the Rand Corporation; and Mr. Robert Ernst is the head of the Aging Aircraft Program at the Naval Air Systems Command, Patuxent River. We're glad to have you both with us.

I would invite you to introduce yourselves and say a little bit about your present job and your background; and then if you have an opening statement or a presentation, please go ahead and proceed. Why don't you both introduce yourselves first, and then we'll go ahead with the presentation.

JEAN GEBMAN and ROBERT ERNST testified as follows:

DR. GEBMAN: Thank you, Mr. Chairman. I'm Jean Gebman, senior engineer at Rand, working on the Aging Aircraft Project. My educational background is in aerospace. My doctoral work majored in structural dynamics with minors in fluids and control engineering.

MR. ERNST: I'm Bob Ernst, the head of the Nav Air Aging Aircraft Program and also representing the Joint

Council on Aging Aircraft, which is a DOD, FAA, NASA, and industry consortium trying to work on age issues. I don't have the storied credentials and degrees that my counterpart here has, but I've got a lot of years of experience working on old platforms and rust and corrosion and obsolescence and some of those types of things.

ADM. GEHMAN: Thank you very much. Go ahead and proceed.

DR. GEBMAN: Thank you, Mr. Chair. Bob and I are going to present two briefings that are very complementary. I'm going to talk about some technical details to give you a somewhat hurried survey of the landscape technically, and then Bob's presentation is going to deal with some of the cultural and programmatic matters.

Next chart, please. This is simply a bit of background. In the interest of time, we'll just press on ahead. Next chart, please.

The examples that I've selected do have a methodology behind them, and this chart is an attempt to try to capture the essence of that. We're going to focus on the top set of items, although aging aircraft do involve all of the functional areas that are listed on the left-hand side of the chart.

Next chart, please. So this is going to be the focus.

Next chart. Whether or not this focus proves helpful to you is, of course, a matter to be determined as your investigation moves forward. So my purpose here today is more to share with you some areas where the aging aircraft experience might prove helpful as you move down the road.

Next chart, please. You all have seen the various diagrams of the Shuttle. I'm going to focus on the left side.

Next chart. And simply make a couple of points. We have four main spars that go through; and when we talk about structures and structural dynamics, one of the things we often quickly look at is the wing route where the spars go through. That's just simply one area that one is always interested in.

Next chart. Another area that's of interest and will be touched on by one of my examples subsequently has to do with the aluminum honeycomb. This is simply a cross-section showing at the top there the interior face sheet, which is aluminum; the corrugation, which is aluminum; and the adhesive bond between the corrugation and the exterior face sheet; and then, of course, the Thermal Protection System underneath. A very sophisticated system. And one of the things we will be talking about later is the matter of adhesion as a method of joining structural materials together.

Next chart, please. This is a list of the samplers. Let's get right to it.

Next chart. B-52. A very interesting story. This often is pointed to as here is why it is possible to maintain a fleet for a very long period of time. We need, though, to be cautious and acknowledge how it was we got to that situation, because you may note that the G model and the D model have long since gone to the boneyard. Corrosion was the principal culprit. The basing at Guam was about the worst base you could be at for an Air Force aircraft from a corrosion standpoint.

Next chart. Even the H model, to get it to where it is today, has been significantly rebuilt in many areas, as these various shaded areas demonstrate. Moreover, it has been based at a location that is relatively benign from a corrosion hazard standpoint, and the maintenance people learned a good lesson from the experience of the G model; and there has literally been a zero tolerance for corrosion. If they see corrosion, it must be removed.

When we visited the depot about six years ago, we looked B-52 and the KC-135s. I was challenging the technicians on the B-52, "Show me the corrosion."

They said, "Dr. Gebman, there is none."

I said, "Folks, it's an old airplane. We know there must be corrosion."

Finally, they were able to show me a detail at the back of the airplane and they acknowledged, we ground out a little bit back here, but this is not even significant.

This airplane is very different from the 135. Next chart, please.

ADM. GEHMAN: Could I ask you to go back a second. In that first bullet, what is a full-scale fatigue test, what's a damage tolerance analysis, and what's a tear-down inspection?

THE WITNESS: The full-scale fatigue test is where you take an article that could be flown in flight and, instead of doing that, you set it up to be loaded cyclically by attaching various jacks and an enormous hydraulic contraption and typically you will try to simulate two -- in the old days, four -- equivalent lifetimes to identify where the fatigue vulnerabilities are so that they can be addressed during production and/or during maintenance.

ADM. GEHMAN: And I assume also recognize -- I mean, in other words if you have a fatigue indicator like a crack or something like that, the idea is that you would then be able to recognize that if that were to happen in a service vehicle.

DR. GEBMAN: One of the most important things you learn from the test is where the cracks are taking place and so that you can set up a maintenance program or do a modification so you don't have to set up a maintenance program. The damage tolerance analysis is a method of studying the growth of fatigue cracks and their significance, giving you further information that you can use for fleet management and modification purposes.

The tear-down inspection took place in the 1990s, largely to identify places where corrosion was going on in areas that could not otherwise be seen. When we do heavy maintenance, we don't take the airplane totally apart. The notion of a tear-down inspection is to take a high-time airplane which you're prepared to sacrifice and literally take every part, open it up, and see where you have challenges.

MR. WALLACE: Is the concept of damage tolerance that you will be able to detect cracks and things and also make predictions as to their growth rates in such a way that you can easily detect them before they become critical?

DR. GEBMAN: Yes. And I would encourage, if I might, that we try to speed through the examples because you will have an opportunity to see illustrations of some of these specific points.

With the board's permission. Next chart, please. Moving on to the 135, corrosion is the principal challenge with that fleet.

Next chart. This is an example of a tear-down inspection. What you're looking at is a drawing of the top view of the full fuselage. Each square is an area that they took the structure apart, opened it up, looked at it sometimes under a microscope. If you see color in the square, it means they found at least light corrosion present. Just about every square that they did a detailed examination of, they found some indications of corrosion with that fleet. That is a result of the materials that were selected, the environment in which it is operated, and the maintenance program which it had through its lifetime.

Next chart, please. Similar view. This time it's the wing structure.

Next chart, please. As a consequence, over time when these airplanes go in for heavy maintenance now on a five-year cycle, it can take a year to do the complete job.

Next chart, please. This chart shows declining labor hours required. We are now at a point where the labor hours to do that heavy work is eight times what it was the first time it was done when the airplane was about eight years old.

Next chart, please. Until very recently it was the Air Force's intent to keep all KC-135s to the year 2040 or thereabouts, at which point the fleet would be 80 years of age. Recently the senior leadership has decided that the older airplanes, the E models of which there are somewhat more than 100, need to be retired sooner than that; and they are now looking at leasing perhaps a 767 to fill this particular function. So one's perspective about life can change significantly as you learn more and more about the growing burdens before you.

Next chart, please. Moving on now to a new decade. Next chart. I share this example with you to illustrate some of the complexities and depth and breadth of endeavor one can get into when dealing with life issues. Now, the irony is that this is dealing with the new C-5A in the early Seventies. It had a very unfortunate experience in its full-scale fatigue tests. Fatigue cracks throughout the airplane, especially in the area of the wing.

The Air Force Scientific Advisory Panel convened a study in 1970 for the Air Force, made some recommendations. The following year, a major engineering effort was launched. The independent review team. One hundred people worked for one year, going through the results of the full scale fatigue tests, looking at the different options that the Air Force might consider, analyzing Options A through H, and presenting them to the leadership. Ultimately Option H, wing redesign and replacement, was selected. Once you open up the area of structures, the number of things that you can end up having to examine can be considerable. That's the lesson from this particular example.

Next chart, please. This example is a little bit different. We're focusing on a specific technical issue. It's honeycomb composite material, and it proved, in those few areas where it's used on the F-15, to be quite challenging.

Next chart. These are some of the methods in which the water and the corrosion and cracking and durability issues arose with that particular fleet. To the extent that this proves of interest, the area of honeycomb composites, this particular fleet -- and there are some other examples -- might be worth looking at.

GEN. BARRY: One comment on that. This is also the leading edge of a lot of the wing forms in the F-15s, particularly in the tail as a point. So might be of interest in the board.

DR. GEBMAN: Yes, sir. Thank you.

Next chart. Moving on to the Seventies, here we have two examples dealing with the loads that actually occurred, exceeding what the designers thought the loads would be.

Next chart. This is a classic. The F-16 was designed for both air-to-air and air-to-ground work; and it turned out that in the air-to-ground mission area, the loads that the structure encountered very quickly exceeded the capacity of the structure as it was designed. This illustrates the importance of really monitoring your loads through your life cycle so that you can take that load information and update your expectations as regards fatigue cracking.

Next chart, please. This is the process. This is the durability and damage tolerance analysis process and I'm certainly not going to lecture on this today, but this is a summary that you might find useful as your work moves forward. When I look at this, I look at it from not only a structures viewpoint but also from a systems viewpoint. You can literally go through that chart and change its orientation from fatigue, which it was designed for, to corrosion or other kinds of things that affect an aircraft as it ages. Indeed, today people are working on the development of what's called a functional integrity program approach, which mirrors this aircraft structural integrity kind of program.

Next chart, please. The B-1 example is a little bit different. Here we were dealing with acoustic fatigue, which is a dynamic phenomenon and it's a bit like the tuning fork. If you hit the tuning fork, it will vibrate at a natural frequency. Well, aircraft structures, if excited at their natural frequency, will engage in vibration, and this can greatly accelerate the propagation of fatigue cracks. That's the essence of that particular story. It's an interesting one from your all's perspective to an extent because it involved both thermal, aerodynamic, and structural dynamics. It turns out that the designers deliberately had hot exhaust from the engines going over the control systems at low-speed flight to increase the control authority of the control surfaces.

Next chart, please. Now for our final example. Next chart. This is an airplane that served quite long in terms of landings. It was designed for 75,000, and in flight hours it was not all that high. It was designed actually for 50,000. This example illustrates the three things listed on the chart.

Next chart, please. Imagine yourself flying over the Pacific in this particular airplane. You're in Row No. 5. You have the seat next to the window, and over your left-hand shoulder there's a fatigue crack. From the NTSB's excellent work, it appears that the sequence we're going to talk about started at the fastener hole indicated here. What's important to focus on here is the length of the fatigue crack. The blue is supposed to depict the sky. From the outside of the airplane that crack was only a tenth of an inch long, and yet it contributed to a sequence of events that we're going to look through in the subsequent charts.

Next chart, please. Part of the problem is that it wasn't just one crack at that fastener. There was one on the opposite side, as well. It was only .11 inches. So from a detection standpoint, this would have been a bit of a challenge to be detected visually just from a casual walk-around kind of inspection. From a fracture mechanics standpoint, though, the crack is really a half inch long because when you look at the stress intensity at the tip of the crack, what it depends upon is that total length, that .53 inches. And fatigue cracks, we now know, grow at a rate that is a function of how long they are. So the longer the crack, the more rapidly it will grow as that part of the structure goes through its next cycle of loading up and down.

Next chart, please. Not only was Fastener Hole 5 cracked on both sides, but there were also adjoining fastener holes numbered 3 through 9 that also had these kinds of cracks.

Next chart, please. Consequently, Fastener Holes 3 through 9 simply zipped across one afternoon when the loads hit a particular level; and this particular sheet of metal separated from its counterpart.

Next chart, please. The problem is -- and I must apologize, this chart didn't quite make the translation from Macintosh to PC the way I had hoped -- this chart is intended to illustrate two pieces of skin with an adhesive material between the skins. You see, the fasteners were never designed to carry the load. The load was supposed to be carried by the adhesive. The adhesive broke down. There was corrosion that took place. So we have a combination of adhesion failure and corrosion going on, destroying the primary joining mechanism. The fasteners picked up the load, but cracks developed very quickly because they really weren't intended to carry the load for very long.

Next chart, please. The failure next was supposed to be stopped by what's called a fail-safe strap. These are spaced every couple of feet. But it also was glued, if you will, to this skin. The glue had eroded over time. Corrosion was taking place. So when the load came zipping down to the fail-safe strap, it too broke.

Next chart, please. Indeed, all of the fail-safe straps broke between the two major bulkheads that define the boundaries of this particular failure. Fortunately, there was only one fatality, although there were a number of other injuries. The silver lining to this particular cloud is it caught the attention of the aerospace community, and since then there have been a whole series of efforts that really were stimulated by this and some subsequent events.

Next chart, please. One of the matters you all will be talking about later, I think, might be somewhat related to this chart. This was not a matter that was brand new in 1988. The first signs of it were back in 1970, and the bullets in this chart sort of trace some of that history.

Next chart, please. So in closing, two more charts. Next chart. In looking back at the life cycle management of fleets over time, there are some things that seem to serve us well, and they're highlighted here. We talked about the durability and damage tolerance analysis, the full-scale fatigue tests, tear-down inspections, updating the damage tolerance analysis with new loads data because loading environments change over time with flight vehicles, and maintaining high levels of system integrity.

Next chart, please. In closing, many fleets have flown way beyond the traditional points of retirement. In studying these fleets, each seems to have its own unique story in terms of the challenges it had and how those challenges were dealt with. We hope, we at Rand on the Aging Aircraft Team, that this quick survey of the landscape may prove of some aid to the board as you continue your important work.

Thank you.

ADM. GEHMAN: Thank you very much.

MR. ERNST: I'm hoping to see a slide here in a minute that comes up.

I want to thank you for the opportunity to talk to you a bit more about the cultural issues. Dr. Gebman and I compared slides for the first time about two hours ago, and you'll see some tie-ins to his slides that is more by coincidence in our mutual experience than by any preplanned coordination.

One of the things I want to focus on is cultural, and it goes back to part of the problems that I saw in Dr. McDonald's Shuttle Independent Assessment Team back in 1999 and some changes that I think need to be made in the aerospace industry.

Next slide, please. I also want to offer the apologies of Colonel Mike Carpenter, my counterpart in the Air Force Aging Aircraft Program, who was still stuck at Wright-Patterson. You'll see these slides we kind of do interchangeably on here. This one's a little dated, but it shows the growth of the age, the average age of our fleets over the last 10 or 12 years, most of it in the DOD side from a procurement holiday. When you're talking about aircraft reaching 20 years of age, that's an average age. You've got some like the B-52 and the KC-135, H-46, they're getting up in the late 30s.

We are in unprecedented areas in dealing with aging aircraft. It's not like we can go back and find the predecessor of the B-52 and see how it did in its 45th year. There isn't that data. As you can see from Dr. Gebman's presentation, there are a lot of complex issues. I use the phrase, "This isn't rocket science," but it really is a complex issue, an age type of rocket science in there. Even though we have a lot of very, very talented individuals working on these issues, we're kind of a one-of-one type of scenario. We're out in new areas in there.

I also want to show that the systems, even that are old, it doesn't mean they can't be effective. I think all we have to do is look to the recent aircraft performance in Operation Iraqi Freedom to see that our legacy platforms, when they're put in the hands of qualified operators and maintainers that are dedicated to their jobs, can do a tremendous job and do a great performance. But sometimes those aircraft, when they get up in age, we have new issues that we have to handle in there.

The challenge we need to do is balance when can we recapitalize. There's no idiot light that just sits here and goes, ding, "Replace this aircraft and buy new aircraft." We have to look at a variety of factors, things such as fatigue tests, tear-down inspections, load surveys, complex issues. And frankly, they aren't very sexy. When you talk about I want you to go study corrosion and rust propagation in aircraft, that's not the thing that the young kid out of school necessarily wants to focus on. So there are some challenges there.

Next slide, please. One of my other hats that I put on to cover my bald head is part of the Joint Council on Aging Aircraft. I wanted to explain a little bit about this. This was a grassroots group that got together a little less than two years ago because we all realized in the Air Force and the Navy and the Army and Coast Guard and DLA and NASA that we did not have enough resources. You can read resources as people, money, and time to be able to handle all the issues adequately but we said, you know, we're taxpayers and every April 15th I look at my tax statement and say, gee, I'd like to see if I can reduce that tax burden somehow. So we decided to cooperate and graduate and see if we could share things together and work together on certain issues in here. This group met in about August of 2001, the Joint Aeronautical Commanders group said, "Hey, what are you doing on aging? Let's get together and formally charter this group."

Next slide, please. So if you know anything about the Joint Aeronautical Commanders Group, the service three stars, at the systems command that report to the Joint Logistics Commanders group in there. They have a series of boards, and we were adopted by them and became one of their boards.

Click it again for me and bring up my next pretty picture. There's the people we have from the leadership of the different aging aircraft communities. And we are a board and what we're trying to do now is bring the attention of aging aircraft issues up to the other members of the board and to try to get things changed.

For example, training. We went around and we found out that sometimes our maintenance training wasn't up to snuff in certain areas. So we went back and said, "Hey, how does that training curriculum that was done when the S-3 that Admiral Turcotte flew was delivered in 1974, how should that change?" And we went through and looked at seeing some of those things because aging is going to change some of your core functions and logistics and engineering and supply support and those issues and our job is to bring focus to those.

Click it again for me, please. Next slide, please. What is the mission of the JCA? Twofold really. One is to identify and investigate issues. But we're not just a think tank. We're not going to put a pretty little report that says you really need to go, you know, build this or you need to do this. We're also serving as program managers that are fielding products, especially in the transition area, taking a lot of the new technologies that are out there and look really good, putting them on aircraft and making sure what application they work. That's our focus. And that's one of the biggest pitfalls we have on an aging side is taking all that really neat stuff out there, all those science fair projects, and putting them on platforms.

Next slide, please. Ironically, I sat in the airplane late last night and said what are some of the characteristics of a robust, good successful program; and you'll see a lot of similarities to what Dr. Gebman presented. The first thing we have to do is understand how all of the components, whether it be an O-ring, a structure, an ejection seat in a fighter aircraft, whatever you need, how does that age. If you look at the way we classically develop air vehicles, we spend a lot of time focusing on the development side, getting it up to initial operational capability, and then we've qualified all those issues, they're good, we just kind of do some monitoring of our data but we really don't know all the interdependencies of all those different materials and how they age as a function of time, how they age as a function of changes in environmental regulations, how the load changes, the pilots are going to fly the airplanes differently. We have mission changes on there and we now want to be able to do this or do this or drop this bomb. You can look at all the DOD airplanes over time and see the mission changes. So we have to understand how each of those subsystems are effective in the system of systems.

The next thing is monitoring our fleet usage data. You give a pilot an aircraft, and he's going to find unique ways to be able to fly that airplane in an environment, especially with new mission growths that we've got to counter. The way you do a fatigue test is you go and you estimate how many 1G, 1 1/2, 2G maneuvers, how many landings, how many takeoffs, how many pressurization cycles, and you put it all in there and you literally, you know, bend this thing like it's a piece of silly putty to see where a crack's going in. But you're guessing how that airplane is going to be used 20 and 25 years in advance. And one of the changes that we've seen is we need to go and monitor that fleet usage, collect that data, and then update that fatigue testing because, you know, I guarantee things are going to be different ten years from now, just as they were ten years ago.

You need to utilize that fleet data to go back and not just collect it in some big data morgue but go back and say: How are your original calculations? Are you using up your service life earlier? You know, the Navy went and bought some F-16s for their adversary squadrons, and we used them up in about four years because they were all doing the shooting down their watch type of stuff very quickly in there. The mission changes, the requirements change, and we have to be able to make sure our original predictions -- they're not wrong, but they've got to be validated. It's kind of like me taking my two thumbs and going like this and saying, yeah, I can figure out and calculate how I'm going to go to the moon. You've got a lot of mid-course corrections you have to do.

The last issue which was brought up before, I found it amusing to hear the previous panel talk about the daily reporting systems in PRACA. We need to collect good data, but we need to have that data resident at the subject matter expert's fingertips, not in some type of huge data base in the sky that nobody can get to. And all those elements need to be in there. It's more than just neat technology. You have to have all these elements and, folks, this ain't sexy but this is the core that allows you to manage a fleet effectively.

Next slide, please. The Joint Council on Aging Aircraft, working together, try to run their own programs and share this data together, is trying to make process recommendations and not just field issues. Microcircuit obsolescence was brought up today. What data do we need to buy in our acquisition programs to make sure that we can support the rapid changeover in technology, because we're not going to drive it in Department of Defense or NASA anymore. When we have to get with the industry and figure out what data we need, what's the best approach, that's going to require some acquisition changes, some process changes -- again, not just technology -- but yet we will take those technologies, evaluate them, and say these are the ones we need to select.

I once told a group that I was walking along the beach and picked up a pretty seashell and out ran three guys selling corrosion solutions. I mean, there literally are hundreds of technologies; and I think I broke my corrosion lead's pencil when he got up to about 84 different areas. I said let's get six out there and be successful. We like good ideas. That's what fuels the reduction of our problems with aging aircraft, but we need to also make sure that we are pushing not all of them but we are pushing the top couple of them.

We are facilitating the transition, making sure that we are prototyping them on the aircraft. We do not fly what we have not tested; and I can show you story after story after story when we did a prototype test, something else happened, either we had a sealant or we had a compound, or wash cleaning fluid that interacted and we need to be able to evaluate those issues.

Of course, we're promoting knowledge management. What is the cost of aging? Where is that big idiot light that says: "Buy more F-18EFs and retire S-3s for tankers"? Where is that point that we can make the right economical decision? And there's a paucity of data on those issues and it's kind of like everybody has their own way of calculating it and we're working with Rand, trying to get all those groups together.

So we're working together on a variety of issues from process to technology to acquisition to knowledge management type of solutions.

Next slide, please. That's what I do on my part-time job.

We've been tasked by the Aeronautical Commanders Group to try to foster a national strategy, working DOD, NASA, FAA, and industry. What do we need to do? A lot of our effort, about 80 percent of our time, is on what I call tactical initiatives, what is the best way of inspecting wire, what is the best corrosion compound, yada, yada. About 15 percent of our time or more is strategic areas. What do we need to do to handle diminishing manufacturing sources and obsolescence? About 5 percent of our time is on things like what is the right amount of sustaining engineering that we need to have on our team. How much emphasis do we need to have on our data systems? What data do we need to collect?

Next slide, please. We just recently partnered with NDIA and AIA, two industry consortiums, so that we can get feedback from industry, because I'm not going to say that I'm clairvoyant and have all the answers. I've made enough mistakes, I have nine lives based on my mistakes, but I want to get from industry that partnership of where do they think we need to change. Do we need to change our process for buying, for supporting? What amount of balance is there in the government and industry team?

Next slide. You purposely can't read this. I don't want anybody to read this because it's an early version. But we've actually gone to doing road maps where we've surveyed -- and this is from wiring -- from both a technology point of view, an acquisition, a logistics, a training, all those areas, all the different programs that are out there. When you see those pretty little red things, well, green is good, yellow is ehhh, and red is real bad. You see where we need to build a strategy, and we're trying to make sure that all of our funding and resources, they're not joint but they're at least lined up and all pointing in the same direction and we're pulling in the same way.

Next slide. What are some of the successful models of teams that we've stood up. Too often we have a hearing like this and we go in there and Congress passes a new law and we anoint a new person to be the czar of something and he comes out, or she, and puts out lots of mandates. And maybe I'm a cynic -- well, I know I'm a cynic -- maybe I'll admit it -- but that doesn't always work.

One example I want to point out is what we did with the JCAA corrosion steering group. The reason it was successful is we took the materials experts in each of the sites and married them up with the program teams, put in logistics people for publications and training, a cross-functional IDT, and said, "You guys tell us what to do." My role now becomes less of a manager and more of a barrier-removal expert. At least that's what I call myself. They call me something, other things, but we can't say those in public. So we need to build those from the bottoms up and not just create something from the top down that puts more unfunded mandates on us.

Next slide, please. Summary. I think our aging aircraft problem's a serious threat. I think it's something that requires an infusion of resources, an infusion of capital, and a national strategy to be done. At the Joint Council on Aging Aircraft, we're trying to coordinate those different areas. You can come back and judge whether we're successful or not. I think the industry cooperation is critical. We're not going to say that this is a government-only issue, but we're listening for the best possible practices. I will steal from anybody and any group and, as Winston Churchill said, he would even say a kind word for the devil in the House of Commons if he would help him against the Nazis. I'll even partner with the devil if he'll help us with our aging aircraft strategies, and I think we need a strategic process that requires that collaboration. And the last time I checked, we need NASA's involvement in there. Their involvement's increasing, but we need to remind NASA that one of those A's stands for aeronautics and we need them and their expertise. That's all I have, thank you.

ADM. GEHMAN: Thank you very much.

MR. WALLACE: I think the focus has been mostly on structures, although Mr. Ernst did talk about avionics and wiring. I know that in the civil sector where I came from, after Aloha we launched, of course, a very extensive aging airplane program. I feel like the structural part, at least perhaps in the less challenging field of civil aircraft operations, is reasonably well handled or at least that we currently feel that the aging systems challenge is greater -- and wiring in particular.

I wondered if you have any sort of conceptual thoughts on aging systems, wiring, and whether or not there's a different approach. You talked about the need for accurate reporting and that sort of thing. But in many respects those seem to be some of the more difficult challenges.

MR. ERNST: You could pick any subsystem that you want and the process that was set in place -- from analysis, technology, investments, prototyping, data collection -- that Dr. Gebman showed, needs to be followed through. And I believe that the FAA's wiring non-structural program follows some of those classic issues. In having been part of it and actually teaming with the FAA on some of those areas in wiring, you can see that it follows the same type of elements in there.

Wiring is a major issue. We made some mistakes when we selected the wire types in some of our vehicles in the Eighties. We did some qualification testing on it, and it had some very adverse characteristics. I'm trying to be nice. We now need to make sure that we're developing things that are not just saying, yeah, throw that one away, build all new aircraft, but can inspect it to make sure the bad characteristics, i.e., the arc tracking that was associated with aromatic polyimide insulation is not prevalent. But all those elements require smart people working together and the success story is -- I'm not sure you're aware of this, but the FAA has spent a fair amount of money really investigating the different types of inspection technologies, whether it be frequency domain reflectometry, time domain reflectometry, scanning wave ratio, and a whole bunch of things that make my brain hurt. And the Navy is actually doing some of the transition and manufacturing of those systems and buying and fielding them initially in our depots and our organizational-level troops. The Air Force is doing the same thing. We're working together on these issues and eventually we're going to get products that the commercial industry can take back in on. So you see the FAA do the early R&D, the Navy and the Air Force do some of the tech transition of prototyping and measuring and quantifying what percentage of wire chafing is now degraded that you have to replace it -- what are those red, yellow, green thresholds -- and then the commercial aircraft industry can pick up and procure those things without having to develop all those issues. The process is pretty much the same, but we need to make sure we have a robust area in all those issues. Wiring is in pretty good shape. Corrosion in structures is in pretty good shape. If you want to talk about helicopters and all those rotating machinery, it's a pocket of poverty.

MR. WALLACE: Well, following up on one of your points about the type of detailed inspections required, I mean, can you speak to the issue which I know was very much discussed sort of in the post-Aloha inspection implementations of just sort of numbingly monotonous maintenance tasks and the human factors associated with that?

MR. ERNST: I like the choice of words. One thing that when I got a chance to sit inside or look at the internal bay, cargo bay of the Columbia in '99 was at Palmdale and there were wiring issues, the primary method of inspection of wiring was the Mark 1 motto, eyeball in a mirror. And I sat there with the Air Force wiring technologist on a team, George Zelinski, a very detailed, knowledgeable individual, and I tried to see if I could find those myself because I'm an engineer. I've been around wiring enough times. I couldn't see those issues that they were required to pick up. And we had a system then that was mind-numbing, that required a lot of expertise and experience and there's technology out there that can do that better and, more importantly, can do that as a precursor to failure. You don't have to wait until you see insulation to say, yes, it's through. What we need to get to is a prognostic system where we can check non-intrusively, not pulling bundles apart, but we can check those wiring bundles and say aha, I'm starting to get some breakdown whether it be due to hydrolysis, whether it be to chafe, vibration, wear, gremlins, whatever, and say now I've got 80 percent through. At 20 percent I now ought to go on a scheduled maintenance procedure and put that together. And that's where we need to go and that's part of a holistic wiring strategy that I believe we have right now. We just have to get it funded and implemented.

MR. HUBBARD: I have a question for Mr. Ernst. You made a passing reference to NASA's PRACA problem-reporting system. Could you characterize for us what you think are the best characteristics of the kind of accurate problem-reporting system you referred to in your slides?

MR. ERNST: A system has to be real-time. It cannot be a system that takes 18 months to collect data. It's got to be something that is easy for the operator or maintainer to input. The Navy system, years ago, was a paper system where the poor guy, after working a lot of hours fixing the aircraft, would fill out the paperwork and, because of that, there were inaccuracies once in a while -- not in Admiral Turcotte's squadron, of course -- but there were inaccuracies every once in a while we went back and looked at those types of things.

MR. WALLACE: Are you trying to sell him something?

MR. ERNST: I could tell stories, but I won't.

ADM. TURCOTTE: We go back.

MR. ERNST: It has to be a system that is easy, simple, robust, and it has to be something that tells you something about the failure, not bug-in-the-cockpit type of issue and then say, "I removed the bug." You need to go in there and say, "Hey, I had some failure issue," and it needs to tie back in from the operator what his perception of the failure, because he's going to describe it, "Hey, this didn't work." He's not going to say that you had a corrosion on Pin 5 of your connector which stopped your data flow. That's going to be the engineer, and it has to tie those systems together with some software that can easily do some trend analysis. And another point we have to do is we have to keep the data long enough to do trend analysis. And there has been a push to throw systems and data away after 18 months and we need to go back five or six or seven or eight or ten years to get a statistical sample size. So those are some of the characteristics, and we're working to get some of those systems implemented now.

MR. HUBBARD: On the report that my predecessor Harry McDonald did, one of the shortcomings that he found was that the PRACA system did not appear to have all of these characteristics you just mentioned.

MR. ERNST: Harry called it the data morgue.

MR. HUBBARD: Data morgue. Yes. One of the things that you commented on just a few minutes ago was getting the material to the subject matter experts at their fingertips. Can you expand on that a little bit?

MR. ERNST: Sure. Let's switch to an avionics box failure. We need to not only have it so that a data expert who knows the system can write trend reports but the information if we get a failure back, let's say, on an INS system that failed, that individual who's cognizant of that system needs to go in there and say, "Have I had other failures on this system? Can I find some trending? Is it just recently or periodic? Can I go in and find out if memory chips or whatever type of chip is failing in other systems?" He needs to be able to do that research, that forensic science at his computer terminal and a lot of times our data systems will give us great reports on how many maintenance manhours we spent, three months late. And when we get a mishap in, when we get a box that's been failed, we need to understand and have that information right there at our fingertips.

MR. HUBBARD: It would be as if you only got a report on your checking account every three or four months.

MR. ERNST: Yes, sir.

MR. HUBBARD: Thank you.

ADM. GEHMAN: Mr. Gebman, in one of your viewgraphs that you presented on the heavy maintenance work days per depot for KC-135s and also in the heavy maintenance workload ratio which showed how much depot-level maintenance is required, how it's grown over the years, in your experience -- and I'll ask both of you this -- is that an accurate indicator that there's something else working below the system that you need to go look at? Is just keeping track of how much depot-level maintenance is required and how it's growing, how does that relate to characterization of aging?

DR. GEBMAN: Excellent question.

ADM. GEHMAN: Or is it just interesting?

DR. GEBMAN: Excellent question. We have studied now all of the Air Force's fleets and have compiled the statistics for, in particular, the labor hour growth over time; and it seems that once you get beyond 15 years, you're almost certainly facing a future of climbing work to be done -- some fleets that will start a bit sooner, the fighters tend to start sooner, their lives being somewhat shorter than the larger aircraft. It just seems to be a feature of aging. It might well be somewhat analogous to people. In the older years, we find ourselves going to the doctors somewhat more often than in our teenage years.

So if you want to have a sense of the age of a fleet, one measure that you might look at is, well, how is the maintenance workload changing over time. And when you see that steep part of the curve, like the presidential transport, the old 707 known as the VC-137 in Air Force nomenclature, that one literally exploded over a couple-year period and those airplanes are no longer with us.

So it's certainly something to watch. We've tried regression analysis, various statistical methods to try to correlate the rate of rise, the characteristics of fleets. We're making some progress in that area, but this is an area where there's a lot that's not known.

MR. ERNST: You want to mention the cost-of-aging study?

ADM. GEHMAN: Go ahead.

MR. ERNST: One of the issues is I had seen the Rand data almost when I started in the aging aircraft program about four years ago and we've shared back and forth and just recently the Joint Aeronautical Commanders Group Aviation Logistics Board has kicked off an effort that we're part of to look at what are these factors, can we translate the KC-135 experience to other Navy aircraft and other Air Force and Army helos and try to understand what are those factors so we can get a better idea of what's causing it and what the trend lines are. Just having information that says my cost is going up is not sufficient to be able to correct the problem. You need to then drill down and say, okay, but why. You know, I think on the KC-135 they have a pretty good idea of that. But that's what you need to do is not just look and say, yes, it's going up by 7 percent but you need to understand why is it going up 7 percent and what can you do to try to mitigate that curve.

ADM. GEHMAN: So my understanding is that, unlike the Dow Jones Industrial Average, the fact that older aircraft require more maintenance is not remarkable in and of itself and is not an indicator that anything's breaking or anything's going wrong. You've got to have much, much better indices at the system, subsystem component level in order to determine it.

MR. ERNST: And it's not just age. I'll give you an example. We were talking this cost of aging. I don't remember the numbers off the top of my head but one of the folks at Tinker said it's costing them X number of hours to paint a KC-135 now and it cost them a lot less ten years ago. And they said we're not adding one more ounce of paint. The problem is that you've had different changes in environmental regulations over those years, and you've got to make sure you're accounting for things properly. I mean, those environmental regulations aren't bad, but we've decided that this hurts Bambi and Flipper and those types of things and we want to take them off and it requires different steps and you've got to factor that in there. A lot of the cost growth you're seeing is due to things that are not age, either environmental or fleet usage. Yes, they're going to go up, but they may go up in a certain time to a manageable point and then where that curve breaks, that's what we have to figure out.

DR. GEBMAN: I'd just like to hasten to add that Bob is absolutely right. You need to look at the underlying mechanism. If the workload is climbing because you now have to tend more to corrosion and you're satisfied that you're able to see the corrosion and tend to it, that's manageable. In the area of fatigue cracking, you have to be a little bit more careful. Rising workload may indicate that you're getting more and more cracks closer and closer together, and one of the very important assumptions that we make in managing fatigue cracks is that the neighborhood is healthy. So as the population density of cracks starts to get too high, you run into a situation where you might have thought you were fail-safe but, in point of fact, the neighboring structure can't carry the load.

DR. WIDNALL: I'm sort of sensitive to this issue of aging aircraft because I worked on the B-52G when I was a freshman and I worked on the KC-135 when I was a sophomore. So my friends are still out there.

What I want to talk about is composite materials. I was a little sorry that you sort of excluded that from your chart, but I'd like to get a sense from you about some of the challenges associated with these composite materials. How well do we really understand their fatigue properties? Do we really understand their properties as well as we understand metals? What about their exposure to UV radiation and high temperatures and corrosive chemicals and all those sorts of issues? And I know we're using these more and more in our aircraft fleets in general and in particular on the Shuttle. They're obviously a key part of it. And it's just not composite materials but other kinds of brittle materials, sort of what I would call nonstandard materials.

DR. GEBMAN: Thank you for asking this question because when I was thinking about what to talk about today, I really struggled with do I talk about the areas where we have depth of knowledge that might be useful to your investigation or do I talk equally across the areas even though the depth of knowledge is shallow. Clearly, with metals there's a lot that we know, especially on fatigue, and we're learning rapidly on corrosion.

In the area of composites, I think that Charlie Harris from NASA Langley at the conference earlier this month of the AIAA, American Institute of Aeronautics and Astronautics, this big gathering, 780 people, 525 papers, Charlie gave a talk about the progress in composites and he was very positive and upbeat about all the good technical work going on. And that was all appropriate. But then he shared with the group a round robin exercise where they sent problems around to people, the same problem to work on, and people came back with different answers. And then they did another exercise where they even told people what the problem was and they still came back with different answers in terms of the methods and the assessments.

So the whole area of composite materials is one that might be analogous to where we were with metals back in the 1950s. Back in the 1950s, we had the alloy-of-the-month club; and that's where the B-52 and the KC-135 came from. The young engineers were finding out better ways to do the chemistry to get strength, but they didn't have time to understand the durability, the fatigue properties, and the corrosion properties. I'm somewhat sensing that, with composites, we're still inventing cleverer ways to get strength but we don't yet understand the long-term durability characteristics. The science is far more complicated because with metals it's homogeneous, it's the same material, with composites you've got fibers and glues or resins and it's a very complex interaction to try to model and we're not good at it yet. So anything that is made of composite requires even more circumspection and attention than probably the metals.

DR. WIDNALL: I was afraid of that.

GEN. BARRY: Excellent presentations by both of you and raises a lot of questions. As you know, the board has taken a very serious approach to aging spacecraft in this what we call R&D development test, however you want to call it, environment.

A couple of comments. Your references to the Air Force, as obviously I'm familiar with, where we are older than we've ever been before. We've never been in this era in the United States Air Force -- as is the Navy. We're approaching ages where the average age of our platforms of 6,000 is 22 years old. So even within the data experience base that we have flying airplanes, we're approaching new environments.

Now, let's translate that over to spacecraft. We are entering a new era in spacecraft, with reusable vehicles in an environment of aging. We've never been there before. So we've got two parallel efforts going on that certainly can kind of cooperate and graduate, as we've seen evidenced by the Navy and the Air Force here.

I've got a couple of quick questions and then a rather larger one. First question is: Is NASA involved in any of this Joint Council on Aircraft Aging, as far as you know?

MR. ERNST: Yes, sir. NASA has been involved in the aging aircraft effort since Aloha, prior to me being in it. The efforts at Langley in structures and corrosion NDI have been solid. Just recently, Christmas timeframe, before Columbia, they said, hey, we recognize we need to help you in that national strategy; and they're getting more involved. We need even more. I need to fill in gaps.

GEN. BARRY: On your side as well as the space side? I mean, are they translating lessons learned to both aero and space?

MR. ERNST: Yes. I'm not going to tell you it's even and homogeneous throughout, but I know that in wiring, the Shuttle folks at Kennedy are in lockstep with my guys and the FAA and I know the aerospace side and structures are working real well together. We're trying to see where the gaps are and plug them in there. We need more involvement, but they have been involved.

GEN. BARRY: All right. Let me ask this. Two things. Let's just talk about corrosion and let's talk about fatigue cracking that, Jean, you mentioned earlier. Right now we have capabilities within our aircraft to do stress-testing that you mentioned as an example. We have programs that are not only based in the United States -- Australia has an excellent one on how do this. I think we all recognize that who are in the industry. What can we do insofar as spacecraft are concerned because obviously they are larger and we translate that to our larger aircraft insofar as dynamic testing is concerned, because I don't think it's unfair to say that managing aging spacecraft in NASA, for the large part, is done by inspection. So how do we translate that, what we've learned in aircraft, over into NASA as a possible recommendation?

MR. ERNST: I think you need to break it down into the subsystem component areas. For example, we had this discussion on the McDonald's team three years ago now, on the SIAT team on wiring, where we had totally different environments but we could take the Air Force and Navy's experience with aromatic polyimide insulation and say here's what we saw under these load conditions. Now, under a probably higher vibration, higher thermal but shorter duration environment, how is that going to translate? We know how that fatigue, so to speak, environment can translate and run a new model to see what it should do with the Shuttle program.

That's the kind of transformation that could be done, but only if you know how each of those subsystems and the materials of those subsystems is going to behave as a function of time and age and environment over a number of years. The problem is a lot of times we don't know that information. So we know how it works here, we know the loads are different, but we don't know how the age is going to translate as those factors are translated, if that makes sense to you. I don't think it's hard to do that, but you have to invest in some age-related studies and that's not necessarily the top of the list.

GEN. BARRY: One of the concerns we have is to be able to analyze how the Orbiters have been shaken, rattled, and rolled over these many years, especially when we take into consideration that this was a spacecraft that was designed to be flown 100 times in ten years and now we're multiple years, decades past that and we are still only at the 20s and 30s. A question then is, you know, how do we maybe translate some of the lessons learned on how the spacecraft are flown within spec but, you know, after a while, get some kind of stress loads on them that can be accumulated over time and measured. Now, translate, if you could, the lessons learned that we've developed on aircraft that might be able to be translated over to NASA.

DR. GEBMAN: Could I have Chart No. 24, please.

MR. ERNST: You guys are going to learn this chart, because he wanted to show this to you.

DR. WIDNALL: He's ready for you.

DR. GEBMAN: This is a really tough question. Obviously, with the Shuttle we don't have the luxury of a full-scale fatigue test. Obviously doing a tear-down, if this was an aircraft fleet and when we had hundreds or even tens, we'd consider taking the oldest one and tear it apart and see what ails it and then use that to guide future work. When you're down to three, that's not an option.

So then you ask yourself, well, what might we do? And when you look at this diagram, on the top row, the matter of force tracking data and loads analysis, there may be some things you could do in terms of assuring that NASA has developed all of the effort that it can, evaluating the strain gauge recordings and pressure recordings from prior flights, and that you really have as excellent a record, historically, of the loads that have been imposed on the structure as you can possibly get.

The next thing you then could consider doing is, given the best loads data, to go back and, using more current finite element analysis methods which have improved greatly over the decades, to go in and do some spot checks on your stress computations to make sure that you've got the best that we can do in terms of estimating stresses from the given loads and then take it the next step and go in for the fatigue part to check on the crack growth calculations, the fracture toughness issues, and to make sure that the engineering community has really been resourced and tasked to do everything that we can to understand the health analytically of the fleet.

Then the final thing you might consider doing, from the debris that you do have, in effect, you have already a partial tear-down circumstance and to go in there at some point and literally take apart that which is still connected together and really check for like adhesion on honeycomb, how is that, that waffle still adhering to the face plates, and just get as much mileage as you can out of your debris in terms of understanding what the health of the remaining fleet may be.

MR. ERNST: Slice up your poles, your joints, rivet holes, things like that. That's what we do routinely.

To follow on with the chart that Dr. Gebman put up, you'll notice a couple of things. One, do a mid-life assessment of the loads. You know, the Columbia originally was kind of a flight-test bird and I believe had some several hundred pounds of instrumentation and sensors in there to measure its fleet loads. To give you an example, the P-3 and S-3 program just recently completed mid-life fatigue testing at Lockheed, and we found drastic changes to both loads from what they were anticipating. The maneuvers were a little different. The theoretical issues, the early introduction issues slowly change over time. You know, it's like boiling a pot of water. It doesn't boil all at once. And I think you need to go back and really do those load surveys.

You also need to do some type of tear-down. You can't cut up, you know, the Atlantis and make it a series of razor blades and fractographic analysis and stuff; but the Columbia, when they had wiring problems in '99, NASA did go and remove certain wire segments. You can go in without cutting the whole thing up and remove certain panels, remove tiles to see adhesion, remove subsystems. When a part's going through an overhaul, take this part on overhaul and do those types of things. So there are things that you can do; but again, you've got to have a proper program to get that environment and see how we're doing.

The S-3 example in fatigue tests, we had 12 points that we considered life-limiting on the aircraft. Four of those they knew in the original fatigue tests and the odds were out of there. We found an additional eight points that were due to the loads, and due to the tear-downs that we saw microscopic cracks. We were able to go in and cold-work fastener holes in that aircraft and give it fatigue life back. Real simple operation, real cheap, and not have the 305-inch wing cracks we had in the P-3. So you're able to do some of those things if you invest in the time and the resource and have a robust program.

DR. GEBMAN: If I might, I'd just like to follow up. Could I have Chart No. 7, please. There's an important aspect that I neglected in my answer, and that is that we're dealing with a spacecraft. And I apologize. Obviously with something like the Shuttle, you have thermodynamics acting as well as structural dynamics; and in addition to getting a solid characterization of the historical loads, you also want to get a solid characterization of the historical thermodynamic exposure because -- take a spar cap, any one of those four spar caps that are identified with the arrows. If, in the course of the history of a particular spar cap, it has been exposed to temperatures different than the other spar caps, then the loads in that part of the structure are going to be different by virtue of the thermal expansion of the material. So this is a very complex thermal as well as structural dynamic circumstance.

ADM. GEHMAN: Let me follow up on that before I call on another board member. Do I understand that you are suggesting that it's useful in the study of aging aircraft to establish some measurements of what I would call stress cycles or something like that? We understand age. We understand landings and takeoffs. But there are other events which cyclically stress the aircraft, particularly in the case of the Shuttle. And it's useful to keep track of those, in addition to the obvious ones like landings and takeoffs and how many months, hours and all those kind of obvious things.

DR. GEBMAN: These things with aircraft are tracked routinely. Exceedance curves are developed which are a statistical way of representing even the small variations.

My most recent comment suggests that we should also construct a thermal exceedance spectrum, as best we can from the historical data, so that to the extent that we've got differential thermal expansion of structure going on, we can factor that into the loads that the members receive.

You see, there's two load levels. One is the aerodynamic load and the inertial loads applied to the gross structure. The other issue of load is, for a particular structural member, what load does it see over its lifetime; and that can be driven by thermal expansion issues, just as it can be driven by the aerodynamics. And given the historical records of the temperatures, the engineers should be able to construct and may already have done thermal exceedance curves to go along with load exceedance curves.

MR. ERNST: I think you need to look at every environmental factor and see if there is a similar type of a correlation in there. We've done a good job of fatigue tracking. We're tracking a lot more parts than we used to. The models are a hundred times more detailed than they used to be. We can calculate things a lot finer, but I think you need to look at all the different loads in environments that any vehicle goes on and say, okay, what's changing, what's the effect of that over time.

ADM. TURCOTTE: For both. Kind of the 3Cs in aging -- you know, Kapton, Koropon, and corrosion -- which go back a long time in finding problems with Kapton wiring, with Koropon bonding, de-bonding, heat translation, all of those things. That's Part 1 of the question. Could you, kind of both of you, talk a little bit about major lessons learned from both fleet usage, commercial usage, and your knowledge to the extent of findings on the Shuttle, both, you know, galvanic or intergranular types of corrosion.

Part 2 question. If you were king for a day with your knowledge of the PRACA data base, what would you do to improve it?

MR. ERNST: You're going to get me in trouble. I was very nonpolitically correct about the PRACA data base in 1999. And I have not seen it since then but I think if you go back and you read the Shuttle Independent Assessment Team report, you will find that the comments of the group were less than favorable on PRACA. I'm not saying that the Navy and the Air Force and the Army's data systems are perfect, but we're taking steps in the right direction. So I really can't comment on what they're doing today. I know they made some improvements, but it was pretty abysmal back in 1999 and, I think, masked some of the issues that feed into your risk equation that we saw back then. I think that was a mistake.

As far as handling some of the materials and some of the issues with Kapton, aromatic polyimide insulation manufactured under the Dupont trade name Kapton -- get that correct -- we didn't do a good job on establishing realistic life cycle testing for that material when it was introduced. Kapton has a lot of good properties. I don't believe I said that, but it has a lot of good properties. It's very, very tough. It has some very adverse characteristics that we never tested for. But I think you can go through several other tests and I know there's been arguments with the FAA on the flammability tests, whether that's applicable, and there's lots of different tests and we didn't do a good job of running a qualification test and an aging test that's run on a short period of time that's trying to cover 20 or 40 years. So we made some mistakes on that.

The other issue is once we had problems with the wiring insulation, I don't think we developed realistic scenarios. If you look at the cost of replacing and rewiring a whole aircraft, it's several million dollars. Well, do I really need to do it? Do I need to do it in all areas? Which platforms do I need to do first? And what we have done now is develop a bouquet of options. Whatever color of flowers you want and whatever kind of room, it goes together. Because what my wiring options on the F-14 Tomcats, which are going to be retired in the next four or five years, is totally different than what I would do on earlier production F-18s or P-3s that are going to be around a little longer. So you have to develop options based on risk so that you can do things quickly, cheaply, easily, and get it done and not just give one option is all.

So I think two issues. One, we didn't do a good qualification testing and we need to continue, just like the life cycle testing, just like the fatigue tracking where you update it and you get better; and the second issue is we didn't develop any options.

DR. GEBMAN: On the matter of wiring, the Air Force in the case of the KC-135 embarked on a major rewiring program about five years ago; and that is going to probably continue for the next four to five years, at which point they will have substantially replaced the wiring on the 135. The basis for this was an accumulation of maintenance actions that was becoming increasingly costly to exercise and a concern for flight safety, and those two factors together seemed to have driven the train on that fleet.

Unfortunately, our ability to predict life, we don't have the engineering tools that we have with fatigue cracks, either with composites yet or, for sure, with wiring, which makes those areas very difficult to feel comfortable about with an aging fleet.

ADM. GEHMAN: How comfortable would you feel with the study of the aging characteristics of a main engine that's fueled by liquid hydrogen and burns a thousand gallons a second and produces a million pounds of thrust? How's our data base on how that baby ages?

DR. GEBMAN: Well, on my chart I did include a line that said propulsion; and it didn't get extremely high grades for data or methods or people that really understand life issues in that area. So you've hit another excellent nail squarely on the head. For those areas, going back to General Casey's comments about understanding margins and managing to margins, you really have to worry that as time goes by, you're eating into those design margins and at some point the ice becomes thinner than what you're comfortable with. And that's a technical judgment probably more than an engineering calculation.

MR. ERNST: Follow up. One of the successful programs that the Air Force and the Navy has is on aircraft engines. And they've realized that you've got a lot of moving parts, a lot of high temperatures, a lot of complex interactions in there. And they have what they call CIP, Component Improvement Program, where they go back in and they test and they see where their problem areas are and they incrementally try to infuse newer technologies and fixes in the early parts of the service.

Again, that's one of those that's always fighting to try to get resources adequately in there, but if we follow what the commercial industry does, we can really improve the reliability and we can have a pretty good idea and almost get to a scheduled maintenance type of inspection so we're not flying and say, yeah, lost an engine or had a shutdown but, okay, now at 7, 8 hundred hours I have an 8,000-hour interval period and know exactly what to replace. So that's another example where we've taken the methodology that Dr. Gebman talked about on structures and we've transferred over to the engines, and I think both the commercial and the military have very good experience in that being successful.

DR. GEBMAN: I certainly wouldn't quarrel with my distinguished colleague, but I would hasten to add that the commercial engine and even the military engine circumstance with aircraft is far different than the circumstance we're talking about here.

DR. LOGSDON: This is all very far away from the experience of a Washington policy wonk. So excuse me if these are really naive questions. What does the fleet size of three do to the ability to do the sorts of things that you think ought to be done?

And the second question, I think it's really for Mr. Ernst, coming out of his independent assessment experience. Is NASA routinely collecting the kind of data that would feed into the kinds of trend analyses? You know, outside of faults, PRACA and that, is there a data base that you could apply some of these methodologies to?

MR. ERNST: Well, I think all the agencies and commercial are collecting a fair amount of data.

DR. LOGSDON: On Shuttle.

MR. ERNST: On Shuttle? I mean, you look at the Navy programs and Air Force programs. We're collecting 80 percent of what we need. I still think we need to do more -- the cause of failures.

For example, if I went into the Navy's data base on wiring chafing, there is no failure code for chafe right now. What's the primary failure mode for wiring? We're fixing that, by the way, so I can say that. But that's one of the issues. I mean, we're not recording the right type of information in all cases. We're about 80 percent there.

My beef with PRACA at the time was you couldn't go in there easily and extract anything to make decisions. I at least can go into some of the services' data bases and pull some information and get a pretty good idea and then at some point I have to play archeologist or forensic scientist and go back through and do some more digging. But we're collecting about 80 percent. There need to be some other changes; and, unfortunately, data is the one thing that everybody wants to cut in the budget crunch. We don't want to pay for that data.

DR. LOGSDON: If I understand PRACA correctly, you have to have a problem or perceive a problem to even get in the system. I'm saying is the Shuttle even instrumented to capture the kind of data that you would like to have to measure various elements of its aging.

MR. ERNST: Not in all cases, but I think you can probably do some work-arounds with that and be able to check things. I mean, you don't have to do everything in flight. You can do engine run-up cycle times and check temperature rise in there, check component issues, and test stands. Things like that. You can capture that information if you need to.

You need the maintenance-reporting information, which PRACA primarily did. You need to trend analysis like if I get to this certain load level, this is going to impact my fatigue life. And then you need to be able to do periodic instrumentation at times. And it doesn't always mean a full-scale in-flight test. It means capturing some of the data. And that data was available. You could get that. Was it easily, readily available? No, it wasn't readily available.

DR. GEBMAN: Putting my engineering hat on relative to your data question, given that the instrumentation and wiring in the Shuttle and the systems were designed in an earlier era in terms of electronics, it might well be worthwhile rethinking the matter of what are we interested in observing during future flights in order that we might create a more complete record of environment and loads so that we can better manage the remaining lives of the fleet.

MR. ERNST: Health management, health monitoring for the system.

DR. GEBMAN: And regarding your observation of the number three, what does it mean to have three in a fleet? From an operational perspective, one of the early lessons I learned at Rand was that whenever you visit a unit, you always expect -- and Admiral Turcotte will appreciate this -- you always expect at least the Nth airplane to be a source of supply for the others, if you're lucky. Sometimes it's more than just the Nth airplane. So if you have a fleet of three, from an operational perspective, one of the three is needed to support the operation of the remaining two. And to have an operating fleet with just two means that you only have one backup and that's very thin.

MR. ERNST: And I think it makes your correlation. A lot of times when you have how many hundred F-15s and F-16s, you can start looking at the gross number of failures and say aha, I need to look at something. When you have three, you can't rely on that. You have to take a little bit different systems approach to be able to capture your data.

The Navy flies some type model series, you know, that are 12. Twelve EP-3s. And each one of them is a slightly different configuration. But you can capture that information. It just requires a little different approach, and sometimes it's not as robust, predictive, leading edge because you don't have that significant sample size.

MR. WALLACE: Were you suggesting, Dr. Gebman, that sort of the fleet leader concept; or were you suggesting cannibalizing parts? I wasn't entirely clear.

DR. GEBMAN: No matter how good your supply system is in terms of providing parts, you always end up in a circumstance where you have a first-time demand for a part and the last airplane of the unit then becomes the offer of that replacement part. I think that if you talk to the NASA folks regarding the matter that's referred to commonly as cannibalization, it's borrowing a part from one aircraft or spacecraft in order to be able to launch one that's scheduled to go.

MR. WALLACE: Another question. This is jumping subjects a bit. Should the goal of an aging aircraft program grow beyond maintaining the aircraft to be as good as new? What I mean by that is: Should it meld in with sort of obsolescence issues, issues where the technology has simply gotten to be so far behind the state of the art that it either makes sense for economic or safety reasons to upgrade or even reasons of simply maintainability?

DR. GEBMAN: You're raising the issue of replacement, fleet replacement; and we have struggled at Rand with the Air Force long and hard on that matter because, for example, the tanker fleet. It's a very important fleet. Without the tankers, the Air Force doesn't go places. They don't have aircraft carriers to carry their airplanes. So they're very dependent on their tankers; and to have almost all of your tanker fleet wrapped up in one type of aircraft that's 40 years old now and to be planning to do so for another 40 really raises questions.

The first thing we looked at, well, is there a case on economic grounds for replacing the fleet. There was an economic service life study done and it shows rising costs, but it doesn't show the rising cost by themselves being a sufficient basis for justifying a new fleet, whereupon then you start asking questions along the line of obsolescence issues, foregone capability improvements that you can't have without substantial investment in an aging fleet. So this whole question about when is it wise to replace a fleet is one for which we still don't have a good methodology for dealing with.

MR. WALLACE: I really didn't intend to ask that question about replacement. Well, it was a good answer. But about replacing the fleet as opposed to simply upgrading, particularly, I mean, fleet replacement, you know, lots of smart bean-counters with spreadsheets do that for the civil aircraft industry but I think there's a whole different set of different issues with next-generation spacecraft. My question really is more about upgrades.

MR. ERNST: To address that -- and you picked on obsolescence. When you get to the microcircuit obsolescence issue, which has become a science fair, pet rock project of mine over the last 10 or 12 years, there are lots of different options and right now we have some system incentivized to just find this chip to put in this box in a lot of cases. We found about a third of the time that doesn't make sense because not only is that part obsolete but the three around it are going terminal and the whole board's wearing out because we keep replacing it so many times because of poor reliability. So it's probably better at that time to take the whole thing, take the cards out, and make it a lobster trap somewhere and then put a new system in there. That really happens about a third of the time. But we need to again, I think, balance some of the different pots and stovepipes of money that are available, especially in DOD, to be able to optimize those issues and have the best understanding of the age effects, where they're going to be two years from now, because I may make a replacement today and I've got three more downstream. I need to look where I'm going to be three years from now and say this is time to replace this 1988 Tercel that I had with 189,000 miles and go buy something new because this is just the tip of the iceberg. And I don't think we're doing a real good job of that but's one of the challenges of not just maintaining status quo but looking and saying what capabilities, what mission growth areas, where am I going in some reliability issues and balance all of those into like a triangle of a decision matrix.

DR. GEBMAN: There's a fleet that we're looking at now that has the potential for receiving an upgrade to its aviation electronics to give it capabilities to continue its military relevance. And there are also a series of mods being considered to upgrade the engine so that its flight safety features remain appropriate. And similarly with the airframe. And as we're going through the arithmetic on this particular fleet, one of the things that we're seeing is that by the time you're done making whichever of the three mods or all three of them to the fleet, the years remaining becomes very significant to your choice. And when you go to the operator and you ask the operator, well, how long do you want to retain this fleet, well, they're really not sure. So this question is almost as difficult as the fleet replacement question.

MR. ERNST: And you look at the mission changes in the Department of Defense in the last couple of years where we've gone from a Cold War scenario to more of a small conflict and now global war on terrorism and it changes. We have planes that, to pick on Admiral Turcotte's S-3, that were designed to hunt subs that were doing surveillance and tanking and dropping weapons and doing, you know, partridge in a pear tree and everything else. And you need to look at those mission changes as a function of age too and say, you know, I may be able to keep this aircraft doing what it did five years ago but you know I need to replace it. I need to go over here. And we don't always balance those issues.

I know the Air Force is really trying to look at that decision and set up a fleet viability board to weigh the aging factors in these mission scenarios. I'm monitoring that for the Navy to see what they do; and then after they get all the kinks worked out, we'll steal it. But that's kind of the approach. I think that answers it that it's not a simple answer but that's what needs to be looked at. I think the Shuttle has the same issue: Where does it need to be ten years from now?

MR. HUBBARD: I heard one of you mention or whisper the term "vehicle health monitoring," I think. The notion of a fleet of three. I'd just like you to think out loud for a minute or two about how vehicle health monitoring would apply in this case along three lines. One, what would a systems approach be to that, given that we have a fleet of three? Second, realtime versus recorded measurements? Third, what other measurements could you imagine? I mean, we've got a Thermal Protection System, for example, that is pretty unique to the Orbiter versus the military aircraft you mentioned. We've got pressure, strain, and temperature. Can you imagine, in this kind of systematic approach to vehicle health monitoring, what one might do?

MR. ERNST: Let me answer in reverse order. I don't want to bad-mouth technology. And I've talked about some cultural issues but there's some real technology advancements. I know some of the DOE labs have now started looking at electronic signature analysis for failures in motors, predicting when motors are going to fail. There are all kinds of things. I mean, you can literally go around to the different areas and find better ways that people can get precursors to failures if they measure data and give you good information. That would help us understand from an overhaul interview, it would let us know if you had a degraded flight mode issue so that we're not having, yes, that system failed, we have to do something else. It would really help you manage your redundancy a lot better, too. So there are a lot of new technologies beyond the strain gauges that I learned about in college that need to do.

I think the real-time versus recorded is something you need to use a system engineering approach in analyzing. There are certain oil analysis systems that I remember we had a vapor cycle system and by the time you got oil in the filter, you had basically eaten the whole system; it was too late. So putting an oil analysis system that you measure it every ten hours wasn't doing any good. It needed to be real-time.

Not everything needs to be real-time and any information at all, whether it be on one unit or on three units, is a lot more than no information and I think that having some health monitoring systems on any fleet -- Shuttle, the F-18, the S-3, or whatever, F-15 -- gives you information if you use a good systems engineering approach, not just collect data for data's sake but see what are you trying to do with the data and then drive what you need to collect to get data or what technology best does that, I think, is helpful.

DR. GEBMAN: I would like to speak both as a proponent and also share a word of caution. The engineering in me would prompt me to want to put strain gauges and instrumentation in many places. Probably too many. There's a trade-off between the disease and the cure, and it's possible to overdo a good thing. We need to remember that, with this instrumentation, comes wires; and we've already been talking about the vulnerability that wiring can introduce into the system. So what I would think might be helpful is to try to understand what are the critical issues that we're concerned about or we should be concerned about and then ask, for those critical issues, what initially at least modest amount of additional instrumentation might be appropriate and try to really focus on the core vulnerabilities and not to go too quickly too far overboard.

MR. ERNST: We can't be kids in the candy shop. I agree.

ADM. GEHMAN: Thank you, sir. I'm going to ask the last question myself; and, hopefully, it's a brief one. I think probably, Mr. Gebman, your Chart No. 3 answers this question; but I want to allow us to listen to it for a second. Would you list the aircraft aging areas of examination as to which of them appear to be mature technologies and which of them appear to be not so mature? Obviously the detection of corrosion, of course, is obviously a big one and I suspect we probably know a lot about that.

DR. GEBMAN: Probably the quickest answer to the question would be to focus on the first column and the last three columns. In the last three columns, we have my subjective assessment of where we stand in terms of data, methods, and people. The metals area for structure, we're in very good comparative shape to the others.

In corrosion, our data and our methods are still somewhat embryonic but now, thanks to the various laboratories really engaging the last several years in a more aggressive way, we're building a core of people that are knowledgeable in the area.

The business of adhesion, we haven't paid much attention to it. And my sense is that our data and methods are below low and even the number of people really knowledgeable in that area is not great.

Moving down to the composites, there's a lot of people out there. There's a fair number of people doing excellent, promising research; but the fruits of that research in terms of data and methods is still forthcoming.

In the area of propulsion, the general area strikes me, especially when we're thinking about Shuttle types of applications, as not particularly high. The whole area of high-cycle fatigue is still a challenge for the engine community, even for commercial aircraft.

Then the "Other" category. This is the one that worries me most because it's oftentimes the one that's not getting the attention that's the one that bites you the hardest. Functional systems, pumps, motors. The TWA 800 killed more people than metal structures in recent times, and that may well have been down in this "Other" category, either the wiring or the functional systems.

So as the board moves forward with its good work, attention to all of the technical areas. And all that I've tried to accomplish here today is to bring forward that there are some areas where the aging aircraft community really has depth. If that proves to be relevant or of interest, the community is certainly prepared to help. In the others, it's going to be more challenging.

ADM. GEHMAN: Well, thank you very much. On behalf of the board, I would like to express our appreciation for your attendance here today and your complete and helpful replies to our questions and the information that you've given. You're obviously great experts and we've learned a lot and we hope that we can apply it to this problem. We appreciate your attendance.

We're going to take about a ten-minute break while we seat the next panel, and we'll be right back.

(Recess taken)

ADM. GEHMAN: All right. We're ready to begin our last session of the day.

It's a privilege for the board to recognize Dr. Diane Vaughan from Boston College. Dr. Vaughan has written an influential and well-read book on the Challenger accident. We are continuing our look into the business of risk assessment and risk management. This is one of the classic studies on the Challenger accident. Most of the board members have at least read parts of your book, Professor Vaughan; and we're delighted to have you here.

DR. VAUGHAN: Thank you.

GEN. BARRY: And we're ready for a test.

ADM. GEHMAN: I would like you to please, if you would, before we get started, introduce yourself by telling us a little bit about your background; and then if you would like to say something to get us started, we would be delighted to hear it.

DIANE VAUGHAN testified as follows:

DR. VAUGHAN: Thank you. I'm a sociologist. I received all of my education at Ohio State University, getting my Ph.D. in 1979. After that, I had a post-doctoral fellowship at Yale; and I began teaching at Boston College in 1984, where I am currently a full professor.

My research interest is organizations. I'm, in particular, interested in how organizational systems affect the actions and understandings of the people who work in them. So it's what we call, in my trade, making the macro-micro connection, how do you understand the importance and effect of being in an organization as it guides the actions of individuals. My research methods are typically what we would call qualitative, which are interviews, archival documents, and ethnographic observations. So using these methods, I have written three books, the last of which was The Challenger Launch Decision, which was published in 1996.

ADM. GEHMAN: Thank you very much. You may proceed.

DR. VAUGHAN: All right. I want to start from the point of view of Sally Ride's now famous statement. She hears echoes of Challenger in Columbia. The question is: What do these echoes mean? When you have problems that persist over time, in spite of the change in personnel, it means that something systematic is going on in the organizations where these people work.

This is an O-ring -- not The O-ring, but it is an O-ring. I want to make the point that, in fact, Challenger was not just an O-ring failure but it was the failure of the organizational system. What the echoes mean is that the problems that existed at the time of Challenger have not been fixed, despite all the resources and all the insights the presidential commission found, that these problems have still remained.

So one of the things that we need to think about is when an organizational system creates problems, the strategies to make the changes have to, in fact, address the causes in the system. If you don't do that, then the problems repeat; and I believe that's what's happened with Columbia.

What I would like to do is begin by looking at what were the causes of Challenger, based on my research, to point out how the organizational system affected the decisions that were made, and then make some comparisons with Columbia and then think about what it might mean, taking that information, to make changes in an organization to reduce the probability that this happens.

One of the things that we have learned in organizational --

ADM. GEHMAN: Excuse me for interrupting. If I may ask a question while we're still on this subject. On your first viewgraph, the first bullet, you said when you find patterns that repeat over time despite changes in personnel, something systemic is going on in the organization. There are no negative connotations in that sentence. You didn't say something wrong is going on in the organization. I assume the obverse is also true. If patterns repeat over time and you keep changing people and you keep getting good results, then it's the system --

DR. VAUGHAN: The system is working. Right. It's the fact that there is a bad outcome that we're looking at here. Thank you.

ADM. GEHMAN: Thank you. Sorry for the interruption.

DR. VAUGHAN: I wanted to begin and go back over just really briefly what happened in Challenger. First, the presidential commission reported that there was a controversial eve-of-the-launch teleconference during which worried engineers at Morton Thiokol, the solid rocket booster contractor in Utah, had then objected to the launch, given that there was going to be an unprecedented cold temperature at launch time the next day.

Marshall management, however, went ahead and launched, overriding the protests of these engineers. Not only did the commission discover that but also the fact that they discovered that NASA had been flying with known flaws on the Solid Rocket Boosters' O-rings since early in the Shuttle program, that these flaws were known, and known to everybody within the NASA system.

May I have the next slide, please. What happened was what I called an incremental descent into poor judgment. This was a design from which there were predicted to be no problems with the O-rings, no damage. An anomaly occurred early in flights of the Shuttle, and they accepted that anomaly and then they continued to have anomalies and accepted more and more. This was not just blind acceptance, but they analyzed them thoroughly and on the basis of their engineering analysis and their tests, they concluded that it was not a threat to flight safety. It's important to understand, then, that this history was a background in which they made decisions on the eve of the teleconference. And that was one more step in which they again gradually had expanded the bounds of acceptable risk.

Next slide, please. One of the things that's critical with Challenger, and also now, is the fact that we tend to look at bad outcomes and look backwards and we're able then to put in line all of the bad decisions and apparently foolish moves that led up to it. It becomes very important to look at the problems as they were unfolding and how people saw them at the time and try to reconstruct their definition of the situation based on the information they had when they made their choices.

Next slide, please. The Challenger launch decision was, in fact, a failure of the organizational system. And I hope, by going through the explanation, it will show why it was not groupthink; it was not incompetent engineers, unethical or incompetent managers.

Next slide, please. So what happened? Richard Feynman called it Russian roulette, which implies that there is a knowing risk-taking going on. The result of my research, I called it something else, the normalization of deviance. And I want to use the organizational system perspective to explain how this happened.

The idea of an organizational system is that there are different levels at which you have to do your investigation. So the first is the people doing the work, their interactions, and what they see; the second level is the organization itself; and the third level has to do with the environment outside the organization and the other players that affect what's going on internally.

So let's start with the bottom layer, the people doing the interaction. First, it's important to know that they were making decisions against a backdrop where problems were expected. Because the shuttle was designed to be reusable, they knew it was going to come back from outer space with damage; and so there was damage on every mission. So simply an environment like that, to have a problem is itself normal. So what to us in hindsight seemed to be clear signals of danger that should have been heeded -- that is, the number of flaws and O-ring erosion that had happened prior to Challenger -- looked different to them. The next slide will show how they looked as the problem unfolded.

What we saw as signals of danger, they saw as mixed signals. They would have a problem flight. It would be followed with a flight for which there was no problem. They would have weak signals. Something that in retrospect seemed to us to be a flight-stopper, to them was interpreted differently at the time. For example, cold, which was a problem with the Challenger flight, was not a clear problem and not a clear cause on an earlier launch. Finally, what we saw as signals of danger came to be routine. In the year before Challenger, they were having O-ring erosion on 7 out of 9 flights. At this time it became a routine signal, not a warning sign.

The next slide, please. That's what's going on on the ground floor. So the question is then how does the organizational system in which they're working affect what they're doing and how they're interpreting this information and how their decisions move forward. This is what I call the trickle-down effect. Congress and the White House were major players in making decisions, and their policy decisions affected how people were making decisions in the project.

The budget, the problem of Challenger starting out with insufficient resources, meant that the only way the program got going was by Challenger, by the Shuttle program being responsible in part for its own livelihood. That is, it would carry payloads. The number of payloads it would get paid for annually were expected to contribute to its budget.

So early on, the Space Shuttle project was converted from what during the Apollo era had been an R&D organization into a business. Contracting out and regulation both had altered the Shuttle program so that it was much more bureaucratic. There was lots of paperwork. A lot of people who had been in pure engineering positions were reversed in the sense that they became more administrative. They were put in oversight positions, and they had a lot of desk work to do.

Finally, when the program was announced, it was announced that it would be routine to fly Shuttles into space. It would operate like a bus. So the expectation that it would be routine also had an effect in the workplace. The effect was to transform really a culture that had been pure R&D, with emphasis only on the technological discovery, into one that had to operate more like a business in that cost was a problem, production pressures were a problem.

The notion of bureaucratic accountability made the agency what some people told me was bureau-pathological. That is, there were so many rules, there were so many forms to be filled out that these kinds of tasks deflected attention from the main job of cutting-edge engineering. It wasn't that the original technical culture died but that, in fact, it was harder to follow it through with these other influences on the shuttle program.

How did these actually play out on the ground? Next slide. The original technical culture called for rigorous scientific and quantitative engineering, real solid data in the form of numbers to back up all engineering arguments; and that was still true. However, also with the original technical culture, there was a lot of deference to engineering and engineering expertise based on the opinions, valued opinions, of the people who were doing the hands-on work.

The latter was harder to achieve within a bureaucratic organization where hierarchy dominated. The schedule became a problem interfering with the decisions by compelling turn-arounds in time to meet the schedule, so that expected research on hardware problems sometimes continued past the next launch. So they were still getting more information while a new launch was in process.

It also affected them in that the engineers and managers truly followed all the rules. In the midst of a system that many people at the time said was about to come down under its own weight before Challenger, what was happening was the fact that they followed all the rules in terms of having the numbers, in terms of procedures, gave them a kind of belief that it was safe to fly. Engineering concerns had to be backed up with hard data or there couldn't be money set aside to do a correction to the program. Hunch and intuition and concern were not enough.

Next slide, please. The third part is to say, well, there was a long incubation period here. Why didn't someone notice the trend that was going on with the Solid Rocket Booster project in terms of O-ring flaws and intervene? This is where the organization's structure was at that time a problem. The safety system had been weakened. One safety unit had been completely dissolved, and staffing had been cut back. Top administrators, because of extra work in an expanding program, were no longer able to maintain what in the Apollo program was known as the dirty-hands approach -- that is, keeping in touch with the technology, the problems, and the riskiness of it.

And the anomaly tracking system, which was another way that you could get warning signs, made it very difficult for administrators to isolate serious problems. At one time under their Criticality 1 category, which is the most serious label that you can give to a technical problem, they had 978 items on it. So how, of those, do you sort out which are the most serious?

Next slide, please. With this as an outline, I'd like to move to some comparisons, the echoes that Sally Ride talked about. First, here I'm drawing analogies. I spent nine years on the Challenger book and I haven't done this on this case, and your investigation is still under way. So where I'm able easily to identify the similarities, it's harder to define the differences; and what we see now as similarities are yet to be proved. So my goal here is just to maybe point you in some ways to look, and not come to any conclusions.

First, in both circumstances, Columbia and Challenger, a crisis -- well, let's say it was a crisis of uncertainty. Circumstances happened for which they had no background experience. They came to this condition of high uncertainty with a belief in acceptable risk -- that is, based on all the Flight Readiness Review decisions that had preceded, they believed they were flying with a vehicle that did not have a problem that was related to, in Challenger, O-rings and, in Columbia, the foam problem. They believed in their own analysis. That was this background, and they had engineering reasons for believing that.

Second, in each of those cases, Challenger and Columbia, there had been an event in the recent past that had some import for their decision-making that night. For Challenger, the year before the launch, STS 51B was launched in January. The condition that the engineers on the eve of the Challenger launch were concerned about was the cold temperature, which for the next day was predicted to be at an all-time launch-time low. The STS 51B, which was launched in January of 1985, was launched where also cold temperature mattered but not on the launch pad. The cold temperature had been the three previous nights when the vehicle was sitting on the launch pad and the temperature was down 19 to 22 degrees at that time.

The foam strike in Atlantis. There had been several foam strikes preceding the Columbia launch. The Atlantis foam strike, which happened in October of 2002, was the most recent. The history in the foam strikes was that they had problems with imagery, that they couldn't see so much the location of the strikes and so on. So that was part of the history which led to the fact that that night they didn't have or that -- when they discovered the foam strike, that they didn't have good data.

For the cold temperature on 51B, there was a similar effect. At the time when they did the analysis, the engineer who went to the Cape and looked at the vehicle when it was disassembled and looked at the Solid Rocket Boosters was alarmed because he saw that in the base of the putty in the groove in which the O-rings lay, the grease was charred black like charcoal; and he believed that this was significant. But when they came forth after that with their analysis of 51B for the next Flight Readiness Review, their analysis showed them that it was still safe to fly. They had had damage of the O-ring, they had serious O-ring erosion, and they had had for the first time hot gases that had gone beyond the primary O-ring to its backup, the secondary O-ring, and their analysis told them that in a worst-case scenario, it would still work. It would still work.

Where does cold come into this? The engineer who saw the charcoaled grease had this feeling that, intuitively, this was bad. So when he argued that cold should be a serious concern, they had at that point had many things happening with O-rings. The smallest thing could cause damage. So, for example, a piece of lint in the bed of putty in which an

O-ring lay could cause erosion. Each time something different had happened. They believed that there was no generic problem because they were not having damage on every ring on every mission. Sometimes they would not have any. So that he could not prove that cold was a correlation with the O-ring damage.

They decided at that point that they should get some cold temperature data; but they didn't scramble to get it, as this engineer said. The reason they didn't was they believed it was a unique incident, that the chance of overnight temperatures of that low for three nights running in Florida was, in his words, the equivalent of having a 100-year storm two years in a row. So there was no scramble to get temperature data. They did some resiliency tests, but they did not have systematic temperature data. So in both circumstances, when the condition of high uncertainty came up for both Columbia and Challenger, they did not have a lot of supporting data, they didn't have the best data available to them and this, it turned out, mattered.

The third point is that the organization's structure interfered with good communication, and it interfered in several ways in which there seem to be parallels across cases. There were, in this case, missing signals. People who had information, if that information had been relayed up the hierarchy, might have made a difference. People in the Challenger evening teleconference were in three different locations, and they were in telephone communication but not video. People were in different locations who did not speak up, so their message didn't get across on the main teleconference line.

Why didn't they speak up? Some people felt that that was their specialization, they hadn't worked on it recently, and therefore though they had some input and they had some information, they didn't know what the most recent data was. Some people didn't speak up because it simply wasn't their specialization. Other people didn't speak up because they trusted in the hierarchy, they trusted in the key people who were running the teleconference to guide it in the right direction, they trusted the engineers at Thiokol to do the analysis. Those were some of the reasons.

One of the parallels with Columbia comes up in the accounts of the e-mails that were circulated from approximately the 21st on, worries of concerned engineers. From newspaper accounts that I've been able to conclude and the e-mails themselves, that in a sense they were marginal to the process, they had not been brought in early on, this was a conversation they were having among themselves. They were also specialized and felt that perhaps they didn't have the same information that other people had. There was a trust in the hierarchy; and, as one of them said after a press conference early in your investigation, "We didn't have the data." That is, they were concerned they didn't have any hard numbers.

One of the characteristics of the conversion from the Apollo-era culture to the Challenger-era culture was that intuition and hunch didn't carry any weight. They carried weight in everyday, daily decision-making and batting around ideas, but when it came to formal decisions like the Flight Readiness Review, it was hard data, it was numbers that were required. And in this case it was significant to me that he said we didn't have the data and therefore, not having the data, they didn't feel empowered to speak up in these e-mails and carry them upward farther.

There is evidence of production pressure in the Challenger case that I haven't seen yet in Columbia. In Challenger, there was a deadline for the engineers to make their preparation for their eve-of-the-launch teleconference engineering recommendation about the relationship between the cold temperature and O-ring erosion and what they expected, what they were recommending in terms of launch. They scrambled to put their analysis together, dividing up the work, and began faxing their charts over the telecon line without having the time to look through them, and if they had taken that time, they might have noticed ahead of time -- if they had collectively looked through them, they might have noticed ahead of time that they didn't have a strong correlational argument. So as a consequence, it was a weak argument in terms of the engineering culture at NASA. The hard numbers didn't hold together. They couldn't prove that there was a cold temperature correlation with O-ring damage.

At one point the key engineer said, "You know, I can't prove it. I just know it's away from goodness in our data base." But in that culture, that was considered an emotional argument, a subjective argument, it was not considered a strong quantitative data argument in keeping with the technical tradition at the time.

So far there isn't any evidence of engineering concerns during the history of the foam problem like there was with Challenger either. Afterwards, there had surfaced some memos in Challenger, the previous year in particular, as engineers at Thiokol were trying to get through the bureaucratic rigmarole in order to get the help they needed to try to analyze the problem; and they were working on a fix at the time.

The other point I wanted to make was about bureaucratic accountability. What was obvious with Challenger was that on the eve of the launch that the concerns of the engineers were not prioritized. It also seems to be the case in the requests for the imagery from Columbia that concerned engineers discovering the foam strike at this point described it as it was large. There was nothing in their experience like this. It was the size of a Coke cooler. This was unique. They met, a team of approximately 37 engineers, and made a request for better visuals than the ones that they had from ground camera; but somebody up the hierarchy canceled the request. In a condition of high uncertainty. One of the comments that I read in the newspaper -- and I don't claim to have all information on this -- was that the request had not gone through proper channels, which points to me the significance of rules and hierarchy over deference to technical expertise in this particular case.

There are many conclusions we can think about from this, but one of them is that in both of these situations, following the normal rules and procedures seemed to take precedence; and we know that, in fact, in conditions of uncertainty, people do follow habits and routines. However, under these circumstances where you have something without precedent, it would seem that this would be a time not for hierarchical decision-making but for a more collective, collaborative, what does everybody think, let's open the floodgates and not pull on the usual people but especially what are the concerns of our engineers and also to let up on the idea that you have to have hard data. Engineering hunches and intuitions are not what you want to launch a mission with; but when you have a problem that occurs that's a crisis and you don't have adequate information, this is a reverse of the pro-launch situation, in which engineering hunches and intuitions ought to be enough to cause concerns, without asking for hard data.

So what's to be done if it turns out in this investigation that you do, in fact, find a failure of the organizational system? Could I have the next slide, please.

Typically in the results of an accident investigation, two things happen. One is that the technical culprit is found, and a technical fix is recommended and achieved; and second, that key decision-makers are identified who had important roles where they might have prevented a bad outcome but didn't. More typically, the organizational system goes untouched. It is, in fact, more difficult to identify the flaws in the organizational system. It's harder to pin it down and it's more challenging to try to correct it. In fact, there are many people who are experts in how to build high-reliability systems and what are the problems with systems from an organizational system that might help in advice in circumstances like this.

Next slide, please. Just looking at the model that I put up earlier where we looked at the trickle-down effect, it leaves three levels at which you might target changes. First, the beauty of operator error is that it deflects attention from key policy decisions made in the past that have affected a program and affected the daily operations. Policy leaders need to be concerned and aware of their responsibility with risky systems and be aware of how their choices affect the hands-on work. They also are responsible and implicated.

Cultures, for example, are hard to change, but leaders must try to change them -- even if they weren't the ones who created them. It's important that they remain in touch with the hazards of the workplace. Whereas in the modern NASA it may be more difficult for administrators to stay in touch with the hazards of the workplace and the dirty-hands approach cannot be carried out like it was in the time of Apollo, still it's important to stay in touch with those.

For example, prior to Challenger, the Shuttle was declared as an operational system. As a result of that and the belief and the expectation it would be routine, citizens were allowed to be taken on for rides. The people at the top of the organization apparently believed that it was not a risky technology and therefore it was safe to take along ordinary citizens. The engineers who were doing the daily work did not believe that it was -- I mean, they were aware of all the problems in the system on a day-to-day basis. They were the ones who had the dirty hands. They were not the ones who made the decision to put a teacher on the Space Shuttle.

Another aspect of concern for top leaders is changes are often made in an organization's structure for budgetary reasons, for better coordination, without thinking about how that might affect the people who are having to make decisions at the bottom. What does it mean, for example, when you have an International Space Station and NASA is now dividing up the work so that there are two combined structures and projects in which decisions have to be made? How are these priorities getting sorted out? Does that affect what's going on in the program?

Contracting-out had a serious effect on the work of people making technical risk analyses. We know hospitals, when they have mergers, often let people go, and it loses the institutional memory and there are startup costs in people getting going again. These kinds of changes should not be made without looking at their implications.

Second. Please, next slide. Target culture. You can't really make assumptions about your culture. We think we understand our cultures, but they act invisibly on us, and so we cannot really identify what their effects are. In one of the comments post-Columbia concerning the e-mails, "We have a safety culture and we strongly encourage everyone to speak up at every opportunity." And I'm sure that they believe that. But when you look at the chronology of events, even in skeletal form in which I'm aware of them, the fact that these what-ifs didn't percolate up the hierarchy, the fact that the engineering requests did not get fulfilled indicates that there are some things that suppress or that are acting to suppress information.

It's also significant, I think, in terms of culture to understand the power of rules. The things that we put in organizations that do good also can have a dark side. It is really important at NASA, because of the complexity of the agency and its projects, to have rules. You couldn't run it without rules. It's impossible. But then there are times when maybe the normal rules don't apply. So how do you train people to recognize circumstances when you have to expedite matters without going through the hierarchy, and how do you empower engineers to get their requests filled?

Finally, targeting signals. Missing signals are obvious in both cases. What does it mean to try to reduce missing signals? One is to truly create a system in which engineers have more visibility, their concerns have more visibility on a formal and informal basis. Second, the safety system. The parallel with Challenger and the reduction of safety personnel is also a parallel with Columbia. When you reduce a safety system, you reduce the possibility that other people are going to be able to identify something that insiders have seen and normalized the technical deviation. And the slippery slope. When you're working in a situation where problems are expected, you have problems every day, and people are busy with daily engineering decisions, it becomes very difficult to identify and stay in touch with the big picture.

How do you identify the trend so that people are aware when they are gradually increasing the bounds of acceptable risk? It is certainly true, based on what we know about organizations and accidents in the social sciences that this is a risky system and what we know is the greater the complexity of the organization, the greater the possibility of a failure.

The same is true of organizations. Organizations are also complex systems. The greater the complexity of the organizational system, also the greater the possibility of a failure. When you have a complex organization working a complex technology, you're never going to be able to completely prevent accidents, but the idea is to be more fully aware of the connection between the two so that you can reduce the probability that a failure would occur.

That's it. Your turn.

ADM. GEHMAN: All right. Well, that's a bucket full.

Since you studied the Challenger decision so carefully, and even though we're talking about Columbia here, let me ask a Challenger question, even though it's loaded because it has Columbia implications. Several things you said struck me, and they're related to each other. One is that you can't change the behavior unless you change the organization. You can change the people, but you're going to get the same outcome if the organization doesn't change. Yet in another place up there, you said beware of changing organizations, because of the law of unintended consequences. You've got to be really careful when you change organizations.

What do you make of the post-Challenger organizational changes that took place, particularly in the area of more centralization and program management oversight? What do you make of all of that?

DR. VAUGHAN: The changes that I am most familiar with are the ones related to launch decisions. That is that immediately following, they put an astronaut, former astronaut in charge of the final "go" outcome of the Flight Readiness Review procedure and they tried to integrate engineers, working engineers, into the flight readiness process more. I'd say that there is always a problem in organizations in providing the stability and the centralization needed to make decisions and make sure information gets to the top and providing the flexibility to respond to immediate demands; and without, you know, really studying this, I would say that what we know about Columbia is that flexibility, at least in a couple of circumstances, really wasn't there. That becomes interesting in thinking about the differences in the pre-launch decision-making structure and post-launch decision-making structure. That is, the post-launch decision-making structure is actually designed to create that kind of flexibility so that you could pull in people as you need it and so on.

What's ironic about it is it looks as if had there been either a direct route for engineering concerns to get implemented to shortcut what really little bureaucracy there seemed to be in that process that that would have helped, that if, you know, that could have circumvented the kind of need for hierarchical requests for imaging. In terms of the overall impact on NASA, I really can't say that.

ADM. GEHMAN: From my understanding, though, one of the post-Challenger results has been a much more formal FRR process. As you are probably aware, no more telephone calls, it's all face-to-face, it's done at the Cape, and you've got to be there and they're done in big rooms like this with hundreds of people in the room with several different layers, everybody there, and then there's a whole lot of signing that goes on. People at several layers actually sign pieces of paper that say, of the thousands of things that I'm responsible for, they've all been done with the exception of A, B, C, D, and then they have to be waived or something like that. Then they go through a many, many hour process of making sure that everything's been taken care of and every waiver has been carefully analyzed and in front of lots of high-level people. So it's very meticulous, it's very formal, and it's an eyeball-to-eyeball commitment that my organization has done everything my organization is supposed to have done.

Is that the kind of an organization in which weak and mixed signals can emerge? I mean, is that the kind of organization which would recognize mixed and weak signals and routine signals? Is that compatible kind of with your -- I'm still talking Challenger -- with some of the principles you outlined here?

DR. VAUGHAN: That was fairly much the procedure that existed at the time of Challenger, where every layer of Flight Readiness Review had to sign off on it. The criticism at the time, post Challenger, was that what was happening was the engineers who were making the analyses and coming forward at the Level 4, the ground level of Flight Readiness Review, those were the people who were getting the mixed, weak, and routine signals; but when they came together, they had to come up with a consensus position for their project manager to carry forward. And once they agreed, then they began gathering the supportive data that this was an acceptable flight risk. And as their recommendation worked itself up through the hierarchy, the system was designed to criticize it, to bring in people with other specializations who could pick it apart, and the result of that was to make them go back to the desk and sometimes to do more engineering analysis. That engineering analysis tended always to support the initial recommendation. So by the time it came out the top of the process, it was something that might have been more amorphous on a day-to-day basis was dogma and very convincing, which is why, with a backdrop of having that kind of information, you have people who believe in acceptable risk, it's based on solid engineering and history, who need to be convinced by hard data that something different is happening this time.

The system is designed to review decisions that have been made, that if there is a mistake in the fundamental engineering analysis, they can criticize it, but they can't uncover it at the other layers, which would mean that you would need another kind of system to detect that, such as outsiders who bring fresh eyes to a project on a regular basis. The Aerospace Safety Advisory Panel was very effective during the years of Challenger, with the exception of the fact that their charter kept them coming for visits perhaps 30 times a year. So it was impossible for them to track all the problems; and at that point when Challenger happened, they were not aware of the O-ring erosion and the pattern that was going on.

ADM. GEHMAN: I'm still trying to understand the principles here. It seems to me that in a very, very large, complex organization like NASA is, with a very, very risky mission, some decisions have to be taken at middle-management levels. I mean, not every decision and not every problem can be raised up to the top, and there must be a process by which the Level 2, Level 3, and Level 4, that the decisions are taken, minority views are listened to, competent engineers weigh these things, and then they take a deep breath and say, okay, we've heard you, now we're going to move on. Then they report up that they've done their due diligence, you might say.

I'm struggling to find a model, an organizational model in my head, when you've got literally thousands and thousands of these decisions to make, that you can keep bumping them up higher in the organization with the expectation that people up higher in the organization are better positioned to make engineering decisions than the engineers. I mean, you said yourself, "Hindsight is perfect." We've got to be really careful about hindsight, and I'm trying to figure out what principles to apply.

We as a board are certainly skittish about making organizational changes to a very complex organization for fear of invoking the law of unintended consequences. So I need to understand the principles and I'm trying to figure out a way that I can apply your very useful analysis here and apply it to find a way to figure out what the principles are we ought to apply to this case. So the part that I'm hung up on right now is how else can you resolve literally thousands of engineering issues except in a hierarchical manner in which some manager, he has 125 of these and he's sorted through them and he reports to his boss that his 125 are under control. I don't know how to do that.

DR. VAUGHAN: Well, two things. First, somehow or other in the Shuttle program, there is a process by which, when a design doesn't predict an anomaly, it can be accepted. That seems to me to be a critical point, that if this is not supposed to be happening, why are we getting hundreds of debris hits, if it wasn't supposed to happen at all. It's certainly true that in a program where technical problems are normal, you have to set priorities; but if there is no design flaw predicted, then having a problem should itself be a warning sign, not something that is taken for granted.

The idea is to spot little mistakes so that they don't turn into big catastrophes, which means spotting them early on. Two things. And one I'm certain that NASA -- maybe both of them -- that NASA may be very aware of is the fact that engineers' concerns need to be dealt with. I can understand the requirement for hard data. But what about the more intuitive kinds of arguments? If people feel disempowered because they've got a hunch or an intuition and let somebody else handle it because they feel like they're going to be chastised for arguing on the basis of what at NASA is considered subjective information, then they're not going to speak up. So there need to be channels that assure that, even giving engineers special powers if that's what's necessary.

The other is the idea of giving more clout to the safety people to surface problems. So, for example, what if the safety people, instead of just having oversight, were producing data on their own, tracking problems to the projects for which they're assigned and, in fact, doing a trend analysis to keep people's eye on the big picture so that the slippery slope is avoided?

ADM. GEHMAN: Thank you for that.

DR. VAUGHAN: Let me add also that there are other models of organizations that deal with risky systems, and social scientists have been studying these. They have been, you know, analyzing aircraft carrier flight decks and nuclear operations and coal-mining disasters. There are all kinds of case studies out there and people who are working in policy to try to see what works and what doesn't work. Are there lessons from air traffic control that can be applied to the Space Shuttle program? What carries over? Is there any evidence that NASA has been looking at other models to see what might work with their own system?

I know that in air traffic control they use an organizational learning model. What we find out from this comparison between Columbia and Challenger is that NASA as an organization did not learn from its previous mistakes and it did not properly address all of the factors that the presidential commission identified. So they need to reach out and get more information and look at other models, as well.

Thinking about how you might restructure the post-launch decision-making process so that what appears to have happened in Columbia doesn't happen again, how can that be made more efficient, maybe something -- maybe it needs to look more like the pre-launch decision process. But is there any evidence that NASA has really played with alternative models? And my point about organization structure is as organizations grow and change, you have to change the structures, but don't do it without thinking about what the consequences might be on the ground.

DR. LOGSDON: Could I ask just a short follow-up to that. Diane, your book came out in 1996, I think, right, and was fairly widely reviewed. We at the board discovered in some of our briefings from outside folks that the submarine safety program uses your work as part of the training program for people who worry about keeping submarines safe. Have you had any interactions with NASA since the book came out?

DR. VAUGHAN: No.

DR. LOGSDON: Have you ever been invited to talk to a NASA training program or engage in any of the things that you just discussed might be brought to bear?

DR. VAUGHAN: No, though, in fact, as you said, the book did get quite a lot of publicity. I heard from many organizations that were concerned with reducing risk and reducing error and mistake. The U.S. Forest Service called, and I spoke to hotshots and smoke-jumpers. I went to a conference the physicians held, looking at errors in hospitals. I was called by people working in nuclear regulatory operations. Regular businesses, where it wasn't risky in the sense that human lives were at cost. Everybody called. My high school boyfriend called. But NASA never called.

(Laughter)

ADM. GEHMAN: Anybody want to comment on that?

GEN. BARRY: What was his name?

ADM. GEHMAN: Let me finish my thought here. Professor Vaughan, again we're back to this organizational issue which I'm trying to determine the principles that I can apply from your analytical work here. If the processes we're talking about in the case of NASA, if they didn't follow their own rules, would that alarm you? What I mean is if there were waivers or in-flight anomalies or systems that didn't work the way they were supposed to work and, in the fact that they didn't work the way they were supposed to work, somehow started migrating its way down lower in the message category to where it wasn't sending messages anymore and therefore it was technically violating their own rules because they're supposed to deal with these things, would that be a significant alarm for you?

DR. VAUGHAN: Well, I think that one of the things to think about here is that NASA is a system that operates by rules; and maybe one of the ways to fix the problem is to create rules to solve the problem. So what are the rules when engineers need images, for example? Can't they find a way where they have their own authority, without seeking other authority, to get the necessary images? So I think I read that someplace, where the harmony between the way the organization operates and thinks in the key aspects of the culture itself are something that you might want to build on.

DR. WIDNALL: Actually I'm starting to frame in my own mind that the problem is that there is, in fact, one underlying rule and it's a powerful rule and it's not stated and it's not stated as simply as this question of following your own procedural rules. But let me sort of get into that. I've certainly found your framework very helpful because I've mused over this issue of how an organization that states that safety is its No. 1 mission can apparently transition from a situation where it's necessary to prove that it's safe to fly, to one in which apparently you have to prove that it's not safe to fly. I think what's happening is, in fact, that engineers are following the rules but this underlying rule is that you have to have the numbers.

DR. VAUGHAN: Right.

DR. WIDNALL: That's not the rule you stated, which was you should follow the procedures and resolve all anomalies.

DR. VAUGHAN: This is a norm.

DR. WIDNALL: Those are these kind of rules. I'm talking about the really basic rule that says you have to have the numbers. So that basically means that every flight becomes data and that concern about an anomaly is not data. So a flight with an anomaly becomes data that says it's safe to fly. So the accumulation of that data, of those successful flights, puts the thumb on the scale that says it's safe to fly; and people who have concerns about situations in one of these uncertain situations that you talk about, they don't have the data.

So I think it may be getting at, in some sense, changing the rule to one that it is not okay to continue to operate with anomalies, that the underlying rule of just having data is not sufficient to run an organization that deals with risky technologies. Because otherwise you're just going to end up with a pile of data that says it's okay to fly, and you're not likely to get much data on the other side.

ADM. GEHMAN: Is that a question?

DR. WIDNALL: That's kind of a comment.

DR. VAUGHAN: I completely agree with you. One of the reasons I emphasized in an earlier slide that you need to understand your culture is that it works in ways that we don't really realize. So how many people there understand the effect of intuition and hunch, which are absolutely integral to good engineering, and how the kind of impression on numbers suppresses that kind of information in critical situations?

People are disempowered from speaking up, by the very norms of the organization. Things like language. For example, the term I've read in the paper, "That's in family." That's a real friendly way of talking about something that's not really supposed to be happening in the first place. In nuclear submarines, they don't talk about it as "in family"; they talk about it as a degradation of specification requirements, which has a negative feeling to it. These kinds of languages which we think of as habits of mind reflect attitudes that are invisible, but the language really shows.

So the question is, you know, how can you get back in touch with the importance of engineering intuition and hunch in formal decision-making. Usually it works in the informal decision. You know, I think that's why the NASA administrators believe that they've got a safety culture and that people are free to express whatever they think; but when it comes to a formal decision, they fall back into the formal rules and that expression of concern doesn't get expressed.

Even if you take something as simple as an engineering presentation, the fact that it's reduced to charts, which are systematic, gets all the emotion out of it. It begins to look even more routine. The engineer in Challenger who saw the burned grease, the black grease, was seriously alarmed. I asked him, you know, later, "Did they see this? What did they see? Did they get a photograph?" He said yes. I said, "How did it look in the photograph?" He said it did not look serious in the photograph. So emotion is keyed to some kind of a logic based in engineering experience, and it should be valued and a way found to express it.

GEN. BARRY: Diane, I'm going to ask you a short question, and then I'm going to ask a longer question, if I may. First, the short question, focusing on organizational failure. The Rogers Commission, did they fall short on institutional recommendations in the aftermath of Challenger, or were they good ones and they just weren't followed through by NASA?

DR. VAUGHAN: The Rogers Commission was very good at identifying what they called contributing causes and what I would call system causes. That is, they identified safety cuts, cuts in safety personnel. They identified the failure of NASA to respond to recommendations of the Aerospace Safety Advisory Panel. They identified the history of the program and the fact that it was a design that was built on budget compromises in the beginning. They identified production pressures. They identified all those kinds of outside sources that had impacted the decision-making and that were a part of NASA's history.

In the recommendations, they didn't come forward with anything that said give them more money, change the culture. They weren't sociologists. They weren't social scientists and not trained to think about how that might have actually worked. The way it looked like it worked was in the sense that there were pressures there and key managers, namely Lawrence Malloy, who was the project manager for the Solid Rocket Booster project at that time, was the operator who made the error. Once that happened and the key person was identified and people changed and new people came in, then the system problems remained.

They fixed the technology. They fixed the decision-making structure in ways I described earlier. But the organization didn't respond and neither did -- in keeping with my point earlier about top leaders being responsible -- the organization did not respond in terms of getting more money beyond what it took at that point to fix the technical problem. They got an initial boost, but they've been under budgetary constraints all along. The recommendations in the volume of the presidential commission were related strictly to internal NASA operations. They were not directed towards policy-making decisions that might have affected the program.

GEN. BARRY: Okay. Let me build on that a little bit and just carry it on and see if this resonates with you. Let's talk about a bunch of items here and see if this falls true with what you know to be from Challenger that might be able to be translated over to *Columbia*.

First of all, you stated that with Level 4 identifying problems and being able to try to communicate that up the institution, the organization kind of stymied that. So I would characterize that as needing to prove that there is a problem in the early stages of the FRR or before flight. I think post-Challenger, you know, there has been a fix on that and, remember, the Flight Readiness Review is supposed to prove not only launch but also en route, in orbit, and then of course on recovery. So it's the whole flight. It seems like they've solved the problem on trying to say is there a problem in proving it. To post launch. There's, some would argue, an attitude that you have to prove there is a problem. So we kind of fix it on the launch side; but after it's launched, we kind of relegate back to maybe the way it was prior to Challenger: Prove to me there is problem.

Now, if we try to look pre and post launch, pre-launch is very formal, as Admiral Gehman outlined earlier. You've even alluded to it in the book. Post-launch, it could be argued, less formal, more decentralization, more delegation certainly, okay, from what we see at the FRR prior to launch. Multi-centers are involved prior to launch. I mean, they all meet and they all sit at the same place, they're all eyeball-to-eyeball. Center directors are represented, program managers. Post-launch, again decentralized, it's mostly a JSC operation. Of course, KSC gets involved if they're going to land at Kennedy.

There's a tyranny of analysis pre-launch maybe and that is because you've got -- well, you have a long-term focus because you've had time. But post-launch, there's a tyranny of analysis, but it's in real time because you don't have as many hours and you've got to make decisions quicker and all that other stuff.

The real question -- if this resonates with you at all -- could it be argued that during Columbia, NASA had a "Prove there is no problem" prior to launch and post-launch it was "Prove to me there is a problem" and we have this formal and informal kind of focus. It seems to me after Challenger we fixed the prior to launch, certainly with having people appear in person and no VTCs or no over-the-phone. Everybody had to be there in person. And we have maybe a problem that we need to fix post-launch now with the MMT and the decentralization elements and maybe the delegation.

I certainly don't want to relegate it to a headquarters level, but there are some things that need maybe to be fixed there. So I would ask really your opinion that is there some kind of a delineation in your mind, from what you know to date, pre- and post-launch, that we might be able to help provide solid recommendations on to improve NASA?

DR. VAUGHAN: I'm wondering if the post-launch flexibility is such that you can, in fact, have similar things going on in two different parts of the process in which people are not in touch. So I understand that video requests really originated from two different points, and working engineers in two different locations, and that they didn't really know that the other had originated a request.

It certainly seems that the mentality of proving your point when you've got a timeline like you do and it's an unprecedented circumstance, as it was with Columbia, is wrong, of course, in retrospect. The question you're asking is how can we convert that into a process that prevents this from happening again.

No, a famous sociologist named Donald Cressey once told me when I was beginning the analysis of the Challenger launch, "It's all these numbers. It's all these numbers, and there are these debates about issues. Why don't you do it like they do it in the Air Force? You just should have a red button for stop and a green button for go." And there's a lot to be said for simplifying a complex system, whether it's decentralized or centralized, so that key people can respond quickly and shortcut the hierarchy. I don't know if that begins to answer your question. But there may be need to be some more rules created in the sense that --

GEN. BARRY: And this is really stretching it but --

DR. VAUGHAN: Maybe it needs to be more formal than it is and maybe it needs to be more like the pre-launch procedure in terms of the rigor of numbers of people from different parts who are looking at problems that crop up while a mission is in process instead of waiting just -- I mean, some sort of a formalized procedure where there's a constant ongoing analysis instead of you've got worried engineers in two different locations who are kind of independently running around, trying to get recognized and get attention to the problem.

MR. WALLACE: NASA's taken quite a pounding here today but I'm wondering what we can --

DR. VAUGHAN: I thought this morning they were coming off pretty good.

MR. WALLACE: I would just like to talk about what we can sort of learn about what they do well -- in other words, areas where we don't seem to have this normalization of deviance or success-based optimism. Like BSTRA balls and flow liner cracks and some of those fairly recent examples where there were serious problems detected with the equipment, in some cases detected because of extreme diligence by individual inspectors and really very aggressively and thoroughly fixed.

It seems to me that part of the problem of normalization of deviance is sometimes the level of visibility that an issue gets. How do you sort of bridge that gap between those things that get enough visibility or sense of urgency and those that somehow seem to slip below that threshold?

DR. VAUGHAN: Someone said after the book was first published -- and then again now I've been getting a lot of e-mails. Someone said at the time the book was published, "I bet if you took any component part of the Shuttle and traced it back, you would find this same thing going on." Perhaps doing a backward tracing on other parts of the Shuttle could show you two things. First, what are the circumstances in which they're able to locate an anomaly early and fix it so they stop it in its tracks and avoid an incremental descent into poor judgment? Are there other circumstances in which the same thing is happening? Can you find circumstances where you do have the normalization of deviance going on?

It's interesting in the history of the Solid Rocket Booster project that there was a point at which they stood down for maybe two months to fix a problem. How is that problem identified? What are its characteristics? I would bet that the more uncertain, the more complex the part and the more amorphous the indications, the more likely it is to project into a normalization-of-deviance problem, given the existing culture where flying with flaws is okay in the first place.

MR. WALLACE: Well, sort of following on. Earlier you said -- and good advice for this board -- that we should try to see problems as they saw them at the time and not engage in the hindsight fallacy or whatever that's called. I mean, I'm not sure you said this; but my assumption is that that's almost the only way you can learn to do better prospectively. I mean, do you have any other thoughts on that? In other words, to see the problem as they saw them at the time, to me, is almost a step toward the discipline of seeing the next one coming.

DR. VAUGHAN: Right. It's an experimental technology still; and every time they launch a vehicle, they've made changes. So they're never launching the same one, even though it bears the same name. This is a situation in which, like most engineering concerns where you're working with complex technologies, you're learning by mistake. So that's why post-flight analysis is so important. You learn by the things that go wrong. Every once in a while you're going to have a bad mistake.

ADM. GEHMAN: Did I understand the point that you made both in your book and in your presentation here is that the answer to perhaps Mr. Wallace's question lies in the theory of strong signals? In other words, if NASA gets a strong signal, they act on it. No problem. They very aggressively shut the program down and go fix it. The problem is in the weak, routine, or mixed signals. Those are the ones that seem to bite us. Of course, there are a lot of them; and they don't quite resonate with the organization. Is that a good analogy?

DR. VAUGHAN: It is. The idea of a trend analysis is that it could pick out stronger signals from lesser ones before it becomes, you know, an enormous problem; but the recognition of the pattern is important, bringing forth the pattern so that the people who are making decisions are constantly in touch with the history of decisions that they've gone through before.

I have to say with that, though, it's important that they have quantitative evidence to fly. Maybe the more qualitative evidence could be brought in in other ways further up the chain, that whereas in Flight Readiness Review, for example, they present everything on charts and they ask -- the purpose of Flight Readiness Review is to clean the hardware and get it ready to go. The purpose of it is to clear up the problems as it works its way through the Flight Readiness Review process. What happens, as I mentioned, is that the engineering argument tends to get tighter and tighter because they're constantly doing the work to investigate and respond to questions and, in a sense, defend what they've said or find out if there are flaws.

At the time of Challenger, I read thousands of engineering documents for all the Flight Readiness Reviews that they had had and I didn't see anyplace in the Flight Readiness Review process that would allow for the presentation of simply intuitions, hunches, and concerns, where qualitative evidence might be presented, like a clear image or even a vague image of a piece of debris the size of a Coke cooler, for example, rather than charts for an engineering analysis, you know, that there ought to be room in the process for alarm.

ADM. GEHMAN: In your experience, particularly with what I'm calling these weak signals or this muttering around the room that the O-rings can't take freezing temperatures but we're not really sure whether they can or cannot, I have in my mind a model that says that it's unfair or not reasonable to set as a standard for the organization to act on literally hundreds of these misgivings that the tens of thousands of people may have and that it's an unfair standard to require the people who have these doubts to prove that their doubt could cause the loss of the vehicle or the crew. But I have in my mind that it's a more reasonable standard that management should realize that the accumulation of signals from the process are cutting into their safety margins and that you can accumulate these things not in a measurable way but in a subjective way, particularly in a regime in which you have very thin safety margins to begin with, that you should be able to reasonably determine that you're narrowing your safety margins in a way that should concern management. Is that a reasonable characterization of the standard or the bar that we set here?

DR. VAUGHAN: I think that shows up in the problem of lack of data in both of these circumstances, that there were early warning signs and in neither case had those early warning signs been pursued and say, "Well, the imagery is bad. We know this is happening. We can't see exactly where it's hitting. Why don't we get this now?"

I mean the power of the e-mail exchange was that they really hadn't thought the possibility of failure through. There was no plan for what needed to happen if there was, in fact, a serious tile hit and damage to the wing, what would they do at re-entry and what would it mean to attempt a wheels-up landing at the landing site, and that failure to pursue the trajectory of having a problem that's repeating. Like if you think about cost, you think about cost maybe in terms of if that's a factor in making issues a priority at NASA, which obviously it is anyplace -- you can't fix everything -- think of the cost if you simply don't have the data you need, which is, I think, the most stunning thing about the comparison of the two cases. At the time when conditions were highly uncertain, in neither case did they have the data; and having that background data is important.

ADM. GEHMAN: In your review of the Challenger decision, did you personally come to the conclusion that the launch decision would have come out differently if the Morton Thiokol engineers' split decision -- because some of the Morton Thiokol engineers said it was safe to launch, but they were split on that -- and if the managers at Marshall had reported that there was a split decision, that the FRR would have come out differently? Did you have any evidence of that?

DR. VAUGHAN: The managers at Marshall did not know that there was a disagreement at Thiokol. That was one of the problems with them being in three locations. No one ever thought to poll the delegation. So no one on the teleconference knew really where anyone else stood. They knew what Thiokol's final recommendation was and they assumed that Thiokol had gone back and re-analyzed their data, seen the flaws in it, and been convinced it was safe to fly. So the fact that not everyone was heard from was critically important.

By the same token, Thiokol engineers didn't understand that they had support in the other places, that one of the NASA managers who was at the Cape was really sitting there making a list of people to call because he believed that the launch was going to be stopped. So that was truly a problem.

Now I've lost sight of your question.

ADM. GEHMAN: The question is: In your research about Marshall, did you come to the personal conclusion from talking to people that the fact that the cold temperature analysis at Morton Thiokol was a split decision, that that would have made any difference at Marshall? I mean, did anybody say, "If I had known that, I would have changed my mind"?

DR. VAUGHAN: Yes. However, the goal is for unanimity and here's again where numbers count, that in the instance where engineering opinion is divided, then they make what's known as a management risk decision, that the managers take over and the managers at Thiokol then, who knew that their engineers were split, made a management decision. In retrospect, that was the most horrendous example of failing to listen to your technical people who said, "You know, I can't prove it, but I know it's away from goodness in our data base."

ADM. GEHMAN: This principle that I'm following up on here is important because we do have to be careful of hindsight. And it may be that, even armed with what is admittedly a minority opinion of a bad outcome, it could be that these are judgment calls that are made in good faith with people doing the best they can and they make a mistake. I mean, they call it wrong. So the question is whether or not we can indict the system, based on these incidents.

DR. VAUGHAN: I think you have to analyze -- you have to do a social fault tree analysis and figure out what actually happened and what went on, how is information relayed. I'm sure that's work that's ongoing with you.

ADM. GEHMAN: That brings me to my next question -- and pardon me for monopolizing the time here. Another good writer on this subject, who I think is Nancy Leveson, in one of her models she suggested that we need to diagram these decision-making systems because, just as you say, it's not a person, it's a culture, it's an organization that's really driving these things. Are you aware that anybody's ever diagrammed the FRR or the waiver, in-flight anomaly disposition system? Has that ever been diagrammed, to your knowledge?

DR. VAUGHAN: Not that I know of. But what would be more interesting would be to look at the more informal decision processes because the rules are so strong for how information is addressed in Flight Readiness Review that that would probably turn out the same every time. What you would want to look at are the more informal processes and try to map them and understand where the information stopped and why it stopped.

MR. WALLACE: I'd like your thoughts on the concept of whether an organization, this one, can sort of become process-bound. You cannot fault the thoroughness of the processes. But, I mean, is there a point at which they can almost subvert other thinking processes, that people become so confident in the thoroughness of the processes and the fact that they're tested, they reach a comfort level with processes where they become the be-all and end-all?

DR. VAUGHAN: Well, that's one of my main concerns about NASA, that the fact that it is a very rule-guided organization and the fact that they do believe that when they follow all the rules that they have done their best and have confidence. That's why the rules tend to carry such heavy weight. Not only do they aid them with the process but then they have a cultural effect which builds confidence. If you're not in touch with the everyday engineering data itself, you can lose sight of the fact that it is still an experimental system. So it's the dark side of the organization. The same kinds of procedures that you implement to make it work better also can have an unanticipated consequence, and that's why keeping in touch with all the ambiguities in the engineering decision-making would be important.

Any other doubts and concerns? You know, by the time you get to the top of the Flight Readiness Review process, nobody's going to say that. One of the proposals from the presidential commission was that an engineer accompany his project manager at each level of the Flight Readiness Review tier, the feeling that because engineering concerns did not get carried up to the top prior to Challenger and in the eve-of-launch teleconference, they thought that would be a good idea. Rather than the engineers at Level 4 turning over all their information to their project manager and then the project manager carries it forward, let's integrate engineers into the process. But can you imagine some engineer in the top Level 1 Flight Readiness Review with 150 people, after all that's gone on, standing up and saying, "I don't feel good about this one"?

ADM. GEHMAN: Well, I agree with you. I agree with you. But I would compound that with an organizational scheme in which even though that engineer works in the engineering department and technically doesn't work in the program office but his position and his salary is funded by the program office and he wouldn't exist if the program office didn't pay him. In other words, we've wickered this thing to where the money flows down through the projects and they send money over to the engineering office to hire people. So now put yourself in the position of this guy who's going to contradict the officer who's paying his salary, and you don't have a very comfortable formula.

DR. VAUGHAN: I understand that. I think there's a parallel situation with safety people.

ADM. GEHMAN: Well, yes and no. There is a safety organization in the programs and in the projects and their positions depend upon the largesse of the project managers, but there's also an independent safety organization.

DR. VAUGHAN: I meant in terms of rank. Like independent authority and power based by where they come in the GS ranking system.

ADM. GEHMAN: Absolutely. That's a question I'm going to ask you after General Barry and Dr. Logsdon have a shot.

DR. LOGSDON: What I have is a comment that's as much directed at the board as it is at Professor Vaughan. It's just that the discussion made me think of this line of reasoning. We've been talking about the rigor of the pre-flight process for Flight Readiness Review, compared to a different structure for what goes on during a mission. There's almost a symbolic element here. The management of the launch is a Kennedy Space Center responsibility; and the moment that the Shuttle clears the launch tower, the control over the mission shifts to Johnson. Sean O'Keefe is trying to say that NASA is a single organization, but he's got a long way to go to achieve that goal. These are very proud organizations and, of those, Johnson is the very proudest of the proud because it's one of the only two places in the world that knows how to manage a space flight. There are now -- what's it, '61 -- so 42 years of experience of managing humans in space.

So we're beginning to talk about maybe we can examine the process of mission management and see whether it measures up to some standard of high-performance organizations, and I think that's what we have to do. But there's a lot of received wisdom and maybe it's ossified wisdom by this point in the process. So as we go towards that, I think we have to make sure that we don't have unintended consequences. So, as I said, that's just a comment, not a question.

ADM. GEHMAN: Would you like to comment on his comment?

DR. VAUGHAN: Well, he directed that to the board, as well.

ADM. GEHMAN: In the interest of time, I'll go on to General Barry.

GEN. BARRY: I'd just like to add one more thing to your parallel kind of discussion between Challenger and Columbia. Could you just see if there's anything you know of that you could add to this kind of construct? You know, there was a lot of organizational changes here in the last couple of years. We moved Palmdale to Kennedy. We moved the Huntington Beach engineering support mostly to JSC but some to KSC. And, of course, we've got the International Space Station support going on. So there's some organizational elements that are unique to Columbia this time; but there are some Challenger organizational elements, too. You know, the JSC leadership was being shared by Jesse Moore at that time between JSC but he was also running the space flight program as an associate administrator. Also, we had an interim administrator at the time during Challenger. Are there any parallels that you're seeing between the organizational aspects between Columbia and Challenger?

DR. VAUGHAN: At the administrative level?

GEN. BARRY: Well, just organizational elements that we might be able to draw from.

DR. VAUGHAN: One, but it's cultural. It seems like there is a gap between perceptions of risk between working engineers and top administrators. So at the time of Challenger, engineers were very concerned with every launch, even though they had gone through all the rigors of the procedure; but at the same time, the people at the top thought it was an operational system. The parallel I see is, you know, working engineers really familiar with what's going on and having concerns, but decisions made that really do echo the period of Challenger where it's okay to take citizens along for a ride, which suggests that top-level administrators have rather lost touch with the fact that it is an experimental system, a message that they clearly understood post Challenger.

John mentioned symbolic meanings, and they can be really important. It's hard to judge exactly what the effect is of a top administrator believing that it's again safe enough to fly people who are not trained as astronauts. Subtle things like "faster, cheaper, better" can have an effect on a culture, even at the same time that you're doing everything possible to encourage safety.

Certain actions have symbolic meaning. The fact that you have a safety representative sitting in on a Mission Management Team or in a particular wherever they're assigned can have symbolic meaning. Signs posted that it's safety, safety, safety can convince that you have a safety culture; and yet when you look at the way the organization works, you may not have as strong a safety culture as you wished. The safety person who is assigned to Mission Management Team decisions, if that is the case, is in a position of not having hands-on information and reviewing their decision but not, in a sense, dependent upon them because they have the leadership responsibility. So what kind of weight, you would want to know, is that person really bringing to that situation? Do they have the influence that they are listened to? Do they have the data to really do anything more than oversight at that point? How do you really put them in a position where they can recognize a warning sign and talk with people who are higher ranked than they are, in a definitive way, that is convincing in a crisis situation?

ADM. GEHMAN: That leads to my question. That is, would you be content -- let me just outline this in rough form -- of a process to satisfy that issue. That is, that senior management, the management who's got the ultimate responsibility for these decisions, that they would kind of be forced to listen to these engineering doubts because of an organization in which you had checks and balances among essentially coequal branches of some kind. In other words, that the engineers were organizationally and culturally equal to the project managers and the safety and mission assurance people were not only -- I agree with you. I understand exactly what you're saying. It's not good enough to just sit at the table. You have to come to the table with some clout and usually that clout's in the form of analysis or data or research or else I won't sign your chit for your money or something like that. You've got to come with something. And my model suggests that if you did that, you would be creating some degree of managerial chaos but, on the other hand, you would be making sure that engineering reservations and engineering concerns were well researched and got surfaced independently at the right level. So you've kind of got this trade-off between a little bit of managerial chaos, you would have the danger of the organization not speaking with one voice and all those kinds of things but, on the other hand, you would satisfy the requirement that signals would get heard.

DR. VAUGHAN: Surfaced.

ADM. GEHMAN: Does that sound reasonable?

DR. VAUGHAN: It does sound reasonable. Someone said if every engineer aired every concern, you would never launch a mission; and that's probably true.

ADM. GEHMAN: Probably true.

DR. VAUGHAN: It seems in post-launch conditions where the clock is ticking, in line with Dr. Barry's suggestion about how could we restructure the post-launch decision process, that it would be especially important, then, to create that kind of an open process.

ADM. GEHMAN: Okay. Well, thank you very much, Dr. Vaughan. You've been very patient with us. We hope we haven't tried your patience too much as we try to understand the very sound principles that you have exposed us to, both in your book and in your briefing here today.

The board is sensitive about the law of unintended consequences, and we want to be very careful that we understand more about these managerial principles before we go writing something down on a piece of paper that we might regret. But your study has had an influence on this board and we're indebted to you for coming and helping us through it today.

DR. VAUGHAN: Thank you. Thanks for having me.

(Hearing concluded at 4:38 p.m.)

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APPENDIX H.9

May 6, 2003
Houston, Texas

Columbia Accident Investigation Board Public Hearing
Wednesday, May 6, 2003

9:00 a.m. - 12:00 noon
Hilton Houston Clear Lake
3000 NASA Road One
Houston, Texas

BOARD MEMBERS PRESENT:
Admiral Hal Gehman
Brigadier Gen. Duane Deal
Major General John Barry
Major General Ken Hess
Dr. Sheila Widnall
Mr. Roger Tetrault
Mr. G. Scott Hubbard
Mr. Steven Wallace

WITNESSES TESTIFYING:
Dr. Gregory Byrne
Mr. Doug White
Mr. Steven L. Rickman
Dr. Brian M. Kent
Dr. Dave Whittle
Mr. Paul S. Hill

ADM. GEHMAN: Good morning, everybody. This public hearing of the Columbia Accident Investigation Board is in session. We have three panels of two people each to hear this morning. The purpose of today's hearing is to put into the record and let the Board hear an update of the very latest data that we have on data from the Orbiter, information from the debris, and information concerning the testing of the Flight Day 2 object which was observed orbiting with the Shuttle. This will bring the Board completely up to date with the latest information we have from all of the analysis that's been going on.

The first of our panels today, we're delighted to have two people who have been working on this project since day one and are very knowledgeable in exactly what went on onboard the Orbiter.

We are grateful, gentlemen.

Doug White is the Director for Operations Requirements in the Orbiter Element of USA; and Dr. Gregory Byrne is the Assistant Manager, Human Exploration Science, at JSC.

What I would like to do, first of all, gentlemen, is read you a statement that you will attest that you are telling us the truth. Then I would ask you to introduce yourselves, say a few words about you, and then if you have an opening presentation, we will let you have the floor and we'll listen to your presentation.

So before we begin, let me ask you both that you affirm that the information you're going to provide the Board today is accurate and complete, to the best of your current knowledge and belief.

MR. WHITE: I do.

DR. BYRNE: Yes.

ADM. GEHMAN: All right. If you would introduce yourselves, please, and then we will start the presentation.

GREG BYRNE and DOUG WHITE testified as follows:

MR. WHITE: I'm Doug White. I'm Director of Operations Requirements for United Space Alliance. My responsibilities include turn-around requirements, problem-solving for during the turn-around, and in-flight; and I'll be presenting a summary of the MADS data today.

DR. BYRNE: I'm Greg Byrne. My normal job at JSC is Manager of the Earth Science and Image Analysis Laboratory. For the 107 investigation, I'm the lead of a much larger image analysis team which includes imagery experts from across the country. And I'll be presenting today some ascent video and film.

ADM. GEHMAN: Thank you very much. You can proceed.

MR. WHITE: Greg, why don't you go first.

DR. BYRNE: Okay. I understand, Doug, that you have a long briefing. So I'm going to be short and just answer questions as they come.

Can I have the first slide, please.

First of all, by way of introduction to the team, the Image Analysis Team consists of both NASA organizations and non-NASA. As I mentioned, imagery experts from around the country. The NASA organizations include Johnson Space Center, Kennedy, Marshall, and Langley; and then outside of NASA we have independent assessments from folks at the National Imagery and Mapping Agency, NIMA, and Lockheed Martin at three locations across the country.

So let me start with an overview of the imagery we have to work with. You've seen these views already. They have been released to the public. We have two primary cameras that we're able to work with to analyze the debris event on ascent, the debris that struck the wing. Two cameras: E212 and ET208. I do have some short movie clips of these.

But by way of introduction and background for these two views, E212, the imagery that we had to work with was original. We took the original negatives from the camera and had it digitally scanned at the highest resolution. So we had the best-quality digital imagery to work with from that camera. That camera gave us the best view of the bipod ramp area, which was the source of the debris. It also gave us the best view of the debris itself for size measurement. The drawback to that view was that we had literally no view of the impact area from that particular view.

The other camera view is a video camera. It's called ET208. We also had it digitally scanned from the original tape. The advantage of that particular tape is that we do see the impact area directly; but it being video, it's inherently less resolution than the film. But it does give us a full view of the debris all the way to the impact area.

Next slide, please. Also, by way of background, here's a layout of the KSC area. It shows the relationship to the launch pad, which is that circle right there, with the two cameras which are south of the launch pad. Then that red line, that is the Orbiter trajectory going uphill. Now, the event happened at about 81 seconds. It would put it right around there by that bubble five. So these are the lines of sight to those respective cameras.

E212 was the closer one. It was about 17 miles away. ET208, further south, was about 26 miles away. So the cameras were distant from the Orbiter, but they are essentially telescopes with cameras mounted to them and they track automatically and so we get a good view.

Next slide. Let's go ahead and go to the movie. Eric, if you would key up that movie for me, please.

What we're going to show here is that ET212 view. It has both the visible frames and what we call a difference mode of frames. We'll show those side by side in movie format and then track the debris on down. So on the right is the normal view, and on the right is a difference view.

Just looking at the normal view first, the debris exits from the bipod area and strikes the underside again. Again, we don't see the actual strike, but we do see the debris cloud, post-strike. It passes entirely underneath the wing. We don't see any evidence of debris or a debris cloud coming over the top of the wing. So that's an indication to us that the strike was entirely on the underside of the wing, below what we call the stagnation point on the leading edge.

The difference view highlights changes from one frame to the next; and so it's useful for highlighting the debris because, of course, the debris wasn't in the frames previous to the event itself. So it does highlight the debris, and again you can see it tracking on down. Unfortunately, what it does is also exaggerate the size of the debris. So you can't use it for size measurements, but it does give you a better view of the debris itself and then the post-impact cloud coming on down.

The cloud appears to be pulverized foam or perhaps tile. We can't tell if it's tile or not, but upon closer inspection -- and I'll talk about this later if I have time -- we do see actual chunks of debris. You can see them as they pass through this region here, by the SRB. There are actual chunks of debris in that view, as well.

Next slide, please.

ADM. GEHMAN: Greg, let me interrupt a second here with a question. I think this is a good point. Are there Launch Commit Criteria for the number of cameras that should be working? Are cameras a Launch Commit Criteria?

DR. BYRNE: I don't believe they are, but I'm not the person to ask.

MR. WHITE: No, they're not.

ADM. GEHMAN: So whether you've got one working, two working, or four working just depends on whether you're having a good day or not a good day.

DR. BYRNE: Okay. This next view is another movie view that shows the actual trajectory. We map the trajectory to try to understand the character of the debris as it comes on down. What we'll see in this movie is that it appears that the major piece of debris acts as a parent, so to speak, that it spawns smaller pieces along the trajectory. So it's possibly shedding smaller pieces and we can see them pass under and then the major parent piece is the one that strikes the wing. So let's go to that movie, please.

Another conclusion was that we saw no evidence of more than one strike other than the major parent piece.

Okay. Here again, we'll see the event begin around the bipod ramp area; and maybe we can go slowly frame by frame, if that's possible. Yellow is the major parent piece. It originates here. Frame by frame. The piece is spawning off. Little pieces in blue and then other smaller pieces in red keep on coming down. You see the other red and the blue pieces pass underneath and then the parent piece striking and then here are individual post-strike debris chunks that we're able to track and measure sizes. We're still working on that.

Okay. Let's go to the next slide, please. The other camera view, the ET208 video, again, as I mentioned, we see it all the way from the bipod ramp to the impact area right there on the leading edge. Again frame by frame, we can map it on down; and let's play this movie very quickly.

I was asked to bring the best quality copies of these, and that's not possible on a setup like this to view it in best quality. For that we would need our laboratory facility or something similar to it. We might not have any luck with this one. It worked back at the facility. Okay. Why don't we go on? I apologize for that.

Back to the E212 view. Once again, we can map frame-by-frame the trajectory of the debris coming on down, just as we can map frame-by-frame in the other view, and we can take those two camera views together. Go to the next slide, please.

With those two camera views, we can define line-of-sight vectors for every point along the trajectory or every place where we see the debris in those frames and we can then use a two-camera solution to derive a three-dimensional trajectory of that debris as from source to impact. That's very important for us to be able to determine the point of impact and three-dimensional velocities.

Next slide, please. Concerning the debris source, we have a couple of lines of evidence that tell us that, yes, indeed, it was the bipod ramp or the immediate area next to the bipod ramp that was the source of the debris. I mentioned the three-dimensional trajectory mapping that we do.

Here this red line is one of those trajectories that we've mapped onto the CAD model of the External Tank. So we take the imagery and then we employ CAD models and overlay the imagery on the CAD model and that gives us a graphical representation of the Orbiter that we can overlay the trajectory onto for visualization and, as you can see, there's the bipod ramp on the left side of the Tank. This trajectory maps it to right adjacent to and on top of. That's an indicator that, yes, it was the bipod ramp.

In the next view, take the imagery itself. Next slide, please. And we do some enhancement. As I mentioned, the E212 view gives us a view of the bipod ramp but not a very good one. But if we do a technique of frame averaging in which you overlay multiple frames and do some enhancements and bring out detail, you can see in this before-and-after view – the before being on the left where we've averaged 22 frames immediately before the shedding event and then 21 frames immediately after the shedding event -- if you look at the differences before and after, and there's the bipod ramp. It's a slightly different shade of color, slightly lighter color than the Tank, so you can see it. It's very subtle, but there is a definite change to that area. It's whiter, as if to expose the white substrate underneath.

Next slide, please. We have measured the debris size, again from that E212. We took a frame-by-frame measurement of the debris. Here's one frame on the left and another on the right, just to give you an example of how the apparent size of the debris changes frame-by-frame. Obviously it's tumbling. It's tumbling and so it is changing its orientation relative to the camera line of sight. So in every frame it has a different appearance. But if you take this frame-by-frame measurement and lay them all out, you can deduce from the multiple frames an estimate of the size and our estimate is given there, 24 by 15, in the length and the width. Now, we weren't able to determine that third dimension, which was depth; but we were able to determine that that depth is a much smaller dimension than the other two. It's plate-like, a length and a width and a much smaller third dimension, plate-like, and that we could not determine from the imagery alone.

Next slide, please. 3-D trajectory analysis. As I mentioned, we're able to map to the wing to determine impact locations; and we had several analyses. Again, my team consists of many different organizations, in many cases working independently and so getting different results; but when you take them all collectively, we are able to determine that the impact location was in the range of Panels 6 through 8. Now, when I say impact location, we have to keep in mind this is a big piece of debris and that it's likely to strike multiple panels. But the center line of the trajectory, at least in this model -- and this is just one example of the several that were generated. Here's the center line of the trajectory, and the center line intersects the wing at that location right there. So in this model, X would mark the spot of the center of the impact; but, of course, it's a big piece of debris and then there's uncertainty in that trajectory on top of that. So that would then spread out our area of impact location across these three panels and then the other trajectories are also showing some dispersion, as well. So we can't exclude the possibility that Panels 5 and 9 were at least partially impacted. So that's our range, 6 through 8, plus or minus one, and more likely outboard than inboard.

Next slide, please. We did measure the velocity, but we weren't able to pinpoint it. The total velocity -- we got actually three components of velocity, and when you add them all up, the total velocity was in this range measured from the imagery -- 610 to 840. Now, that's a wide range and I'm disappointed our team was not able to pinpoint it any better than that, but we're fundamentally limited by simply a few data points to work with. When you're working with so few data points, especially in four dimensions, X, Y, Z, and time, then you can get a wide range of answers, and that's why we have this wide range. But I am confident that the total velocity, the true velocity, is within that range. But it takes more than just imagery alone to nail down the impact velocity and so we've needed to apply some physics to the problem. So we're turning our results, our trajectory data over to the folks who are working the fluid dynamics and applying some air-flow dynamics to the problem to get a better estimate of the velocity.

Of course, all of this is going to feed into the impact testing; and everything we've been doing up to this point has been driven by the need to feed the impact testing. So our schedule has been pushed to meet that schedule.

Next slide, please. In regards to what can we see on the bottom side of the wing, ET208 gives us a direct view of the underside of the wing and, again, these frame averages before and after. On the left is before the event, before the strike to the left side of the wing, or rather the left wing. Then on the right is the "after" view. Same averages. In the "after" view, when you do the differencing, we simply don't see any difference before and after. So that's an indication that tells us that we simply can't see any damage. Of course, the Orbiter perspective is not the best in this view and our resolution is not very good and we estimate the resolution would be about 2 square feet. What that means is that in order for us to see damage, we would need at least a 2-square-foot area of difference to see it.

ADM. GEHMAN: Which is on the order of three or four tiles square, I guess.

DR. BYRNE: Something like that.

ADM. GEHMAN: Two tiles by two tiles.

DR. BYRNE: Of course, that's presuming that the damage would be in the form of tile removal to have a high contrast between the dark normal tile on the top versus the white substrate underneath. So that would assume a high contrast in the damage.

MR. WALLACE: What might you expect to be able to see as far as damage to the lower surface of the RCC and the T-seals?

DR. BYRNE: We wouldn't expect to see any damage to the leading edge. Again, I mentioned --

MR. WALLACE: I mean, is there a degree of damage that you're confident you could have seen?

DR. BYRNE: Yes. About a 2-square-foot.

MR. WALLACE: Even in the RCC? Or are you just talking about the acreage?

DR. BYRNE: Just in the acreage. I wouldn't expect to see any damage in the leading edge because contrast is all-important and a hole in the leading edge would be presumably a dark hole against a dark background. In a view like this with the resolution that we have, we simply wouldn't see it, even if it were a gaping hole, I think.

ADM. GEHMAN: I don't have any argument with that conclusion; but what about the sharp edge, leading edge of the RCC there? I'm thinking about a notch or something missing, even though I agree, when you've got the dark RCC against a dark hole against a dark background, you can't see anything. But what about the leading edge there? Is that enough definition there to indicate some -- I mean, you've got that nice leading edge against that nice white background.

DR. BYRNE: If there were a large enough gap, I think we might be able to see it. If there were an entire panel missing or two panels adjacent to each other missing, it's possible that we could see it because it would show up against the white background of the fuselage. So, yes, that's conceivable; but, of course, we didn't see anything like that.

Next slide, please. The last slide, I mentioned the debris post impact. The wing is up in here, and the debris after the impact is sweeping on by. This is an area of work that we're still pursuing to characterize better the size of these chunks post impact and primarily to see, well, two things: Is there any hardware in there? Can we say it's tile or can we say it's a T-seal or something of that nature? That's a very difficult task, of course. But also to characterize it to compare it with what we see in the impact testing. My team is also involved with the impact testing, doing the photogrammetry in those tests, so we want to compare those tests, which is what we see here, to see does it make sense.

That's all I have.

MR. HUBBARD: Thanks, Greg, for that description. I've got maybe four or five questions here, a number of which are intended to just illuminate things that have been in the realm of rumor and give you a chance to talk about this and perhaps put it to bed if it's not factual. The first one has to do with a statement that I have heard several people make that there was another camera, a third camera. Some people have called it Camera 204 and so forth. So can you talk a little bit about that?

DR. BYRNE: I can, yes. There was another camera that saw the debris. If we can pull up that map. The second slide, I think. Camera 204 was well south of the other cameras. I don't have a mileage exactly, but well south.

MR. HUBBARD: So much further down.

DR. BYRNE: Much further south. It did see the left side of the Orbiter with basically the same perspective as 208, but much further away. So a worse view in that regard, worse resolution.

Now, early on in the analysis, of course, our analysis team, even during the mission, screening all of the imagery from all the cameras, we saw that debris in 204. But early on in the analysis, it was discarded as un-useful for analysis simply because it was so much poorer in resolution. The debris looked like a fuzzy blob. At that time, as I have mentioned, it was disregarded. Since then, especially in regards to the velocity calculation where we were strapped with having so few data points to work with and in that sense any data point is a good data point perhaps, one of the team members -- it was the folks from Marshall -- went back to the imagery to try to get more data points and they did access that 204 camera and determined that possibly two frames, two data points from E204, were useful for their trajectory analysis and subsequent velocity calculations. So they did fold that into their calculation, and we discussed that with them last week. Their result is brand-new as of last week.

The bottom line is we don't know if it adds value or not. Marshall did their analysis with 204 and then redid it without 204 and got the same result. So although the error associated was much larger and they did determine that the error was much larger, it didn't seem to hurt the analysis but didn't seem to help it either. So that's the story on 204.

MR. HUBBARD: Okay. Very good. Thank you. So what you presented today, Camera 212 and Camera 208, represents still the best available evidence for all the calculations you've done.

DR. BYRNE: Correct.

MR. HUBBARD: The second thing has to do with the number of objects. A lot of speculation about the spawning, how many pieces came off and so forth. Can you just expand a little bit on how many objects you have clear evidence that exist and resolve that dispute a little bit?

DR. BYRNE: Right. Early on, that was the big question: How many particles are we talking about, how many impacts were there. To this day, I don't think we've had total team consensus on that, simply because at the top of the trajectory -- first of all, on 208 we only see one piece of debris throughout, in that video view from far away. It's in 212 where you can see more than one piece, but how many there are is still indeterminate. There's almost a shell-game juggling act going on at the top, and trying to pick out which piece is which, when is very difficult to do. But we had determined early on that we think we saw three pieces, three distinct pieces.

Now, whether they originated as three pieces from the bipod -- in other words, came off in three pieces originally -- or whether they were spawned, that we have never been able to determine because literally now you see them, now you don't. It's that sort of game going on at the top. Even frame by frame, when you see a piece of debris, the next frame it's gone. So either it's a very thin piece that when it turns edge on, you simply don't have the resolution to see it, or whether it goes behind another piece, we don't know. So it's very difficult to determine, but at one point we thought we saw at least three distinct pieces.

MR. HUBBARD: Okay. And the best evidence that is available shows only a single strike.

DR. BYRNE: Only a single strike and that being of the major piece and all these others.

MR. HUBBARD: Now, you did mention tumbling, but you didn't talk about the rate. I've seen numbers and viewed these videos, of course, several times. The sense from one group was it was tumbling at about an 18-hertz rate, 18 cycles per second. Is that still the case?

DR. BYRNE: Well, that was the measurement that was done. Our partners at NIMA did a very innovative calculation to try to discern the tumbling rate. What they did was look at the different color channels in the film -- the red, green, blue, RGB -- and the foam, being a shade of orange, would stand out better in the red-green channel. So they looked at the different channels and plotted frame-by-frame the intensity of those three color channels and looked at the variation in the intensity. And just in that rough calculation, that variation in intensity came out to be 18 hertz.

Now, we all recognize -- and NIMA did, too -- that that's very crude because we have so few data points to work with, that to try to do a frequency determination from so few would give you an enormous error bar. But that was the only handle that we had, the only analytical handle that we had at all to try to determine rotation rate of that piece of debris. I do not have confidence that the rotation rate was 18 hertz, but that's all we have.

MR. HUBBARD: So the conclusion there is -- would you say it is clearly tumbling but the rate is, we've only got one data point?

DR. BYRNE: It is clearly tumbling and in our analyses we worked with the still frames to get the exact measurements, but you have to work with the motion as well to get a big-picture view of what's going on. And in that motion, when you put the debris in motion, you can clearly seen with your mind's eye -- your mind's eye can integrate between frames -- and you can determine at that time it is tumbling. But to take it the next step and say what the tumble rate is, in an analytical process, that's the difficulty. There's no good way to do that.

MR. HUBBARD: The before-and-after picture you showed of the bipod ramp area where it's dark, light, dark, light -- and I think if you were able to flicker those, it might be even more obvious.

DR. BYRNE: Yes. In fact, I should have brought the movie form of that where they're overlaid, and you can go before and after in a movie format, and it shows up very clearly.

MR. HUBBARD: Do you have an estimate for how large that bipod ramp area is?

DR. BYRNE: That's something we've been working on.

That also is very difficult because when you apply a software routine to do the differencing, the software is detecting the change in the image before and after. Well, when there's so much noise in the imagery, which there is here at that scale, then literally the entire image after looks different because of the noise. So what we've done to date is do a manual estimate of that area of change, and our area was consistent with the size of debris. I believe we were getting somewhere in the order of 30 inches by 15 or 16 inches of the size of change. Again, consistent with the ramp itself, consistent with what we measured.

ADM. GEHMAN: Scott, how you doing down there?

MR. HUBBARD: Ready to yield the floor, sir. I'm probably dangerous because I have a little knowledge about this area.

ADM. GEHMAN: I'm watching the clock.

Mr. Tetrault.

MR. TETRAULT: Greg, last week I think we were using a velocity of approximately 640 feet per second; and I noticed today that 640 is in the lower element of the range that you threw out there. Would you describe what's been going on that appears to have revised your calculations a little bit?

DR. BYRNE: Yes. As I mentioned, that was one of our disappointments, that we weren't able to nail it down better. The first four or five analyses that were done by the various team members came up with a range of total velocities between 610 and 700, and the average of all of those were 640. So that's what we put forward originally. Last week our friends at Marshall came in with a new, different analysis. They used a fundamentally different technique than some of the others. And they came up with a much higher velocity that was in that higher number, 840.

Well, we had a peer review, so to speak, of that and with all team members last week -- and this is brand-new, last week -- and the Marshall analysis passed the peer review, so to speak. We couldn't say, "You're wrong." In fact, I can't point to any one analysis and say it's the best. I can't point to any one analysis and say it's wrong -- because, again, so few data points that we're working with in four dimensions, you can fit almost any curve to those data points and get a reasonable answer.

MR. TETRAULT: Does a higher velocity suggest a smaller piece?

DR. BYRNE: Now, that's straying a little bit away from our area of imagery alone. But in the transport analysis, the next step that we're feeding our trajectory data over to, in order to meet the transport analysis model, that is true. The smaller mass would require a higher velocity.

ADM. GEHMAN: Okay. General Hess, do you have a question?

GEN. HESS: I just have a couple here. Real quick. In your earlier comments, you kind of qualified the bipod ramp as being the source, by saying we have a couple of lines of evidence that indicate. Do you have any lines that indicate that it's not the bipod ramp?

DR. BYRNE: No.

GEN. HESS: Okay. Looking at the video, I know that most of your effort almost entirely was focused at the debris and the debris strike. Have we analyzed the video beyond 81 seconds to see if the debris is --

DR. BYRNE: Oh, yes. What I've shown here is a tiny fraction of the whole analyses that we've been doing; and, yes, we have looked thoroughly at from pre-launch all the way through SRB sep[aration] and beyond. We have looked for any and all indications of events before and after, debris coming off after the 81-second event and so forth. The answer is, no, we don't see any debris other than some normal stuff that we see all the time, SRB slag near the sep.

GEN. HESS: Has your work with all this post-video analysis given you any ideas about what the current state of the art in terms of what the cameras are and what they should be that would have helped you do this better?

DR. BYRNE: The return-to-flight effort is a big one and a lot of that is focused on enhancements, upgrades of the imaging capability of the Orbiter. That's one area that's being closely looked at, what can we do in terms of launch cameras to better our capability to analyze. That's still in work. High-definition TV might be one way that we need to go. The film cameras are good. You really can't do better than film, but we're strapped fundamentally with the problem that here we are on the coast and the Orbiter is moving away from the coast very quickly. So we're going up and away from our camera assets and so just losing sight of it very quickly.

ADM. GEHMAN: I'm going to have to interject myself here so we can get on. We'll reserve the opportunity to ask more questions later, but let me ask two quick ones. This level of photo analysis takes a considerable amount of time. It's taken a couple of months now. Would I be incorrect in saying that this level of photo analysis, for example, these 20- and 30-time enhancements and things like that, would not be available during the 14 or 16 days of the mission?

DR. BYRNE: No. They were, actually. That before-and-after view of the underside of the wing, for example, was something that we had done during the mission and, again, to see if there were any damage. It's interesting that much of what I am presenting here -- we have concluded after three months and thousands of man-hours across the country -- much of what I'm presenting is similar, if not exact, to what we had reported a week after launch, during the mission.

ADM. GEHMAN: That's important. Thank you. And the last thing is you did not discuss what you can determine about the angle of impact with respect, for example, to the plane of the wing or however else you want to measure it. Very briefly, can you say something about the angle?

DR. BYRNE: Yes. The three-dimensional trajectories that we measured were three-dimensional, X, Y, and Z. So from those trajectory analyses we were able to measure a range of impact angles. Almost all of it was in the X. However, we did measure a slight Z component, upward and into the wing, of approximately 0 to 3 degrees. And in the Y component there was a small outboard Y; the range was about 2 to 10 degrees.

ADM. GEHMAN: All right. Good. Thank you very much.

Mr. White.

MR. WHITE: If you could pull up the presentation. I'm going to talk about the MADS data. That's the Modular Auxiliary Data System. This is a separate data system from the operational instrumentation system that we were able to see real-time. This data is only recorded onboard, and we were very lucky to find the recorder intact and the tape in very good shape and we were able to pull that data off.

Go ahead to the second slide.

ADM. GEHMAN: Doug, I think it's useful for the people who have been following this that this is the recorder that the Board has been referring to as the OEX recorder.

MR. WHITE: That's correct.

ADM. GEHMAN: We're going to properly name it here.

MR. WHITE: Well, the MADS system is the name of the entire system, which is the avionics, the electronics to condition and report the signals, and the sensors and the wires connected to them. The recorder itself was an early model of the recorder, which was called the OEX recorder, the Orbiter Experiments Recorder. In the subsequent vehicle, we just called it the MADS recorder; but the version that was on 102 was called the OEX recorder.

On 102, it had the most sensors of any of the vehicles for the MADS system because it was the first vehicle built. Through the years, some of those sensors have broken and fallen offline and during the recent major modification a lot of the sensors were removed or the wires were cut and just left in place, but there were 622 measurements onboard, located throughout the vehicle. Most of those are pressure, temperature, and strain measurements; and I've broken down into three large categories there. You can see the left wing, about 259 -- we had more of our measurements there than anywhere else -- right wing, about 220; and then other places altogether, 143. The avionics to condition all of these signals, all of these wires run to the mid-body, about Bay 8 of the mid-body, and then they're recorded actually on the OEX recorder, which is in the crew module. As I said before, none of this data is available to us real-time during the flight.

Next slide, please. First thing I'm going to talk about here is failures of this data. What we see mostly in this data is all of these sensors beginning to fail and going offline, with a wildly variable signature where they oscillate between off-scale high and off-scale low. To us that indicates that the wire bundles that contained these measurements in the left wing were being burnt through and being destroyed. Most of that happens between about 480 seconds to 600 seconds from entry interface; and for those of you working in GMT, that would be 13:52:09 to 13:54:09 in GMT time.

ADM. GEHMAN: Entry interface being?

MR. WHITE: Entry interface is when you first start to encounter a little bit of the atmosphere. That would be 13:44:09. So I broke that down between temperature, pressure, and strain gauges in the left wing, the right wing, and then other measurements we were interested in. You can see the numbers there.

What this chart tells us is that we saw, surprisingly, some failure signatures over in the right wing. There were a number of right wing pressure sensors that went offline, about 30 of them, and that is because they have commonality with left wing measurements, they share a common piece of avionics in the avionics boxes that condition the signals, and as things were being shorted or destroyed in the left wing, that affected measurements in the right wing. So we've been able to tie those events together.

The other thing you notice from this chart is that there were two measurements only that did not eventually fail in the left wing, and those hung in all the way through the loss of vehicle. Those two measurements are strain gauges, which are on the wing surface or on the spar actually that runs in front of the wheel well. That's the 1040 spar. If you look at the wire routing for those particular measurements, those two measurements peel off from the main bundle in front of the wheel well and stay there as opposed to running farther back into the wing. That tells us that the damage that was going on was farther back in the wing and that the wire bundles were being burned farther back in the wing rather than up near the front of the wheel well, because those two measurements did hang in there.

There were 241 measurements that are what we call snapshot measurements. By design, they only take data for a few seconds at a time and then they go offline and the recorder goes and looks at something else. So you only see these little snapshots, bits of data, and it's very hard to determine whether those are failing or not. We suspect that they failed the same way that the other measurements in the left wing did, but we just don't have the data that will show us that.

MR. WALLACE: Can you discuss the time sequence -- maybe you'll get to this later -- with respect to the first off-nominal indications in the telemetered data?

MR. WHITE: Yes. I'm going to talk about that and, depending on how much time we have, I have another version of this which, last time I was here, I talked about the operational instrumentation data in sort of a graphical sequence, marching through the timeline. I have one of those available if we have time to get to that today, but I thought I'd start off with showing you the data and showing you where it looked off-nominal and we'll talk about the sequencing, too.

Next chart, please. Just real quickly all I wanted to talk about in this chart here was we said we saw these measurements oscillate wildly between off-scale high and off-scale low, and can we explain that from an instrumentation system point of view that these were, indeed, failure signatures of these measurements and not real data that it was trying to tell us. We have done that. We've had our instrumentation system experts go and look at how the system could fail and if you shorted this wire to that wire, could you get the signature that you observed in the data. The answer is, yes, you can pick from what we saw in the data just about any combination of shorting or variable resistance between wires to get the observed data.

The other thing we see is that sometimes after this oscillation, off-scale high, off-scale low, that it looks like a measurement returns to a normal state or something that reads real data. This has to do with bias, the way the measurement was set up and its residual voltage in the system; and it should not be interpreted as real data. So after you see the data do one of these wild swings, you shouldn't believe anything that you see afterwards.

Next chart, please. Let's go one more. We'll concentrate on the leading edge of the left wing which is, as Greg told you, where we narrowed down the strike to the Panel 5 through 9 region. We did have some measurements in the left wing, near Panel 9 and 10. We had two temperature measurements, one in the clevis area where the RCC attaches between Panel 9 and 10. That's on the outside of the spar but inside of the RCC. We had another temperature measurement on the back side of the spar, so inside the wing. There's a third temperature measurement in that area, which is on the skin just behind Panel 10; and there is also a strain gauge measurement in that area which tells us the relative strain in that spar. Those are all the ones that you can see highlighted right in this area here.

I've also highlighted the wire run that feeds measurements along the wing leading edge. There's a group here and a group out there and some here and some back in here. Each of those measurement numbers and each of those times is the time when those went offline. So you can see the ones in the leading edge went offline almost all together. The only one that stayed around for a while was this one temperature measurement here on the back side of the spar. That hung around for 522 seconds after entry interface, but the rest of them failed early and we'll talk about those sensors right there at Panel 9 and what they showed us. Again, that tells us that something was coming through the left wing and destroying that set of leading edge bundles first before it got to some of the other sensors in the wing.

Next chart. This is just a wiring diagram of the back of the wing. If you start over here -- these are from photos from the last major mod of *Columbia*. This is looking on the side of the wheel well. Here are some major bundles here that run down the side of the wheel well, but the bundles for the leading edge of the wing go off this way and you can see there's several different bundles here run across the wing. This is the back side of Panel 9 and 10 region, which is down here; and I've got some more pictures of this later, showing some of the measurements. This particular one is a pressure measurement and a temperature measurement. They go through the wing here, and then they run on down the back side of the wing.

Next chart, please. This is just a close-up of the bundles along the side of the wheel well inside the left wing, and we've just numbered them arbitrarily. We started at the front side, but they change their routing and switch over each other. So the order that you see here happens to be 1, 4, 3, and then this is the wing spar and you can see the wires going down the leading edge of the wing there.

Next chart, please. This particular chart is in the Panel 8-9 region, and I highlighted the split there. This is the back side of the wing, looking forward. These are wire bundles running down the wing spar. We, again, arbitrarily labeled these A, B, C, D, E, and you can see measurements there and which bundle they were in, Bundle A, C, or D, and when they failed. Just lining these up in time order, it appears to us that the damage was maybe higher or at least the wing spar began to fail higher up before it worked its way through.

There's one measurement here at the bottom, the one that lasted the longest. We're not quite sure because it's very difficult to tell from the photos whether it's routed in Bundle D or Bundle E. That's this temperature measurement here, which is under this red piece of tape. This is the temperature measurement I mentioned that's on the back side of the spar.

Next chart, please. This is just a graphical way to look at all of those wire bundles failing. We pulled out the ones from the leading edge which we showed in purple; and you can see how quickly those failed, starting here about 480 seconds after entry interface. You can see how quickly those failed relative to the other bundles that I showed you, the larger bundles that ran down the side of the wheel well, Bundles 1, 4, and 3. Also you notice that Bundle 3 had the two measurements that never did fail, had 117 measurements in that and only 115 failed. That's because two of those peeled out of that bundle very early in front of the wheel well.

I also tried to indicate, just for timing, some of the other major events in the timeline that we're familiar with that we were able to get from the real-time flight data. So you can compare when these events were happening relative to those other events. For example, the first Orbiter debris event is way down here.

Next chart, please. We'll talk about some ascent data that we got from those Panel 9 temperatures. This again is just a graphic to show you where things are located. This is a skin temperature measurement, which is on the skin behind Panel 10. We had two temperature measurements, one in front of the wing and one behind the wing, and then we had one strain gauge measurement right here. Then in a side view you can see the one that's in the clevis there of the RCC and then the one that's on the panel behind.

Next chart. Again, just to get you oriented physically, we're looking at the back side of the wing, this is the strain gauge here about the center of Panel 9. There's the temperature gauge on the spar. This is the feed-through for the temperature gauge that goes inside the RCC but outside of the spar, and then there's that lower skin temperature measurement that I was talking to you about that passes through the skin right there.

Next chart, please. So this data compares the temperature rise for the Measurement 9895 -- that's the one on the back side of the spar -- to data from other flights. The RCC cavity is vented. So as you go uphill, the air comes out of the cavity. So you normally see a cooling kind of a trend, which is why all these measurements drop down a couple of bits. Then as you go through ascent, you get ascent heating and the measurement tends to warm up a little bit.

What we see here on STS-107, which is the black line, is it drops down a few more bits than the other ones do and it rises back up a few more bits than the other ones seemed to do. Now, this in itself is not conclusive that we actually had a hole in the wing at this point and that we did have abnormal heating on this spar, but it's just something a little bit different than what we have seen. We've looked at some more data than what I presented on this chart. We have found some flights where we were able to see the dip maybe as big as this one was, but we still haven't found any that rose back up quite as much as what we saw here.

GEN. BARRY: Can you argue that this is definitive evidence that there is a breach?

MR. WHITE: No, I cannot argue that it's definitive evidence; but if I were to put this in a big scenario that says there was a breach at this time, then this certainly would be supporting evidence for that. But I would not hang my hat on this evidence alone. This is not strong enough to say that there definitely had to have been a breach. But it's not inconsistent with the fact that there might have been a breach at this time.

Next chart, please. This is just comparing in numbers what I just said, the other flights, how many bits down it went and how many bits back up. For 107 here, we did indicate that it's a little bit different than other flights.

Next chart, please. Let's go talk about the entry data. Again, we'll talk about the leading edge area here on Panel 9. This is an underside view. There's also pressure measurements --

MR. WALLACE: Can you sort of equate bits to degrees?

MR. WHITE: I believe, on that measurement, one bit is about five degrees, I believe. On the order of five or six degrees.

So there were some pressure measurements we'll look at back here and other measurements along the side wall and the lower skin, as well. Again, that's the inside of the RCC, showing the two temperature measurements we had there.

Next chart. This is that lower skin measurement that's just behind Panel 10, and we compared it to other measurements on this flight. You can see that one gets a little hotter and then the next chart will show you that this area right in here is anomalous heating. This is a little hotter than that measurement ever got on other flights during entry, and this little bump right in this area here also appears to be a little outside of our experience base.

Next chart, please. Here's that same measurement in the black, plotted against that same measurement for other flights. You can see this area here that I talked about is a deviation from the heating we've had before. This measurement normally comes up and flattens off. So we saw a little bit higher. Then all of this stuff here you see, that's the failure signature. That's where the measurement goes unreliable, where we believe the measurement itself or the wires leading to the measurement were being burned through; and then any of the data out here you can't believe, even this little bit out here at the very end. You also see this little bump here, which is a little bit different than we've seen before.

Next chart, please. This is just some graphics showing you some of the temperature measurements along the side wall.

Next chart, please. Some more toward the aft.

Next chart. We'll talk about this data. Here's some of that data, plotted for side wall temperatures. You see some off-nominal heating in these two particular measurements. These are on the side wall fuselage. You can see this measurement rising here, and this one rising here is off-nominal heating. This is not something that you would have seen from other flights.

Next chart, please. Again, these are measurements on the OMS pod. We saw a curious effect on the OMS pod. We saw lower heating for a portion of the flight and then we saw higher heating. So that tells us the vortex that comes along and normally would heat the OMS pod was moving around. It was off of the OMS pod early, when it normally would have been there, and then it was more intense on the OMS pod later. So this black line here, these measurements are actually below where they would have been for this period of time in other flights; and then where all these arrows are about here, all of these measurements start going high again and getting higher heating than they would have been in other flights.

Next chart, please. Getting back to the wing leading edge at Panel 9, the approximate area where we believe the impact was.

Next chart. Again, just the back side view to help you remember. This is the strain gauge, temperature gauge inside, temperature gauge outside, and then the lower skin temperature.

Next chart. So I put all of those on the same graph, and this is the graph that says the first events we saw happening were in this area. These are earlier than the wheel well measurements that I talked about last time. The first thing we see is this strain gauge measurement go up and off, and this is the off-scale failure again. But about 290 seconds is when we see the start of the off-nominal rise.

Here you see the two temperature measurements in the blue and the purple. They began rising earlier than we've ever seen before; and again, they all failed about the same time right here in this region. This one other strain gauge measurement that I showed you was one of the snapshot measurements. So you only have a little bit of data in here and here. You can argue that this might have been off-nominal, but we just don't really have enough data to say. Definitely this part here and then down before it failed was off-nominal, and this is an indication that because of temperature and heating in this area that the strain and the load was shifting and that there was something happening to the leading edge of the wing in this region, the Panel 9 region. Again, as I said, this is the earliest indication -- about 290 seconds after entry interface -- this is the first indication of something going wrong that we saw in the vehicle data. This measurement, again, I already showed you a couple of times. This is the skin temperature measurement, again showing deviation. There's this little hump here and then higher heating in this part before it goes off scale, as well.

ADM. GEHMAN: In front of me, I have the advantage of having the Rev 15 of the timeline; and what you classify as start of peak heating occurs at Time 50:53, is what arbitrarily is called "start of peak heating," which works out to entry interface plus 400 seconds. So you are seeing temperature rises and some strain prior to peak heating?

MR. WHITE: That's correct.

ADM. GEHMAN: So what's happening is that as the vehicle heats up, so are these leading edge.

MR. WHITE: Right, these leading edge inside the RCC, where we wouldn't be expect it to be heating up, before peak heating -- I mean, peak heating, like you said, is kind of arbitrary.

ADM. GEHMAN: It's still hot.

MR. WHITE: It's still hot. We have heating all the way from the beginning of entry interface. So what we're seeing is that heating manifesting itself inside the RCC cavity where we would not expect it to manifest itself. So again, this is a good indication that at this point we did have some sort of breach in the RCC.

Any more questions here? We'll move on and talk about the pressure data a little bit. Next chart.

I'm not going to go through each one of these sensors, but you can see they're all arrayed in more or less the same Y location away from the fuselage. This is the lower surface. We also have a lot of pressure measurements on the upper surface that I won't talk about. This band right here, the forward 8, we see some interesting measurements here; and I'll go through that.

Next chart. These are on ascent. So we're back to ascent now and looking at the pressure on ascent to see if we could determine anything going on on ascent from these pressure measurements. What we see is all the measurements decaying, as you would expect. As you go uphill, the pressure gets less and less; but there's one measurement here which is behind the Panel 9-10 region. We see this bump at about 84 seconds or so, then coming back down, and then another spike farther out. Now, to us that's an indication -- we don't worry so much about the particular value that it went up to but the fact that it took two jumps is an indication to us that something hit that sensor, either clogged the port or moved it or did something to the sensor to cause it to have those two spikes.

Also there's another sensor. There are two types of pressure sensors. One's called a Statham sensor, which is mounted on the surface of the skin and has essentially a very short tube that goes through the tile to sense the pressure. Excuse me. I said those backwards. That's the Kulite. Then the Statham sensor is mounted inside the vehicle, away from the point where the tube goes through, and has a rather long tube running inside the vehicle and then poking through the skin. So the Statham sensor, which happens to be right next to this, we don't see this kind of a spike on, because the actual sensor and wiring and everything was inside and protected; but if you had something hit in the tile where this Kulite sensor was mounted right on the skin, you could have done damage to it. So this data tells us that we did have some kind of a hit in this region, but it doesn't tell us anything more exact than that.

GEN. BARRY: Two quick questions. We know the impact occurred at 81. So this is about 85, 86…

MR. WHITE: Right. So this number is a little bit downstream from the leading edge of the wing. So there could have been something tumbling or coming back a few seconds later that affected this sensor.

GEN. BARRY: When you say tumbling back, you mean like something could have gotten loose and then just rolled back?

MR. WHITE: Right. It could have been debris. It could have been that the tile where the sensor is was damaged and then suffered some further damage, some bits of it came off or part of the sensor became de-bonded somehow or was affected. So there could have been a delayed reaction from the hit.

GEN. BARRY: We know that sensor's not 100 percent reliable. Have we got any indications on any previous flights where we have these kinds --

MR. WHITE: No, we have never seen these kind of spikes before on pressure sensors.

MR. HUBBARD: Just to be clear, again, you're not measuring here -- what you're saying is not a pressure change. You're saying it is something, it's an electrical signal as a result of --

MR. WHITE: Well, it's possible that that was -- especially the first one. The second one is a lot harder to explain as a real pressure change. It's possible there was some sort of real pressure change in this region here. Again, that would be a result of the instrument being affected and maybe the flow around that instrument being changed. So there was temporarily a higher local pressure around that measurement; but it also could be just an effect of the instrument being damaged, as well.

ADM. GEHMAN: And you're confident that the timeline differences between the camera time hacks and the MADS data recorder time, that you don't have a second and a half of --

MR. WHITE: No, these are pretty good times. So whatever it was here was a little bit delayed from the impact that Greg told you about.

Next chart, please. This is another measurement which was again in this same region farther back from the leading edge where we believe the strike happened and you can see the pressure here -- this is compared to other flights of *Columbia*. You can see the pressure there just kind of decayed off a little bit faster. Again, that could have been from debris plugging the tube or something like that to cause it to have apparently lower pressure earlier than the rest of the flights, the earlier flights would have shown.

Next chart. Finally, there are three measurements, again in this same band, that show a very odd behavior around 102 seconds here. Two of them go down, come back up; and one of them makes a jump up. This one we haven't been able to explain yet as any kind of hit or anything, there appears to be some sort of glitch in the instrumentation system. Again, it's something we've never seen before and it's odd that all three measurements, which are not -- two of them are located together. This one and this one are close together. This other one's a little farther up. It's odd that they would all have the same behavior at the same time and then return to what appeared to be sort of a normal reading. Just kind of connect the line here. It looks like it came back to where it would have been. So we're not sure what to make of this yet. But it's something else we're still looking at. Again, this is ascent data; the scale along the bottom is seconds from liftoff.

That's all I had, as far as showing you pictures of the data. If you wanted to go in and look at how these things relate in time, we can go into the timeline charts.

ADM. GEHMAN: Let's see if there are any questions before we do.

MR. TETRAULT: Is it possible to go back to your Viewgraph Number 9?

MR. WHITE: Sure.

MR. TETRAULT: I have two questions. On the upper right and the lower right, there are two pressure sensors, if we get back there.

MR. WHITE: Okay.

MR. TETRAULT: See the pressure sensors in the upper right and the lower right? Those have wires which run back into the bundles, but those are also cut at Times 495 and 497, which to me would suggest that the breach had to be close enough to --

MR. WHITE: Talking about it might have been over here somewhere. Right.

MR. TETRAULT: Right. You had mentioned that you thought the breach was in Number 9.

MR. WHITE: Well, from Greg's data, it's anywhere from 5 through 9. To get a little off of this, our forensic evidence says that it was more likely in this region of Panel 8. So, yeah, it's very possible that it was over here and got these wires.

MR. TETRAULT: That's what I'm trying to get at. To catch that wire right here and this wire right down here, you would probably have to have some breach that would be in this area or further over to the right.

Now, the other question that I have is this one here, Temperature Sensor 9895. You indicated that there's a certain degree of ambiguity as to whether it comes down and goes out this run or goes back up.

MR. WHITE: Right. It's hard to tell whether -- I don't know if you can see this or not. The wire runs down here. It's hard to tell whether it doubles back in this bundle here and runs up this way, or whether it just stays in this bundle and goes that way.

MR. TETRAULT: It is, however, I've been told, that you have a specification requirement that does not allow you to make a pigtail like that on a wire run, so that it would be more likely that, in fact, this wire run goes down this route.

MR. WHITE: That's correct. Yes, sir.

MR. TETRAULT: I see that as important because this wire run comes back up and joins these wire runs at Panel Number 7; and because of the lateness of this sensor going off, it would tend to preclude the breach from being over here in 7 since it joins the other wire bundles.

MR. WHITE: That's correct.

MR. TETRAULT: Would that be a good assumption?

MR. WHITE: That's a good assumption, yes, sir.

MR. TETRAULT: Okay. Thank you.

MR. WHITE: Did you want to get into the timeline?

ADM. GEHMAN: Yes. Please. I'm thinking we have about 20 more minutes.

The two leading edge temperature sensors in the vicinity of RCC Panel Number 9, which are labeled 9910 and 9895, I think. I was looking through, you did not actually plot that temperature rise?

MR. WHITE: Yeah, let's see. If we go back to -- I'm sorry, go back to Chart 26. Sorry to back you up. Let's see, can you get Chart 26 of the previous presentation back?

Those are plotted here. It's just difficult to see because of all this noise from the strain gauge. They're the two: the purple and the blue. Sensor 9910 is the blue, and 9895 is the purple. So you see the blue begin to rise here. That's the one outside the spar, in the RCC cavity, and then followed behind by a rise maybe somewhere in here for the one inside the cavity, and then both of them get very hot very quickly and then begin to go off-scale. As I said, in this particular graph, because I plotted everything together, it's masked in here by the failures of the strain gauge. Here's the first temperature rise and then the one outside the spar; and then here's the temperature rise, maybe somewhere in this range, of the one inside the spar.

ADM. GEHMAN: I want to make sure I'm reading this right. In the case of the blue one, which is 9910, which is outside the leading spar, both the temperature rise and also the time scale is significant in that this almost certainly could not be a cut wire or burning insulation or a slow ground or --

MR. WHITE: No, sir, we believe the data is real data up until right here, somewhere in this area here; and then it becomes very difficult to tell when it starts to go vertical.

ADM. GEHMAN: Now, in the other one, 9895, which is the lower one, that argument's a little bit harder to make because both the temperature rise is --

MR. WHITE: It's more subtle.

ADM. GEHMAN: It's more subtle and it's varied over a small period of time. But your conclusion is that that also is a legitimate temperature rise.

MR. WHITE: Yes. Both of these we believe are real, to somewhere in this point here. We believe those are real indications that we had heat inside the wing at that point. Now, whether or not the breach was farther down and we just had convective heating coming down to that part or whether the breach was nearby -- and you heard some of the other arguments why it should be farther upstream, maybe in the Panel 8 region -- but we do believe that was real evidence of real heat inside the wing.

ADM. GEHMAN: Now, for the temperature sensor outside the spar, the area between the spar and the cavity in there between the spar and the RCC, it's hot in there.

MR. WHITE: Yes.

ADM. GEHMAN: Because the RCC is not really an insulator.

MR. WHITE: Right. The RCC re-radiates. We have a lot of insulation inside the RCC, in the front of the spar, to protect the spar and protect it from the re-radiation of the RCC; and that temperature sensor is buried down underneath that insulation.

ADM. GEHMAN: That was my next point. 9910 is actually buried inside the insulation.

MR. WHITE: Yes, sir. It's down in the clevis where the panel would attach, and then there's lots of insulation over top of that.

ADM. GEHMAN: Right. Okay. Thank you very much. Why don't you go ahead with your timeline.

MR. WHITE: Let's see if we can get the other presentation up. All right. This is similar to the timeline I showed you the last time I was here for the operational instrumentation data and we've mixed in some of those timeline points here. There's an awful lot of ones here. I'll maybe skip some, and there's some that I just left out of here even putting this together, just to try to make it more brief. This is not every single event we have on the timeline and I'm not going to walk you through every single failure of every single sensor here, but I'll try to look at this in a big picture.

Next chart. Now, these are some of the sensors that I decided to plot. I did not plot all 622 of the MADS measurements, just some of the ones that are more interesting. We also plotted some of the OI measurements that you're familiar with here in the wheel well and some of the ones in the wing. Again, these are the sensors that we were just talking about here, and you'll see this area start to have things happen first.

We also tried to keep a color-coding, trying to show what was on what bundles. The blue ones here on this blue bundle which is Number Three which runs down the side of the wheel well and also splits off and runs along the front of the wing. Bundle Number Four is this pinkish one. Bundle Number One is the yellow one, and you can match those up with the pictures I showed you earlier.

As we walk through this, I'm going to keep score over here on how many sensors in a bundle had failed, but you won't necessarily see a dot for each one. So sometimes you'll see these numbers jump a lot and you won't necessarily see that many dots change color.

Next chart. So this is now our new first event that we have at 13:48:39 or 270 seconds -- I believe I said 290 in the other one. Because the rise is so small, you can put a tolerance around the front of that. But that's the strain gauge measurement on the front spar there near the Panel 9-10 interface and we see that begin to rise off-nominal. That's real data we believe that says something is happening to the strain in the wing leading edge spar at this time.

Next chart, please. Again, we see that first rise we just talked about, 9910. That's the clevis. It begins its very subtle rise.

Next chart, please.

ADM. GEHMAN: And that's only 20 seconds now.

MR. WHITE: Right. We've only gone now to 13:48:59. So not very far in the time. As we get closer in, you'll see lots of events start happening within seconds of each other.

The next thing we notice again from the MADS data which we did not have before is now we have an OMS pod temperature sensor which is now showing cooler. As I talked about when I showed you the data, some of those temperatures went down. That says the vortex has now been disturbed and is not hitting the OMS pod the way it normally does. So this temperature here showed a little blue, to indicate it's cooler than it normally would have been.

ADM. GEHMAN: Even though you're not going to show every sensor of all 600 and whatever is was, you have more than one sensor that does that.

MR. WHITE: Yes. We have several in the OMS pod, and I think I have some of them highlighted in here.

ADM. GEHMAN: So it can be corroborated.

MR. WHITE: Yes. It's not just one lone sensor doing this. We see cooling trends on a number of OMS pod sensors, we see them on the side wall temperature measurements here, and then we see off-nominal heating trends as well in this region.

Let's see. Go on to the next one. All right. This is a comm dropout. We're still way out off of the coast of California.

Next chart. Another comm dropout.

Next chart. This is another corroborating measurement. This is payload bay surface temperature again going cooler than it normally would have been at this point in the flight. Shows a little blue dot there.

Next chart. Another comm dropout.

Next chart. All right. Now we see the lower surface temperature. This is the one behind Panel 10 on the surface, and it's starting to rise. It says we've got some kind of heating that's now getting to the surface from probably through conduction through the skin of the vehicle. It's starting to heat that up right there. Again, all of these events are now earlier than anything we had seen in the operational instrumentation data before.

Next chart. Comm dropout.

Next chart. Another comm dropout.

All right. Now, we're back to the spar temperature itself. This is the one on the inside. Now it's beginning its rise; and we're at 425 seconds past entry interface, or 13:51:14.

ADM. GEHMAN: Once again, peak heating is arbitrarily defined as some number 40 seconds ago, I think it turns out 400 or 404 or something like that.

MR. WHITE: Yes, sir.

ADM. GEHMAN: So we are now at peak heating.

MR. WHITE: Yes, sir, we are now at peak heating.

All right. Now we see OMS pod temperatures where we're seeing cooler measurements here and here. We're seeing hotter measurements than we would expect, a little further back on the OMS pod. So right about here.

All right. Next chart. Somewhere in between maybe a slide or so ago that I showed you and maybe a slide or so from now, we believe that the wing leading edge spar got breached. It's hard to tell from the data exactly where that might have been. In a few seconds, I'm going to start showing you a lot of sensors dropping offline. So we know that it had to have breached before the sensors drop offline. It's difficult to tell exactly when that wing leading edge spar was breached, though. This is at 52:05; and this is now where we're starting to notice something different in the aero. This is data that we had seen before, and it could correlate with a time that we started to make the hole bigger or had burned through the wing leading edge.

Next chart. Another comm dropout.

Next chart. Now, this is something different, and we can't really explain this yet. We've tried to get our thermal folks to explain it, and they can't. We've tried to get our instrumentation folks to explain this instrumentation failure, and they can't. We did not see this data until we got the MADS data, but there is a temperature measurement up where the chin panel and the nose cap attach, and one of those measurements began an off-nominal rise. If you looked at the plot of the data, you'll see it going on a normal kind of slope and then it takes a jump, a higher heating rate, and then for some reason it cools back down and joins where it would have been at that time if it had just kept going and continues on its way.

So we don't know what to make of that either physically -- it's hard to explain something heating up and then cooling down and getting back to exactly where it would have been if it had kept on its same rise rate -- but instrumentation-wise it's also difficult to explain it. It's different than the vent nozzle temperatures that we talked before from the OI data. There when you see a higher heating rate and they cool back down again, they're offset from their slope where they would have been. So that extra heat stayed there and they're a higher temperature but the same rate. Here it actually comes back to the same temperature it would have been and then resumes. So it's kind of odd, and we don't know how to explain that.

Next chart. All right. These are the first measurements that we start to see go offline. So at this point here, 5216, we know the wing spar has been breached and that we are burning wire bundles. So there's one back in the back of the wing here. This is a left wing upper-surface pressure that goes off and a corresponding right wing upper-surface pressure that shares a common power supply in the MADS system. Both of those were affected.

ADM. GEHMAN: Doug, can I ask you to go back one or two. I want to go back to the first aero event, I think, which is 5205, I think. First clear indication of off-nominal. I happen to have your detailed line here. The QBAR and the pressures here are still extremely low.

MR. WHITE: Extremely low. Yes, very low.

ADM. GEHMAN: We're talking, according to this, 22 pounds per square foot or something like one tenth of a pound per square inch.

MR. WHITE: Yes, sir.

ADM. GEHMAN: So even though we've got some aero events, the aero pressure --

MR. WHITE: It's less than one percent of atmospheric pressure, yes.

ADM. GEHMAN: It's practically nothing.

MR. WHITE: Yes. That's correct. Yet we can see an effect in the way the vehicle's flying.

ADM. GEHMAN: Also, in about another 11 seconds, we're going to project that the heat penetrated the spar. So even though we've got extraordinarily low pressures here -- in other words, we don't have anything like a jet, like a high-velocity jet here.

MR. WHITE: But the amount of air that's there is very, very hot. There is a lot of heat there.

ADM. GEHMAN: A lot of heat.

MR. WHITE: And the wing spar actually may have been penetrated at this point. In another few seconds, as you said, we'll start seeing sensors drop offline. So we know that the wing spar was breached somewhere before that. The timing of how soon it was breached versus how soon wires start to drop offline, we haven't nailed down yet. So it could have been breached right here at this time.

ADM. GEHMAN: But this is almost exclusively a thermal event at this point.

MR. WHITE: Yes, sir.

ADM. GEHMAN: I mean, it becomes an aero event later.

MR. WHITE: Yes.

MR. TETRAULT: You have done some testing, heat-testing of Kapton wiring and how long it takes.

MR. WHITE: Yes, we have.

MR. TETRAULT: It's my understanding -- and I haven't seen any data -- it seemed, at 2,000 degrees, to take quite a lot a long time.

MR. WHITE: Depending on where the bundle is or where the wire is and how big the bundle it's in, because you know it provides some heat sink and stuff, there's a lot of variables in there. They're still trying to devise some more testing to get a better feel for the kind of heat rates you can put into bundles, but it's not inconceivable that you could breach the spar and less than 30 seconds later you could start burning wires.

ADM. GEHMAN: As we did.

MR. WHITE: Yes, sir.

GEN. BARRY: One quick question on the nose sensor, just to avoid leaving the wrong impression. We've had failures before in MADS data sensors.

MR. WHITE: Oh, yes. We have failures, yeah, maybe a couple per flight, where the sensor fails for one reason or another.

GEN. BARRY: We can tell the difference between a failure and one that --

MR. WHITE: Yes, sir. The folks that are used to looking at the data at every flight can tell when it's failed and we put them on a list and depending on how much time we have in the turn-around -- because these measurements are all Crit 3, that means that we don't need them for anything in flight. It's good data to have and engineers like to see this data, but we don't rely on it for anything in flight. So if they have time to fix them during the turn-around, they'll fix them. Otherwise we'll just fly with a piece of paper that says this one's broken and we'll fix it when we can.

GEN. BARRY: A point to be made. The ones you're showing in this briefing are ones that you determined --

MR. WHITE: Yes, sir. These were all working measurements. Right. I'm not showing you any that were determined to be bad here. Yes, sir.

Let's see. Keep going a little forward. Okay. We talked about the clevis. We talked about the first sensors going offline.

Next chart.

DR. WIDNALL: Could I ask a question? Where is the wire that they share in common? You said they both went offline at the same time. You said they share a common something or other.

MR. WHITE: Well, the power supply and the avionics for the MADS would be about here in the mid-body. But the wiring that they would share would be wiring that comes from here into the avionics box and this wiring here, this blue wiring that runs along the spar and then connects in through here to the mid-body and then over to the MADS avionics boxes. Because we believe what happened is because of a short or a burn-through in this blue bundle here along the leading edge, that it pulled down the voltage to the power supply, which also dropped this off.

DR. WIDNALL: Because otherwise it's sort of mysterious.

MR. WHITE: Yes. We believe we can correlate the right wing ones with the left wing ones where they have failures.

This particular point here, 52:17, is the previous earliest measurement that we had seen. This is from the OI data. This is where we thought things were beginning to happen. Again, if the wing is breached somewhere in this area and we have hot gas entering the wing, there may be enough that gets around into the wheel well just a little bit to cause that temperature. You remember that was just a bit flip and it was very small; but it is possible, with heat coming in through the wing, that we are now seeing that sensor begin to respond.

ADM. GEHMAN: Now, that is significant, what you just said. The temperature rises that we saw on those two spar temperature lines were measured in big numbers, hundreds perhaps.

MR. WHITE: Yes, sir. And I indicated those by making these dots red which says that these were quite significantly out of what they should be at this time, greater than -- well, let's see, I guess in the color-coding here it would be greater than 30 degrees by this time. It gets significantly hotter. Here this is a very small temperature range.

All right. Next. This is a strain in the spar, the 1040 spar that runs in front of the wheel well. Again, we believe we're seeing off-nominal measurements here because of the shifting loads within the wing as the heat begins to damage things; and this is one of the two measurements that never did drop offline.

You notice here in my count I'm starting to show how many have failed in Bundle Number Three, which is the blue bundle here and down the side.

Let's see, next chart. A couple more sensors drop offline. Again, these are all connected to this leading edge bundle here again, again, which is the one that you would expect to fail first, the ones I showed you in the back of the spar, and probably haven't gotten over to start burning any of these yet.

Next chart, please. Okay. The measurements for the temperature here on the leading edge. The surface temperature behind Panel Number 10 on the lower surface and the one in the clevis are starting to look off-nominal. It looks like they're being damaged at this point and that we can no longer trust the data.

Next chart, please. This is the spar measurement itself and, again, the lower surface pressure measurement here showing, again, unreliable data, showing damage trend to the wire.

Next chart. Another comm dropout.

Next chart. You notice we're still at 52 minutes and only 27 seconds now. We haven't gone very far forward.

ADM. GEHMAN: We're going to go second by second here.

MR. WHITE: Pretty much. So if you want to jump a little faster. But you can also notice that my count is increasing here. I've got two failed in Bundle Number One. I've got 20 failed in Bundle Number Three.

ADM. GEHMAN: Well, just go ahead and just clip through them. You don't need to describe each wire that breaks because the next significant events --

MR. WHITE: Next chart. This is OMS pod temperatures. These are the supply water and waste water vacuum vent nozzle temperatures that we talked about before. Showing a little off-nominal heat rise. Again, we still haven't been able to explain how that correlates with anything that was happening back here in the wing.

GEN. BARRY: Another point to be made is this about the time we had our first telemetry reading on the previous operational sensor?

MR. WHITE: Yes. That was actually a few seconds before, when we saw this one in the wheel well rise.

GEN. BARRY: 52:17. So all this that you've shown is preceding.

MR. WHITE: But it is very close. Yes. This is only 52:32 now.

Next chart. Okay. There's another measurement offline.

Next chart. There's some brake temperatures. Again, we had seen these before. That's starting to rise. More heat in the wing. More heat in the wheel well.

Next chart, please. Okay. Supply water dump nozzle.

Next chart. Another comm dropout.

Next chart. The attach clevis now went back to nominal.

Next chart. This is the one on the temperature on the spar. Now it's starting to go offline; and we're still at 52 minutes, now 51 seconds.

Next chart. More sensors offline.

Next chart. Vacuum vent nozzle begins to rise.

Next chart. Now that front spar temperature finally does go offline. So the size of the hole here must have increased enough to take out that sensor.

Next chart. Some more skin temperatures going offline.

Next chart. This is where we start to see roll moment happen. So now the damage into the wing has begun to be serious enough to affect the roll of the vehicle.

Next chart, please. Some more sensors offline. Now we're only at 53 minutes. We've barely gone a minute, and you can see the wire failure counts are pretty high -- 9 of 11, 99 of 138, and 6 of 25.

Next chart. This is an OI measurement that went offline.

Next chart. Some more. These were ones from the OI that had gone offline.

ADM. GEHMAN: Now, these are the four elevon actuator temperatures that went off essentially at the same time.

MR. WHITE: Yes, sir.

ADM. GEHMAN: And this was then noted in mission control in conversations.

MR. WHITE: Yes. These are the ones that alerted something. The MCC began to notice something that was wrong, that these four should not have failed all nearly at the same time.

ADM. GEHMAN: So you might say this was the first indication people on the ground had any idea that anything was happening that was unusual.

MR. WHITE: Yes, sir. That's correct. The temperature rises that we had in the wheel well were pretty subtle and were hard to pick up if you didn't know -- you know, it's only going back and looking at it that we have been able to pick this up. But these measurements failing here were picked up immediately and, as you said, were the first indication to the folks on the ground that they had a problem.

ADM. GEHMAN: And depending on what displays were being displayed at MCC. So even though those wheel well temperatures are telemetered to the ground, they may not be actively looked at at every instant.

MR. WHITE: Yeah. I can't answer that. I can't be sure what the MCC looks at routinely.

ADM. GEHMAN: We do know, based on the video and audio recording in Mission Control, that the loss of these four elevon actuator line temperatures was noted and reported and this is when the conversation started.

MR. WHITE: Yes, sir. And then this, position-wise, we're still not quite at the California coast yet.

Next chart. OMS pod temperatures now start to rise. This is one that was cooler earlier. It's now starting to rise. You can see other parts of the OMS pod. This one is still cooler, and this one is very hot. So we've shifted the vortices around considerably.

Next chart. More pressure measurements going offline. Strain measurements.

Next chart. Some side wall fuselage temperatures rising now. Some of these had also been cooler and now are getting hotter.

Next chart. Again, another side surface temperature behaving badly.

Next chart. Comm dropout. Now some more strain measurements and elevon return line temperatures going offline.

Next chart. Now my supply water dump nozzle, my vacuum vent nozzle returned to nominal.

Next chart. Another hydraulic system elevator -- excuse me, elevon actuator return line temperature going offline.

Next chart. Now, the strain. This is the other measurement that hung in there but, again, is showing an off-nominal reading in front of the wheel well on this spar. Again, it tells us that the load is being redistributed within the left wing. I can't tell you exactly what damage would have caused these measurements to behave the way they did, but there was damage and it was causing the load to redistribute.

Next chart. This is now the first debris sighting. We're over California, and so this was the first debris event. Again, it could have been tile falling off the lower wing. We know we had a lot of heat in here that damaged all these sensors in here. It could be upper-wing skin. It could be upper-wing tile. It could be lower-wing tile. We see a number of tile that indicate that they fell off because they were melted off from the inside, not that they were damaged or melted off from the outside.

ADM. GEHMAN: Of course, this is the first observed debris.

MR. WHITE: First observed debris. There could have been debris earlier. Of course, we haven't found any tile out in California or any debris of any sort out in California that would tell us exactly what it was. We don't have any confirmed debris until we get all the way into Texas.

Next chart. Another debris event.

Next chart. Third debris event.

Next chart. Fourth debris event.

Next chart. Fifth.

Next chart. Lower-wing surface temperature going offline. You can see now pretty much failed all of my instrumentation here.

Next chart.

DR. WIDNALL: Actually this is kind of directed at Greg but related to what you were talking about.

I looked at your image analysis work on some of the re-entry where you're looking at these debris, and I'm very excited about what I saw in your briefing. I assume you are trying to infer ballistic coefficients of these various debris pieces from some kind of relative deceleration of those debris relative to the Shuttle.

DR. BYRNE: My team takes the first step in that process. We analyzed the motion of the debris as it shed and for all of these events where we've made some good progress in analyzing the motion relative to the Orbiter.

DR. WIDNALL: When you say motion, you mean deceleration relative --

DR. BYRNE: Yes. We then turned our motion measurements over to Paul Hill's team. I think Paul's going to speak later. Then his team then calculates from those a ballistic coefficient.

DR. WIDNALL: When do you think those will be available? Is he going to talk about that today?

DR. BYRNE: I think he will. I haven't seen his charts, but I believe he is. In addition to the motion analysis that we're doing on these debris events, we've also done the timelining. But we're also looking at the luminosity, looking at the intensity of the light given off by the debris and trying to use that to determine what other characteristics we can from that -- mass and area in particular. We're making some progress there, too.

DR. WIDNALL: Great. Well, I look forward to that. That's really interesting.

MR. WHITE: Let's see. We'll just continue to flip through these. This is more temperatures in the wheel well now starting to rise. Again, we believe the heat's been in the wing for some time now, maybe for as much as two minutes, and it's conceivable that we're starting to get higher heating in here because of conduction or flow in through the opening in the front of the wheel well.

Next chart. Another comm dropout.

Next chart. More sensors going offline.

Next chart. This is a point in the aero where we start to see the aero change. This is the reversal in the roll moment that you see from other charts. The roll moment was going negative and for some reason it turns around and it starts to grow and go positive. So, again, some possibly significant structural damage within the wing itself or possibly a large piece of skin being shed to affect the aerodynamics of the vehicle at this point.

ADM. GEHMAN: Or jetting.

MR. WHITE: Possibly, yes, sir.

ADM. GEHMAN: Or just some kind of a change in the geometry.

MR. WHITE: Somehow or another the shape -- either because of internal damage, the external mold line changed, or pieces came off. There's a number of ways that we could have affected the aero.

Next chart. More temperatures on the fuselage going up. Again, this one was an OI one that we knew about from before.

ADM. GEHMAN: Okay. I'm going to ask you to just flip forward. I think what we want to get to is 59:32.

MR. WHITE: Actually I only carried this through about where the wheel well, in our estimation, was breached.

ADM. GEHMAN: Then I do have a question about that, about the MADS data, because the MADS data does two things that the previous data, which was telemetry down to the ground, do not do. One is that it fills in the 25-second gap. Remember when we have loss of signal, then we have these 32 seconds which was retrieved, of which there was five seconds of data, 25 seconds of gap, and then 2 seconds of data. So this recorder was running during those 25 seconds.

MR. WHITE: Yes, it was.

ADM. GEHMAN: Anything significant from those 25 seconds?

MR. WHITE: From the left wing -- and you can even see from where we are here -- almost everything in the left wing had gone offline by this time; and what we see over in the right wing, except for those that failed sympathetically with left wing measurements, those measurements all hung in there and appear to be good. So there's no new, startling data in that gap that says there was anything significantly wrong with the vehicle.

ADM. GEHMAN: And the sensors in the mid-body fuselage were all working.

MR. WHITE: Appeared to be working and except for the ones we know of, temperature measurements that were higher than they should be, there were no indications of anything internal to the vehicle going offline.

ADM. GEHMAN: Right. That's one area of information that the MADS data provided that fills in a nice gap for us. That indicates that the vehicle was intact and the electrical system was working and the right wing, at least, was on.

Then another thing that the MADS data does is it continues about -- I forget what the number is -- 9, 10, or 11 seconds longer than the telemetered OI data. I don't know the exact numbers, but it goes for about another 9 or 10 seconds.

MR. WHITE: That's correct. Another 9 or 10 seconds.

ADM. GEHMAN: Is there anything there?

MR. WHITE: Once again, the MADS data, once we pretty much failed everything in the left wing and the higher temperatures that we've been seeing all throughout entry, again, there's no startling data in that extra 9 seconds either.

ADM. GEHMAN: Okay. Board members?

GEN. DEAL: I've got one. It goes back to your very first slide. You started talking about how some of the instrumentation has been taken out and some of it was broken. Can you give me a little bit more insight into what was broken? Did we look into why it was broken? For example, were any of them strain gauges or anything like that?

MR. WHITE: Yeah, I don't have the list. There are probably a handful, maybe a dozen or so, that were offline for this flight; and I could get you the list. I just don't know off the top of my head which ones. I assume it's a little bit of each -- pressure, strain, and temperature.

GEN. DEAL: Just curious if any analysis had been done about why they broke.

MR. WHITE: I don't know the answer to that. They work these things on a routine kind of basis.

MR. TETRAULT: Somewhere in the 300-second area, you showed one of the first sensors on the OMS pod going low. In fact, there were, as I recall, four sensors on the OMS pods that went low just somewhere in that time frame. For those to go low, you talked about the flow of the air was obviously changing at that particular point. Wouldn't that suggest that there was something on the top of the wing that had to be missing at that particular point? We've talked about issues of foam striking the bottom of the wing; but at that point, for that to go low, wouldn't there have to be something that was missing on the top of the wing?

MR. WHITE: Well, we've done some wind-tunnel testing where we just arbitrarily took sections out of the leading edge of the wing; and actually I believe about the Panel 5 region. If you took Panel 5 out, you can actually get cooler temperatures along the side of the OMS pod.

MR. TETRAULT: But that's a full panel, which wouldn't include the top of the wing.

MR. WHITE: That's a full panel, right. What I'm trying to say, I guess, is that we haven't done any wind-tunnel testing with some sort of a protrusion or a missing hole or anything on the top of the wing to see what that would do to OMS pod temperatures. One of the things we have to do to finish our scenarios is to make sure we can understand the aerothermal in such a way that we can get increased and decreased heating as the timeline progresses. But I don't have any data right now that says, yes, something on the top of the wing would cause me cooler temperatures. I do have some data that says some configurations of leading edge damage could get me cooler temperatures.

ADM. GEHMAN: Correct me if I'm wrong here. Is this not a rather unique aero environment because at a 40-degree angle of attack and a 70-degree roll angle -- talking about the top of the wing and the bottom of the wing leads you to a funny conclusion.

MR. WHITE: It's not like a regular air flight, right.

ADM. GEHMAN: It's more like a blunt surface, and so it really presents a real aero challenge.

MR. WHITE: Yes. It's quite difficult to go figure out exactly how the vortices shift around.

ADM. GEHMAN: Right. But we're going to work on that.

MR. WHITE: We're pursuing it. Yes, sir.

MR. HUBBARD: Any thoughts on the source of the comm dropout, communications dropout?

MR. WHITE: Well, there have been some theories -- and again, these are just theories -- that perhaps as we were shedding material, if it had metallics in it, that that would interfere with the comm.; if you were melting away parts of the insulation on the leading edge spar that perhaps you would get enough metal in the stream behind the vehicle to interfere with the comm. But there isn't any way we can prove that. That's just speculation really.

MR. HUBBARD: As far as you know, the transmitter was working and receiver in TDRSS was working. So something interfered.

MR. WHITE: Yes, sir. Right. The only reason we described it as anomalous is that you look at other flights of 102 for these inclinations and these look-angles to the satellite and we didn't see this number of comm dropouts. So we just flagged them as anomalous.

MR. HUBBARD: Thank you.

ADM. GEHMAN: Well, thank you very much, Mr. White and Mr. Byrne. I know that what you've shown us here today represents the tip of the iceberg for the amount of work that's been done by not only yourselves but a great team of people that reach way, way down into both your organizations. We appreciate very much not only this presentation and your willingness to dialogue with us in a very frank manner, but also the hours and days and days and days of work that you and your team have put in and will continue to put in because we have several mysteries here that we can't explain.

The Board is very grateful for your cooperation and also for the energy and the zeal by which you and all your people have pursued this. We both have the same goal to find out what happened here; and we're going to have to find out what happened by good, hard, roll-up-your-sleeves kind of detective work. You and your folks are doing that. So we're very grateful.

You are excused.

The Board will take about a ten minute break while we set up for the next panel, and we'll be right back.

(Recess taken)

ADM. GEHMAN: All right. We're ready to recommence.

For the next panel, we're going to discuss the object that was observed on Flight Day 2, 3, and part of Flight Day 4; and we're very pleased to have two experts join us here today, Mr. Steve Rickman and Dr. Brian Kent.

Gentlemen, before we start, I'll ask you to affirm that you're going to tell us the truth; and then I'll ask you to introduce yourselves and say a little bit about your background and where you work. Then the Board would be pleased to listen if you have a presentation or an opening statement.

Before we begin, let me first ask you to affirm that the information you will provide the Board today will be accurate and complete, to the best of your current knowledge and belief.

THE WITNESSES: We do.

ADM. GEHMAN: Introduce yourselves, tell us where you work and a little bit about your background, and then we'll have an opening statement.

STEVE RICKMAN and BRIAN KENT testified as follows:

MR. RICKMAN: My name is Steve Rickman I'm Chief of the Thermal Design Branch here at the Johnson Space Center. I got involved in this particular endeavor because if you look at the outside of the vehicle, there's a lot of things on there that are either thermal protection or thermal control-related. So I got involved in this effort; and it's been a very, very interesting challenge. I have a Bachelor of Science degree from the University of Cincinnati in Aerospace Engineering. I have a Master of Science degree in Physical Science from the University of Houston at Clear Lake.

DR. KENT: My name is Dr. Brian Kent. I work for the Air Force Research Laboratory in Dayton, Ohio. I'm a specialist in radar signature measurements. I've been working in this particular area for 26 years, the majority of my adult career. I have a Bachelor's and Master's in Electrical Engineering and a Ph.D. The Bachelor's from Michigan State, Master's and Ph.D. from Ohio State. I direct most of the activities not only within our own facility for signature measurements, but I also chair a multi-service panel that works signature standards for the Army, Air Force, and Navy, that is involved in the National Institute of Standards and Technology. So I've been actively involved in quality control efforts in signature measurements for a number of years.

ADM. GEHMAN: And normally we can find you at the Air Force research lab at Wright Patterson Air Force Base. Is that right?

DR. KENT: That's correct, sir.

ADM. GEHMAN: Please go ahead.

MR. RICKMAN: Okay. If I may have the cover slide for our presentation, please.

First of all, I would like to thank the Board for the opportunity to appear this morning. This has been quite an effort. It's involved a number of agencies, NASA, and various organizations within the United States Air Force, and it's truly been a team effort. What our effort has focused on was trying to get an understanding from a ballistics and a radar cross-section standpoint, of the object that we refer to as the Flight Day 2 object that was observed coming off of the *Columbia* from post-flight observations.

Next slide, please.

ADM. GEHMAN: In accordance with the Board's long-standing tradition of never letting any presenter getting past the first viewgraph, may I make the observation that the object was not observed coming off the *Columbia*.

MR. RICKMAN: Yes. Perhaps I didn't state that correctly. It was a post-flight --

ADM. GEHMAN: What I mean is there's no -- unless you're going to tell me something I don't know here -- we don't have any observation of anything coming off the *Columbia*.

MR. RICKMAN: That is correct.

ADM. GEHMAN: It was observed on-orbit accompanying the *Columbia*. One hour it wasn't there, and the next hour it was there.

MR. RICKMAN: Yes.

ADM. GEHMAN: And we don't know how it came off or what -- we don't have any observation of anything coming off the *Columbia*.

MR. RICKMAN: That is correct, sir. We have some charts, I think, that will clarify that.

ADM. GEHMAN: Thank you very much.

MR. RICKMAN: Here's our plan for today. We first want to give acknowledgement to the organizations that have been involved in this rather large effort, give you a little bit of background on what we know about the object, talk about our approach to better understanding it through the radar cross-section testing and the ballistics analysis. I'm going to give a brief description of all the Shuttle hardware tested. Some of the items I have here today. Then I'm going to turn it over to Dr. Kent, who will give a summary of all of the UHF radar cross-section testings and ballistics analysis, and then we'll wrap it up and along the way we'll be happy to answer any questions you may have.

Next chart, please. I mentioned before that has truly been a collaborative effort. It involves the Department of Defense, the United States Air Force, and NASA. You see all the organizations that are listed up there. We could not have done it without the support of all of these organizations, and it truly has been a joy to work with these groups. Everybody's been very helpful and professional, and anything that we had in our way has magically disappeared and we've been able to do our job. So we're very appreciative of that.

Next chart, please. A little bit of background information. While up on orbit, there were 3180 separate automated radar or optical observations of *Columbia* collected. There were collection sites at Eglin Air Force Base, Beale, Naval Space Surveillance, Cape Cod, Maui, and Kirtland Air Force Base.

It's important to note here that each observation was individually examined after the accident. The debris piece was detected. It was a very laborious effort of post-flight examination. It was the most laborious post-flight examination that the Air Force Space Command has ever conducted for a Shuttle mission. It required just over 285 manhours just in the first week alone after the accident.

The Air Force catalogs these things, and you can see the catalog numbers there. It's been referred to as Object 90626, but I think we'll just refer to it as the Flight Day 2 object from this point on.

Next chart, please. This is an example of some of the data that we've been looking at. Just to give you some orientation here, along the bottom is Greenwich Mean Time. This object separated on Flight Day 2. The best time that they have for a window of separation is somewhere between 15:15 and 16:00 on Flight Day 2. That would have been January 17th. You can see how it tracks away from the Shuttle's orbit, which is shown in red there, and it's expressed in terms of delta time (seconds). So this is seconds of separation. The various symbols that you have on the curve there show the various sites that gathered the data.

Next page, please. What we do know about the object is it has certain ballistic characteristics or a B term. What we're looking for are objects that match this ballistic term or B term and what we have up there is the B term there, drag coefficient C sub D, area-to-mass ratio. CD times A over M. And we're looking for objects that fit the .10 meters square per kilogram, and that's believed to be known within about plus or minus 15 percent.

The estimated physical size of the object was between approximately .4 meters by .3 meters. So it's roughly square. And the object was initially in a semi-stable or slow rotation on January 17th, and Dr. Kent actually has some of the data to share with you to show how over time the object began to spin up. The first day it was rotating about once a minute. The next day, in a Cape Cod pass, it was rotating about once every seven seconds. The day after that, it was rotating about once every 3 seconds; and it actually fell out of orbit approximately 60 hours after it separated from the Orbiter.

Next chart, please. Okay. Well, what else do we know about the Flight Day 2 object? We also have radar cross-section data that was taken in the UHF frequencies at 433 megahertz, and it varies between minus 20 decibels per square meter to minus 1 decibel per square meter and Dr. Kent will give you a better understanding of exactly what that measurement entails. With high importance, we've also bounded what the confidence level is within plus or minus 1.33 decibels.

Next chart, please. The way we approached this -- and I'm going to show you a couple of picture here in a minute -- is we had to take a look at what we would see on the outside of the vehicle, what had the potential to get away from the vehicle. In my organization we tend to break those things into two classes, what we call thermal protection materials, or TPS -- those help protect the vehicle against the high entry heat loading. In that category I also put the leading edge subsystem or reinforced carbon-carbon components that there's been a lot of discussion of. And then we also have Thermal Control System, or TCS components, which would be representative of what you would find in the cargo bay. Those components are there more to protect the vehicle from the extreme temperature swings that you would get while going around in orbit, hundreds of degrees above zero to hundreds of degrees below zero in a very, very short time.

So we basically applied two gates that any object or any candidate object had to get through. It had to match not only the RCS -- the radar cross-section information -- it also had to measure the ballistic coefficient. But also we're very mindful of the fact that there's been a lot of debris collected, a lot of forensic evidence down at the Cape. So obviously if something shows up on the floor down at KSC, it's something that we can exclude; or if it was something that we carried with some interest previously, once it is found, then we can exclude that, as well. So candidates failing to match even one of those criteria are excluded as possibilities for the Flight Day 2 object.

Next chart, please. I mentioned before this is an overview of the Thermal Protection System constituent materials. We try to be very methodical in our approach to performing this investigation. We have various materials on the outside of the vehicle. The light blue -- and it doesn't really show up very well here -- represents the LI 900 or the 9-pound-per-cubic-foot density tiles. We also have 12-pound-per-cubic-foot density tiles and 22-pound-per-cubic-foot density tiles. Those comprise the lion's share of the acreage of the bottom of the vehicle.

On the side of the vehicle, we have a blanket insulation that we refer to as AFRSI, Advanced Flexible Reusable Surface Insulation -- we also call it fibrous insulation blanket. That's good to a lower temperature than the tiles. This is in a more benign area of the vehicle. We also have FRSI, which stands for Flexible Reusable Surface Insulation or Felt Reusable Surface Insulation. It's a needled Nomex felt. We also have AETB-8 tiles. I believe those are vacuum-based heat shield.

The tile materials are all going to look very similar to one another. As a matter of fact, I have a sample tile right here. This is the 22-pound density tile. They vary in size and shape as you go around the vehicle, but by and large on the bottom acreage they're approximately 6 inches by 6 inches. So this would be representative of the shape; and, of course, the thickness varies as a function of location. As you can see here, by just testing a handful of materials, you can cover the lion's share of the outside of the vehicle.

Can I have the next slide, please? I already showed you a picture of the tiles. We tested them in a number of different varieties. For example, the LI 900 tiles, we weren't sure what would happen to the radar cross-section if we also included the RTV adhesive on the back and the strain isolator pad, which is Nomex felt. We also didn't know what densification of the tile would do. Densification is a process that we do that increases the density about .15 inches at the bottom of the tile and helps it adhere to the vehicle. So we tested in a densified and undensified state. LI 2200 tile looks the same. Here's AFRSI and FRSI.

May I have the next slide, please? There was also interest early on on testing carrier panels or segments thereof. I have with me here the actual mockup of a carrier panel that we tested up at Wright Patterson Air Force Base here. It consists of 22-pound density tiles, a metal support plate on the back, and also an insulation called horse collar, which is Nextel with a sheet of Inconel in it. So this was tested early on.

At the time we found great interest in that sample. We ultimately asked for and received some flight assets, in particular some actually flown four-tile and three-tile variant carrier panels that have more hardware on them; and we got those up to Wright Pat for testing, as well. We've also tested the horse collar all by itself.

Next chart, please. Given the intense interest in the carbon system, we had some flight assets sent up to Wright Pat. We had a flight RCC panel tested. We have some Incoflex ear muff spanner beam insulation. As a matter of fact, I have that right here. This is Inconel over a serochrome batting, and this would be located behind the wing leading edge panel. So it's normally inside of the wing.

And then our latest area of focus has been on the actual T-seals. This is a T-seal that's undergone testing up at Wright Pat, as well.

Next chart, please. Once we had some preliminary measurements on the reinforced carbon-carbon pieces, we needed to do a little bit of refinement; and one of the best ways to do that was to retrieve some pieces from the debris from *Columbia* down at KSC. What we were looking for are different classes of objects, different classes of carbon objects, like what I refer to as carbon acreage. It's essentially a piece out of an RCC panel. So we tested a few samples with that, with and without lips. We also tested segments of RCC T-seals to get a better idea of what fragment of a T-seal might give you the appropriate radar cross-section.

Next chart, please. That's the outside of the vehicle. Now, if you look inside in the Shuttle cargo bay, there are a number of Thermal Control System materials there. When you look out over the cargo bay and you see a lot of white, what you're really looking at is a material called beta cloth. Beta cloth is a glass fiber material. A lot of times it has a Teflon sizing over it. But if you look at something that creates the cylindrical surface of the cargo bay, what you're actually looking at is multilayer insulation.

Multilayer insulation is a very good thermal control insulator. You can have temperature gradients of a couple of hundred degrees across a sample of about this thickness. If you were to cut into this, what you would see are alternating layers of an aluminized plastic like Kapton or Mylar and Dacron spacer mesh. So there is metalized layers in here. You'll also note that this has metal quilting in here in the form of a stainless steel wire to help it from electrical grounding.

If I can go to the next slide, please. We tested a variety of multilayer insulation blankets: some from payloads, some from the cargo bay itself. We even tested logos off of payloads. I should mention that it's my understanding that they did a survey post flight from the video coming down to see if they noticed any difference in the cargo bay. I believe about 60 percent of the cargo bay is observable from the cameras, and no differences were found. So if there was an object that was conspicuously missing from the cargo bay, it would have likely been detected from that survey.

Next chart, please. In addition to the multilayer insulations, there's various types of bulk insulations that we have in the cargo bay. If you were to look inside here, you would see a glass batting that's inside here. This is beta cloth with the familiar quilting material on it, this is Kapton on the back, and this protects the vehicle, in regions it needs to, from higher heat loads.

There's actually three different varieties of this bulk type insulation. The one I found pretty interesting to look at was this one. This is actually the type of insulation that's beneath the cargo bay radiators. I should point out and did not point out but at mission the last time of about 3 hours and 8 minutes, the port side radiators were deployed. So if there was an object under there that could have possibly escaped, that might have given it an opportunity to do. Those radiators stayed deployed through mission elapsed time about 3 days, 7 hours, and 50 minutes; and then they were redeployed again, I think, on the 11th day of the mission. So this is the type of blanket that you would see beneath the radiators.

We also had a question from a Board member a week or so back, asking us is it possible that any tool might have been left beneath the radiator. We did a little bit of checking into that. The only thing we were able to find as a possibility would be a crimping tool that would be used for blanket snaps. We had some ballistic analysis done on that, and we'll be talking about that today.

May I have the next chart, please? I'm going to turn it over to Dr. Kent now. One final thing I did want to mention, though, just so people are aware of it, is there was an attitude maneuver that corresponds with the time just prior to when we think the object was released. What was happening at the time is the Shuttle was flying in a cargo-bay-to-earth tail-on velocity vector attitude. That happened at mission elapsed time -- well, the GMT on it would be January 17th. I believe it was 14:42 GMT. The vehicle yawed 48 degrees, biasing the right wing into the velocity vector, and then I think it was at 15:17 GMT they went back to the tail-on velocity vector attitude. The nearest maneuver to that, prior to that, was about mission elapsed time eight hours. After that, the next maneuver wasn't until about mission elapsed time 48 hours.

MR. WALLACE: Mr. Rickman, could you characterize that maneuver you just described? Now, I understand it to be an extremely benign maneuver. Would that be accurate?

MR. WHITE: Yes. I'm glad that you brought that up. This particular mission had approximately 500 attitude maneuvers in it, and we've flown missions before where we've had many maneuvers. So this is very run-of-the-mill. This is very, very benign, yes. And I believe this particular maneuver was done for an IMU alignment to support a given payload, an initial measurement unit.

MR. WALLACE: In terms of it imposing any stresses?

MR. RICKMAN: Actually this particular maneuver was done with the vernier jets. Those are about 25-pound thrusters as opposed to the primary RCS, which I believe is somewhere in the neighborhood of 800-pound thrust. So, yes, it was done with very gentle jetting.

ADM. GEHMAN: Thank you very much.

Dr. Kent.

DR. KENT: Okay. What I'd like to do now is to proceed directly into the summary of radar signature and ballistics analysis. I'd like to acknowledge my coworker, Dan Turner, who worked many hours with this, as well as my collaborator out at Space Command, Mr. Robert Morris.

The key point I want to make here on this chart is we've invested about a thousand hours in this activity since the 3rd of March, but I also want to point out, too, that we did testing not only at UHF band, which is the subject of what we're talking about today, but we also did a significant amount of RCS testing at FAA radar bands -- that's the L and the S band -- as well as the ascent-tracking radar that's used when the Shuttle goes up -- it's C band. That information has been turned over separately to the flight directors; and I believe Mr. Hill will be commenting later on how that particular data is going to be used as part of the debris characterization recovery efforts. This particular discussion will solely discuss the UHF testing in relation to the Flight Day 2 object.

Next slide, please. What I want to start off with is to very quickly review the actual data that we have in hand. As we've talked about, it was observed by multiple sensors. I'm going to concentrate on the two sensors that were used that are characterized in radar signature terms. Those were what we call the Pave Paws radar, located at Cape Cod and at Beale Air Force Base. I then will give you a brief description of our test facility and how we use it to actually simulate the same radar signature conditions that were observed for the on-orbit measurements and how we're comparing the two. Then I'm going to basically walk through these candidates that we've examined and show you how very quickly you can, either from a ballistic standpoint or an RCS standpoint, move a large number of the classes of objects off the table and focus our activities only on a few of interest. Then I'll give you a quick summary at the very end.

Next slide, please. This basically I'm showing are the four most reliable on-orbit observation measurements of radar signature. The one in the white, which I did differently, is the one observed at Beale on the 17th of January. What I've indicated there is something that we did throughout the effort but I've added to this particular piece of information. We've added on top of the data, which is in black, a red and a green line that indicates our level of fidelity or understanding or, let's say, level of accuracy of the data that we believe has been taken. This is very important because if you have a certain data range that's like this and you're trying to match another object to it, it's very important that the fidelity range of your actual measurement falls within the actual on-orbit observed, or else it becomes excluded. So we thought it was very important very early on to get the information necessary to assess the accuracy of this data so that we really knew what we were starting with.

So you notice the first yellow chart in the upper-right corner here is the first-day data. You notice this very slow, over 60-second period here of a revolution of a tumble of approximately a period of about once per minute. By the time of the second day, you can see that the tumble period has increased; and by the third day it's gone up quite a bit, shortly before it de-orbited.

Next slide, please. What we glean from this particular information was on the Flight day 2, 3, and 4 tracks, is that the observed RCS varied from, for instance, Flight Day 2, approximately minus 18 to minus 4 decibels per square meter. The Beale data tracked around minus 17 to zero; and that's not too unusual because, remember, they're observing this particular target at different spots in the United States. So that particular object, if it were floating around, would present a different angle to those two radars. The Day 3 and Day 4 tracks varied between minus 15 and minus 2, minus 13 and minus 1.75; and you can see the fidelity.

I should also point out that these particular radars, since they're designed to penetrate through radar, operate in what we call circular polarization. That means that the actual electric field that's radiated from these radars rotates, and this allows superior coverage through bad weather. It's used by Doppler radars, for instance. In this particular case the data was transmitted left circular and received right circular; and as you'll see, the way that we actually take measurements are in linear polarization and then we mathematically combine them to simulate the same numbers.

Next slide, please. This is the advanced compact range at Wright Patterson Air Force Base. That's where my day job is. Basically, it's a major facility. It's an anechoic chamber. It's designed to take radar signature measurements from very low frequencies, around the television band, all the way to very high military frequencies. The actual signatures that we're talking about in this particular comparison at UHF are 433 megahertz, is kind of on the low to mid range of what our capabilities are. The facility is capable of testing actually a very large object, so that objects on the size of what's on the table here are well within our capabilities; and because the levels that we're talking about are fairly high in signature, it didn't present any significant technical challenges in terms of doing the measurements.

Next slide, please. This is, for instance, a setup showing, for instance, that one blanket that Steve just showed you here. That's mounted on a very low cross-section foam. In other words, this foam piece here that actually holds the target has a very low radar scatter, does not contribute to the experiment, and we can also subtract out its residual.

Now, this big reflector that you see in the background, essentially what this is like, you can think of it like the equivalent of a telescope. By putting a radar very close to a reflector at its focus, basically what that does is allow us to simulate a very large separation between the radar and the target, like what was really observed on orbit, in a very small or compact space. That's where the name "compact range" comes in.

Next slide, please. I wanted to start off just to kind of ground you in terms of the data. This is one of the test cases that we run before we do any kind of experiment. It's one of many. This is strictly a 12-inch-by-12-inch metallic conducting aluminum plate. The reason we wanted to present this to you is you'll notice for a square plate this oscillatory behavior here. What we're looking at is we're talking about aspect angle or orientation angle. So in other words, if this is my plate, when we talk about aspect angle, that's the orientation of the plate relative to the radar. So if my radar is out here and I talk about zero degrees, that means I'm looking normal or perpendicular to this plate. As I move it out to, say, 180 or zero or whatever, I'm going off-normal here. So the peak scattering for a flat plate tends to be when you're normal to the plate and the lowest level tends to be off-normal and that depends on the frequency of the radar that's actually illuminating the object.

I should also point out that radar cross-section, the physical property that we're measuring, is not a function of weather. It's not a function of atmosphere or any of those kinds of things. It's a physical property that relates to how much radar energy is scattered from an object, based on what's actually illuminated.

The second thing I want to point out to you is what we normally do is that we normally take these two linear polarizations -- the vertical, which is the VV, and the horizontal, HH, are always referenced to the ground -- and then we construct what we call the circular polarized data, which is the on-orbit data, which rotates continuously. So I wanted you to see that because you'll see the patterns of these kinds of shapes are going to be very similar to this standard that we use so that we know everything is working.

Next slide, please. So I'm going to give you a kind of a close-up of one of these and then I'm going to show you them in large groups because very quickly we're able to eliminate a large number of these classes.

This would be typical, for instance. This is the AFRSI fibrous. It's approximately a 12-inch-by-12-inch piece, and what you have down here is this particular scale is a radar cross-section in decibels per square meter. Now, this looks like a linear scale, but actually think of it in a logarithmic sense, in the sense that something that's minus 40 is four orders of magnitude lower in radar cross-section than something at zero.

So what I've drawn on this right here is this box. This is the maximum and minimum range of the on-orbit observed values. Now, the minimum range is not nearly so much as important. In other words, the observed eye can actually, in terms of a measurement that we make, can be less than that because we have a lot more signal that we can do than they do on orbit. But what's important is this maximum value. You need to be at or in excess of that maximum value somewhere in the aspect presentation of this target for it to be a viable candidate. So looking at this particular device here, this AFRSI, one of the first things you notice is that it's nowhere close to the box. As a matter of fact, it's orders of magnitudes off. The RCS for this thing isn't anywhere close to where it would need to be to be the Flight Day 2 object; and therefore, by default, it's immediately eliminated.

Next slide, please. So let's look at large groups of them because I broke them off into several classes. First, items that we rejected because the RCS is clearly too low. These include the FRSI, the tiles of all varieties -- and that's no surprise. Because what's tile mostly? It's mostly air. They're very lightweight, and it's basically a block of air with a little bit of structure on it. As a result, it inherently has very low cross-section.

We tested both the 9- and the 22-pound variety of these things; the signatures are way too low. So we were able to eliminate the tiles very quickly. The beta cloth that we were talking about on the back of the insulation were also tested. For the most part, those are also much too low. These Freestar, the logos that are typically put on, are nonconducting. There's no metal in them, and it's metal that contributes a lot to the radar signature. So again, those were also way too low.

Next slide. Continuing that, we started off in measuring what Steve referred to as the carrier panel mockup. We did some initial measurements, but we also found out that there were some differences between the mockup that was provided to us and the real carrier panels. So we ended up measuring both, just to be thorough.

And what we find, again here this box is the range of the on-orbit values. The blue is this equivalent circular polarization, and what you notice for the most part that it doesn't get anywhere close to the peak value observed in any of the configurations that we looked. I should also point out that for the more complicated parts, because of their shapes, we generally oriented them in two or three different axes, usually trying to highlight the presentation that we would know would produce the highest radar cross-section so that we would get an idea, since we really don't know the angle between this object that might be tumbling in space and the radar, what its exact RCS is, what we do know is that it took swings in a maximum to minimum. If we couldn't even come close to producing a maximum swing, then likely that object was also eliminated.

Next slide, please. Finally the fibrous thermal blankets, the carrier panel by itself, the collar seal by itself, and the 22-pound tiles, again, were just not anywhere close to where they needed to be from a radar cross-section standpoint. So those particular items are immediately taken off the table.

Next slide, please. The next set of RCS results I'm going to do -- and I'm going to be intermixing a few ballistic results as well with these things -- are on this class of what I call lightweight thermal blankets as, again, Steve is going to be showing you here in a minute. In this particular case what you'll see when you look at these things is you say, "Oh, look, the RCS is very close to the box. It must be a good match." Well, two things I want to let you know. That shouldn't be too much of a surprise because most of these thermal blankets have metalized layers in them. They should look very much like the metal plates that I showed you earlier that we used as a test case.

The other thing that I'd like to point out, Steve, if I could borrow this, is one might say, "Well, but that's a real flat surface and these are kind of crinkly." You need to keep in mind that the radar wavelength that we're looking at on this thing is on the order of 2 feet and, because of that, local, small, minor variations in the actual shape are not going to seriously hurt its radar signature. That will also become important later as we start talking about RCC fragments. So as I look at almost all the classes of thermal blankets, which are all variations on a theme, some type of metalized layer, some type of metalized Kapton, they all look like they could fit very well within the RCS rate; but as I'll show you in a minute, the area-to-mass or ballistics coefficient is not right. I'll show you that data in just a second.

Next slide.

DR. WIDNALL: Wait. I have a question. I would certainly agree with you that the area and the mass are probably not right, but there is also the issue of drag coefficient and I want to know what kind of drag coefficients would you assume. I'm not trying to make these candidates, but you need a drag coefficient. What do you use?

DR. KENT: Right. I think Robert Morris and the space community are using a drag coefficient that I believe -- again, you're asking a little bit outside my area, ma'am, but I believe the number was .2 --

MR. RICKMAN: It was 2.2.

DR. WIDNALL: 2.2?

MR. RICKMAN: 2.2 for a drag coefficient, which is a rectangle on the broad side and then for the tumble, they time-average the area that's presented.

DR. WIDNALL: Okay.

DR. KENT: I'll have that figure for you in just a minute.

DR. WIDNALL: More fineness on that.

DR. KENT: These insulation space blankets are also thermal materials. There's two others that I've included in this particular category where the area and mass is wrong but in this case the item was much too heavy and too large and that's, of course, a full, intact RCC edge which we tested initially just to kind of baseline what kind of signature level we would get at UHF frequencies if an entire edge was intact for whatever reason. Clearly, it's at or much above the observed values. And I should point out this particular RCC, reinforced carbon-carbon edge, has the T-seal installed in the end and that will be important because you'll see a lot of the pattern characteristics from a side aspect are the same because it's the T-seal that's doing a lot of the scattering.

Next slide, please.

MR. TETRAULT: Excuse me. One of the RCC panels that you tested, did it have a spanner beam attached to it when you tested it, the original one?

DR. KENT: The answer to that question is, yes, it did. I believe the picture showed that.

MR. TETRAULT: Will you make sure that we understand which ones have metal attached to them and which ones don't on your testing, please?

DR. KENT: Okay. With the exception of the RCC panel, none of the other items that we had had any kind of metal attachments, no bolts or anything else; but as long as you're on that topic, sir, I will point out that if we're talking about bolts that are like 2 or 3 inches long, at these radar wave lengths -- again, the radar wave length's about like this, and a bolt's like this -- it's going to have quite a low scattering value and it's going to be very non-directive in one of the two radar polarizations. So it's going to be quite a bit lower than the observed values that we're talking about here.

I borrowed this chart from my compatriot at Space Command, Mr. Morris, showing you the series of these lightweight blankets. What I'm showing you is the B term or the ballistic coefficient. I've labeled the various items down here. The important thing is that the Flight Day 2 value here is the solid red line and the dotted lines are its approximate level of uncertainty. So it's not a matter like, well, these things are a little off. They're a lot off. They're quite a bit removed from the possibility. So it was fairly easy, again from a ballistics standpoint, to eliminate these particular items, mostly because they're too light. Now, again if somebody says, "Well, what about a piece twice that size?" Well, keep in mind its area to mass. So making the same material a larger piece is not going to change this value any. So again, that was one of the reasons why these were not very strong candidates.

Next slide, please. Now, I'm going to show you a series of charts where the RCS and ballistics begin to converge. The first item, I'm showing you an example -- I believe this was actually released in the press conference last week -- was the wing spar insulation piece that Steve is holding up here. It was a good match both in signature and insulation. Most of these, I'm only showing you one view. There were actually many views in terms of radar looks at these particular targets. A whole T-seal was tested and shown to be well within the bounds, both from a side aspect and a top aspect. Most recently one of the things that had dawned on us when we actually tested the T-seal -- and I'm going to use this. This, by the way, is the attachment flange for a T-seal. One of the things that dawned on us, because these are fairly strong scatters, that the thing that fits inside of here -- which, of course, is the RCC edge -- would also be a strong scatter. So we made a recommendation a week and a half ago for us to look at what we call acreage candidates or basically pieces of RCC that would be on the order to find out how big a piece that we would have to have to have it to be on the order of the RCS for the Flight Day 2 object.

Now, you just don't go breaking away a piece of a perfectly good, expensive RCC. So the methodology we decided to use was to go down to the actual floor, look on the symmetrical right-side area and look for fragments of RCC that were on the order of the size that we felt as though would be appropriate for signature. So keep in mind that even though we are measuring debris components, obviously they're not the Flight Day 2 object because these were recovered parts from the right side. But they were used to bound the RCS or radar signature of RCC panel acreage.

So these last two items down here, which is what we call Fragment 2018 and 37736 -- which are just designators that they use for the recovered pieces -- both measured very close to the on-orbit range. And these things, even though they don't see much in this particular picture, are quite irregular. The parts can be roughly squarish, but they can have some curvature or they can have a lip on them. The point of the matter is that carbon-carbon is fairly conducting and so it behaves quite a bit, again, like metal.

Next slide, please. Now, I do want to talk in particular about this item. There seems to be a great deal of interest in the T-seal; and we, of course, tested a whole T-seal as part of our initial test package. What we really wanted to do was to test a half T-seal; but again, you don't take a piece of flight hardware and destructively cut it apart.

So what we tried to do is we looked again on the right side of the vehicle and recovered the largest intact fragment that had been recovered from the right side in the vicinity of the area of interest on the left side which was -- again, I think this was a top piece in Panel 10. It's a piece of T-seal that's approximately 33, 34 inches long. But I will point out to you that it did not have its attachment flange, which is this part right here on this particular scrap that we had, nor did it have very much of the apex -- as you see, a kind of C-shaped devices. So what I tried to show on this chart here is this actual green area is the approximate acreage of that part that was recovered and we believe through analysis that you're going to have to recover a T-seal that's going to have to have part of the apex or part of this flange area in order to bring the RCS closer to a match.

If you just take a look at this particular T-seal, what you'll find is that the circular polarization value looks a little bit low. In another orientation, it turns out that one of the polarizations is well within the limits and one is under. This is again the classic issue of the fact that when you're creating circular polarizations from two linear datas, both polarizations have to be high; and in order for this part to be more reactive to the circular polarization, it has to have some curvature. So we feel very confident that this particular item, even though this scrap is a little bit low, that we cannot eliminate as a class a T-seal half that includes the attachment flange or part of the apex in terms of radar signature.

Next slide, please.

GEN. BARRY: And that can mean the top part and the bottom parts?

DR. KENT: Yes. It could be the top section, or it could be the bottom section.

ADM. GEHMAN: Could you go back one, Doctor? The chart there on the left-hand side, the on-orbit radar cross-section. That looks to a layman like that's a pretty good match.

DR. KENT: Well, you see, remember, the on-orbit minimum to maximum falls in here; and the point is that we know we observed values that are close to the top of the box. So what we're looking for are what I'll call these blue lines that are very close to the top of the box at some point in its aspect orientation. As a matter of fact, if you look at the carrier panel, for instance, you'll find that it is consistently about minus 5 at its most advantageous orientation; and the problem with that is we know that the carrier panel's it. We've measured the whole thing. There's no more to add, so it can't get any larger. In the case of this fragmented T-seal, we know that there are pieces of it that we would have liked to have had but we didn't have.

ADM. GEHMAN: So the fact that your results for any azimuth fall completely inside the box is interesting but you need more reflectivity.

DR. KENT: Yes. It's most important that it crests the top of the box, touches or exceeds the top of the box. You don't want it to exceed the top of the box but just a tiny aspect angle because then you get into the whole question of whether you'll ever present that favorable orientation. But it turns out that T-seals have a particularly nice property because in this plane where it has the T, it has a very, very broad radar pattern in this plane, which means orientation is -- it's very insensitive to orientation if that part of the T is intact.

Next slide, please. Now, here are the ballistic coefficients for what I'll call more interesting components. This is the RCC and carrier panel components. Now, this is a different scale than the one I had before. The other one went up to 1.2; and the maximum on this one only goes up to .3. So we're really blowing this up. Here again is the observed Flight Day 2 value. You'll notice the uncertainty bars look larger, but that's only because of the change in scale.

I'm showing you a couple of things. First of all, what I wanted to show here is initially when we were looking at carrier panels -- before those were no longer an RCS candidate -- an intact carrier panel didn't make it anyway and you had to actually explain away one of the tiles or add in the collar in order for it to behave appropriately. The ear muff seal, I think it's called the spanner insulation piece that Steve showed earlier, fits well within the ballistics. The interesting thing is we had an analysis run for this particular briefing on one of these pieces, which is about 100 square inches, and it fits right where it needs to be. Now, since I produced this chart, I got an e-mail from Mr. Morris yesterday. He ran the ballistics on all four of the scraps that we did; and all four of the scraps met the beta term criteria, well within the experimental limits.

ADM. GEHMAN: All four of the scraps of what?

DR. KENT: Of RCC. If we could go back a slide, please.

ADM. GEHMAN: RCC pieces.

MR. TETRAULT: Did all of those RCC pieces include a web?

DR. KENT: They didn't include a web but they were --

MR. TETRAULT: A web. An angle. So that it had a rib.

DR. KENT: No, actually this one did not.

MR. TETRAULT: You had one with plain acreage, and it passed the test.

DR. KENT: Right. It's not quite flat, it had a little bit of a ripple in it. We had one that was attached as an edge. I believe that's the one here, No. 37736. It's got an edge. There were two others, as well, we reported to the Board. Basically it turns out -- again, remember, the radar wavelength is this big and these lips are only a small fraction of this wavelength. It helps to have it, but it's not a crisis to have it. The important matter is the acreage or the size of the piece.

Go forward two charts, please. So basically in this particular chart what I did, of course, is that these had failed the RCS, and so far that the T-seal -- which I would like to point out initially one looks at this thing in either its tumble or its spin axis and it's not hitting the mark but, of course, you could have any state between those two and because they bound the observed value -- both the T-seal or half T-seal still fall within the ballistics criteria.

Next chart, please. So keeping in mind that the Flight Day 2 object must meet the observed physical properties of these components; I can't stress enough that these are primarily exclusionary tests. We started with 31 materials. If the items do not meet one of these two criteria, they cannot be the Flight Day 2 object. At the end of the day, as you'll see, the items that meet both the RCS and the ballistic criteria is this spanner beam insulation, sometimes called the ear muff -- of course, it's excluded if it's not exposed (and I think that's been discussed in the past) -- a whole T-seal; a T-seal fragment that includes an attachment flange that's this part, this end of it or the apex, kind of the middle of the C; or an RCC panel acreage. 90 square inches is the minimum if you're worried about it just having enough radar signature; but if you want to have a little bit of leeway to account for the fact that you don't have all the control of the orientation, probably on the order of 130 to 140 square inch piece of RCC acreage would also agree with this object. It needs to be roughly square, within about 20 percent. Otherwise one of the dimensions has to get a little bit bigger. Again, that does not hurt the area-to-mass or the ballistics. And the curvature, again, is okay because, remember, the wavelength is large compared to local curvature of these pieces.

I will point out that we have been asked by the CAIB to screen an upper carrier panel, and because that's coming out of flight spares and it's taking some time to arrange, that item has not been done yet.

Next slide. Steve.

MR. RICKMAN: Okay. Let me just do a quick wrap-up here. What we tried to do is roll up everything into a one-page summary chart that you can take a look at. What I would offer up is looking at the right-most column, and what we did is we came to our conclusions on these. The green represents items that we feel are excluded -- again, noting that the ear muff is excluded if it's not exposed; otherwise it does meet the criteria.

From all of the testing and analysis that we've done, we feel that RCC T-seals as a class cannot be excluded and RCC -- what we call acreage or pieces of the panel -- cannot be excluded. But there's another point to be made there that the panel acreage itself would have to be on the order of 0.33 inches thick for it to have the correct ballistics. Just so you know the area for a constant thickness piece item, the area-to-mass ratio will scale up. So if it meets the area criteria that Dr. Kent discussed and it meets the thickness criteria, then, again, as a class, you cannot exclude it. It turns out that on the lower panel acreage in the Panel 8 to 9 region, you do have RCC panel acreage that is of this thickness; and it varies elsewhere. That's pretty much all we need to say on that particular chart.

ADM. GEHMAN: Thank you very much.

Board members, any questions for these real smart gentlemen?

GEN. BARRY: You're going to have the final panel testing completed when?

MR. RICKMAN: Are you referring to the upper panel, sir? We need to get the paperwork going to get that out of the flight inventory, and we'll be starting to work that ASAP.

MR. TETRAULT: Let me go back to the 8 or 9 area and whether or not it has that 0.33 requirement. Is the 0.33 in that area only on the spar rib, or is it on the acreage itself?

MR. RICKMAN: Sir, it's on the acreage. I did verify that yesterday.

ADM. GEHMAN: You said that in the case of a candidate that was just flat acreage, RCC acreage, you need something that's between 90 square inches and 120 square inches, which is roughly the size of a piece of paper or a little bit larger.

DR. KENT: Right. It could be larger than that, of course, for the orientation; but if it gets much smaller than that, then that peak signature doesn't come anywhere close to the top of that box that I drew around all those charts.

ADM. GEHMAN: Very good. Board members, anything else? All right.

Gentlemen, you've kind of briefed us there on how much work was involved in this; and we really appreciate it. This object orbiting with the *Columbia* is a great mystery and we don't know if it's related or not, but we had to move heaven and earth to describe what either it is or it is not because it fits into this pattern of circumstantial evidence. It's very difficult to prove the negative, but your help has been instrumental in us characterizing what we have here. We think we have made great strides in clarifying what we've got up there even though, as you have said at least five times, we can't prove anything. So on behalf of the Board, for both yourselves and also the teams that you represent, please accept our thanks. You are excused and --

DR. WIDNALL: I do have a question. Sorry.

ADM. GEHMAN: Hold it.

DR. WIDNALL: My favorite question. Why do things tumble?

ADM. GEHMAN: In space.

DR. WIDNALL: Why does the frequency of tumble increase for this object? Is that correlated with coming down into slightly denser regions of the atmosphere? What's going on?

MR. RICKMAN: I think it could be a number of reasons. I think if you have an irregularly shaped object and you have the center of aerodynamic pressure at a location different than the center of mass, then as you get lower and lower, you're going to have increasing aerodynamic forces on there that would tend to get the object to spin up.

DR. KENT: And if you take a look at, for instance, even the samples, the pieces of acreage that we've tested, they're highly irregular pieces. You, know, one side will have a lip; one side won't. So we have no idea if it were something like that. The chance of a nice, symmetric, clean, square shape coming out are quite low; and it's probably going to have some kind of differential pressure on it.

ADM. GEHMAN: Thank you very much. We're going to stay here. You all are excused.

We'd like Mr. Whittle and Mr. Hill to please come out and take their seats; and we'll get moving on this right away.

(Next witnesses seated)

ADM. GEHMAN: All right. Thank you, gentlemen.

Our third panel is ready. They consist of Mr. Dave Whittle. Mr. Whittle was and has been the Director of the Mishap Investigation Team since day one. He's been in charge of picking up the debris and the recovery efforts, all recovery efforts and all coordination efforts with all the agencies that were helping with this investigation. Mr. Paul Hill is a flight director and has been responsible for the sighting studies and videography. So we're now going learn what we can learn about debris, where it's found, and what we can determine from debris analysis.

So, gentlemen, before we get started, I would like for you to affirm that you're going to tell us the truth. I'll read a statement to you and ask you to affirm that you agree to this. Let me ask you to affirm that the information you will provide the Board today will be accurate and complete, to the best of your current knowledge and belief.

THE WITNESSES: Yes, sir. I will.

ADM. GEHMAN: Would you please introduce yourself and say a little bit about your background and what your day job is, and then we'll listen to your presentations.

DAVID WHITTLE and PAUL HILL testified as follows:

MR. WHITTLE: I'm David Whittle. I work for NASA in the Shuttle Program Office. I have an electrical engineering degree from the University of Texas at Arlington and an MBA degree from the University of Houston at Clear Lake. I have accident investigation training from the NTSB school, from the NASA school, and a Certificate of Air Safety from the University of Southern California School of Aviation Safety.

MR. HILL: My name is Paul Hill. I'm ordinarily a Space Shuttle and a Space Station flight director. For the last few months, I've been leading a team looking at primarily early sightings and videos.

ADM. GEHMAN: Good. All right. We're running considerably late, but we would like to ask you if you would like to make a presentation or an opening statement. If it's all right with you, we'll kind ask our questions as they go along. Whichever one of you is ready, go ahead.

MR. WHITTLE: I'm ready. On February the 1st, I stepped off the airplane at Barksdale Air Force Base to start the first part of this search, what has turned out to be the largest search of this nature in the United States, in the history of the U.S., perhaps the world. In the process of this, we've involved over 30,000 people from virtually every state in the United States. We've involved over 130 federal, state, and local agencies in various roles, from major to not so major. It started off with thousands of volunteers from the people of East Texas. My e-mail every day for the first few weeks was full of people writing me, wanting to help, wanting to assist. We got a lot of phone calls. So we had a lot of people from all over, wanting to help.

Early on, what we were trying to understand was the distribution and the magnitude of where the debris was. As you well know, when you visited me at Barksdale, we were literally putting pins in maps to help us understand how the debris was distributed and where we should be applying our efforts. As time went on, we got a lot more scientific than that.

We had reports from a great majority of the states in the union. We also had one report from Jamaica and one from Bermuda, of people reporting what they thought was Shuttle debris. In many of those cases, they were not debris; but people were seeing all of the publicity and wanting to do their part.

As the magnitude and the position of debris became more and more evident, we developed a methodology and a technique that we felt would allow us to return the great majority of debris. The major players in the retrieval and in doing that was NASA, both the U.S. and the Texas Forest Service, FEMA, and EPA. They did the lion's share of the debris retrieval.

We closed our Texas search on April the 30th. At the end of that time, we had physically on the ground covered, with people walking, over 700,000 acres. We have searched over 1.6 million acres with our air assets, which primarily were helicopters. We've mapped 23 miles of the bottom of Lake Toledo Bend and Lake Nacogdoches. The U.S. Navy Supervisor of Salvage was a major player in our underwater operations, and they dove on over 3,100 targets in Toledo Bend and over 326 targets in Lake Nacogdoches. The days that I was out there, the water temperature was 47 degrees. The visibility underwater was about inches. As of April the 30th, we have about 39 percent, a little over 84,000 pounds, over 82,000 pieces, and that's continuing to change today in that we're still getting calls in.

As much as I would like to find something west of the state of Texas, right now our westernmost piece, as you know, has been the single tile that was reported by a farmer in Littlefield, Texas. That does not mean that we don't think there is something out west. In fact, we have been working and still continue to work in that area.

Analysis from radar, from video, from trajectory resulted in nine what I tend to call NTSB boxes, but nine boxes that were identified where there was a high potential of having something in that area. Sometimes these boxes were large, and sometimes they were small. Four of those boxes were in Texas. With the end of the search on April the 30th, we have completed those boxes. As a matter of fact, the last box and the box that I really personally felt the most confidence in was in Granbury, Texas.

Before they left, the Forest Service sent 800 people out there to search that box. We sent 800 people out there for about two days, searching what I thought was a very high-probability box. And it wasn't just me. A lot of people did.

We did find one tile, but we really felt like there was perhaps some metal in there. There may still be, but we searched it very good. So that completed our Texas searches. The other boxes have been searched in other ways at an earlier time.

That did leave five boxes that were to the west, and those boxes are in New Mexico, Utah, and Nevada. We have finished searching the New Mexico boxes a few days ago; and, in fact, they found about four or five items. It's to be determined whether or not they're Shuttle. They've been sent to Kennedy for analysis. There is an Air Force base around there, and there's a very high possibility that aircraft type material could be in that area. So we need to sort out is it Shuttle or is it not.

We are still working in the boxes that are in Utah and in Nevada, and I expect before the end of the month that that will be complete. We're ground-searching those things. Weather has been a major factor in that we've been kept out of those because of snow and other conditions.

We didn't really give up on the West Coast even. We did that one time even. We had an effort to walk along the coast of California, knowing that there's a possibility that things might wash up on the beach. In fact, that showed no results; but we feel like that there are groups who walk the beaches routinely that were briefed about what might wash up and something may show up in the future.

In doing all this, I've used a U-2, a DC-3, forest penetration radar, hired parachutes, 37-plus helicopters, 10-plus fixed-wing aircraft, imagery from two different satellites, more than one type of hyperspectral scanner, forward-looking infrared radar, the Civil Air Patrol. And, yes, the rumor's true, I even tried to use a blimp.

The one tragedy that came out of this is that we did lose a helicopter that two people died in. One of them was a U.S. Forest Service person. The other was a helicopter pilot from the Grand Canyon area. Other than that, the safety record in injuries and to the 5,000-plus people that we had in the field every day was remarkable.

As of April the 28th, we opened the *Columbia* Recovery Office, and that's located across the street here in the Emergency Operations Center in the Control Center. We ran parallel for two days with the operation in Lufkin to make sure there was no hiccups, no disconnects. In fact, that place is up and operating and we are receiving calls, anywhere from 10 and 16 a day. Our intention is to respond to all of those.

We have a contract with the same people who are picking up and cataloging and logging the debris for the normal search. When necessary, we'll send those people out, even if it takes decontamination. We have the skills. We have a storage area that we have at the NASA Bloom Base in Palestine. So if things are large enough that they can't be FedExed, we will take them up there and store them and then get them down to Kennedy at the appropriate time.

General feeling is that we're going to see a great, big peak around November, when hunting season happens. We've done an awful lot to educate the hunters and we've provided packages for when they get their licenses, where they give some numbers to notify us if they run across things. Unfortunately, there are a number of potentially hazardous items still out in the woods someplace. Those are primarily pyrotechnics and there's a couple of fuel tanks that probably have been open and probably are safe, but you don't know.

All of the local emergency response agencies, all of the county judges, all of the people that would be affected by that have been notified. We passed out circulars. We passed out fliers, pictures, information. So hopefully no one will get injured; and if they find it, they know who to call and how to get it back in.

At some point in time, the *Columbia* Recovery Office here will close. The phone will not go away. We have a toll-free number that can be called, and the phone will not go away. It will be answered by Kennedy Space Center. That will continue for a long period of time. As people find things, they can call it in. In fact, I think you can still call a number for *Challenger*. So that will continue on, and we will close the CRO here.

The number of people. Like I said, there's over 130 Federal agencies. The number of people to thank is endless, and I've named a few of those agencies already. Interestingly enough, there's been a great deal of interest in our operation from other areas, in that, with the heightened awareness of terrorist threats and things like that and Department of Homeland Security, the size and magnitude of this operation has piqued interest and that they have deemed might be a model for following in the event of a similar type response. So we've had a lot of people come down and talk to us and see, try to understand how we did this, how we put it together, and how it worked so well.

That's it.

ADM. GEHMAN: Thank you very much. Any questions?

I'll ask one, Mr. Whittle. I am interested in this last point you brought up, in the sense that, from our visits to you and also from what I understand from reading reports, that the level of local, state, and Federal cooperation was remarkable, maybe unprecedented in a large operation where you have lots and lots of people. And you didn't mention how much this cost either. So there was a considerable amount of money involved in this. My understanding is -- and I think most people agree -- that the level of local, state, and Federal cooperation has not been exceeded in other major instances in this country. Do you have any idea at all as to what to attribute that?

MR. WHITTLE: I get asked that a lot. I think that there was a single-mindedness. Everybody felt ownership, and there was a single purpose. You know, it almost became a family. From the people out in the fields, the U.S. Forest Service folks that were 12 hours a day out there, marching through the fields, sleeping in tents at night, they were all really dedicated to this and proud to be there. That was kind of the attitude for everybody.

ADM. GEHMAN: And the cost? I could ask FEMA, I guess. Really FEMA paid.

MR. WHITTLE: FEMA paid a great deal of that, and the costs are going to be in the $300 million ballpark. They said I was really good at spending money.

ADM. GEHMAN: You did a great job, and I'll just make a comment here for the Board that we have authorized the expenditure of a few dollars to create an official momento that we intend to give to all those people, a piece of paper, a parchment with a nice certificate in which we recognize all those organizations, and then some kind of small coin or medallion that we can give to those people that we would like to recognize all the people that took part in that. I happen to know that you have an accurate list of who it was that you want to recognize.

MR. WHITTLE: Yes, I do.

ADM. GEHMAN: Thank you very much. The Boards wants to recognize that work, and we will do that. Thank you very much.

Mr. Hill.

MR. HILL: I had a few charts that I brought. Mostly pictures to give you an idea of where we ended up with the various facets of analysis. On the next page I summarize more or less everything that we did on the team. I don't intend to go into a lot of detail. I can say at a high level we took the public reports, we took the video, we analyzed the video to try to come up with trajectories for the debris we see coming off, build footprints. We use those footprints to then go search radar databases with the NTSB to find signs of that debris falling down through the radar. We arranged the AFRL radar testing, some of which you heard about just a little while ago, for both the Flight Day 2 object and to give us some sense of truth on whether or not we could, in fact, track the most likely debris in the air traffic control radar or the C bands that we use for ascent.

We also have been talking some about luminosity and spectral analysis, and I'll talk about a little bit of that here in a few minutes. And we went through various other sensor data both with the DOD and with NOAA and the USGS. I can summarize all of that to say that outside of telemetry we have from the vehicle, the OEX data, and the public video, we really have no external data that adds any engineering value yet to the investigation.

We have some ongoing work. If you go to the next page, on that last piece let me just mention on the bottom bullet we have not yet run the tests at Ames to try to use luminosity to estimate mass and drag of the objects that we see in video. We have a good test plan; and we're in the final throes now of deciding if we, in fact, are going to manufacture those test samples and conduct those tests. We pretty much have dropped the spectral piece of the analysis just because confidence is so low that we would get meaningful data.

Everything else you see on here is the open work. It really is just final cleanup work. We have a handful of videos still to process through to calculate relative motion and trajectory for the individual pieces of debris. We've gone through all the radar databases that coincides with our generic debris footprint from California all the way to Texas. We have a few backup passes we want to make through that radar database, and we have some final analysis to do with the radar test data that we already have in house. I'll describe what some of that is here in a few minutes.

The next three pages are debris timelines. You've seen iterations of these, and I think you have this copy. This is the latest and greatest copy from April, and I admit it's difficult to read here in the resolution that I brought.

The big-picture story is, as you've already seen, we know we were dropping debris from California to Texas. Chances are we were dropping debris in areas that do not show up as white dots on this trajectory. These are the ones that we had best angles, best lighting, and we were fortunate to catch in video. Our expectation is if we had more videos from different angles, we would probably have more white dots on here.

ADM. GEHMAN: The white dots represent the position of the Orbiter when the debris came off; they don't represent the ground.

MR. HILL: That's correct. That's the point in time when we clearly see a distinct piece of debris coming off the vehicle, or a couple of indications of flares, which you see out here over eastern New Mexico. There's also a flash there over early Nevada and there's a debris shower. So we have 20 distinct pieces of debris we capture in video plus this thing we call a shower, which looks like some large piece that then splinters into many pieces and then the two flares.

The next two pages just show you the same information with where the people were standing that took the video and their field of view. Most notable, we added one, way to the south in San Diego, which in spite of the range they were at and the 5-degree elevation on the horizon that the video captured the Orbiter, they, in fact, capture the flash and the Debris 6 in their video.

On the next page, it shows you the rest of the trajectory to Texas. You can see the about minute-and-a-half-or-so video gap we have from eastern New Mexico to across Texas. I guess the other thing I would point out is -- and I think you have heard this before -- while we appear to have relatively continuous coverage from that point over east New Mexico all the way back to California, there are places in the video where the tracking was not good or that the angle was not good and we actually can't see the Orbiter at all times. But it's pretty darn close.

On the next page. This is an early generic footprint that we generated from the East Coast all the way to Texas. This is based on some top-level assumptions on where tile would fly if we were to be shedding tile all the way from California to Texas. That area in the middle would be the non-lifting box, which would be our highest-probability area where we would expect to be finding debris as we drop across the CONUS.

On the next page, this is the latest and greatest set of footprints we have for relative motion that we have, in fact, measured off of all the debris. There are a handful still of individual pieces of debris that don't show up here with specific footprints. We have those videos in work, but this already gives you an indication that we have near-continuous footprints, even based on really good trajectory analysis. So from California almost all the way to Texas, we have almost continuous overlap, which clearly makes your chore of going out and searching out west a large one. If each one of these large rectangles represents, say, a single tile, looking for a tile in an area like that is a huge task.

Again, that thin, dark area in the middle, that would be that non-lifting area. That is our highest confidence area where we would expect to find the debris.

ADM. GEHMAN: You're talking about these little lines here.

MR. HILL: Yes, sir. If for whatever reason the debris was to take on some amount of asymmetric lift -- if, for example, it was to drop as a flat plate and not be tumbling -- it could venture off into the wider part of the rectangle.

On the next page, this is an old overlap map. We have an updated one that we're doing some work on to refine, but just to give you an idea how we tried to sharpen the pencil a little bit to come up with better areas to search out west rather than that large swath, we took the areas where all the highest-probability boxes overlap and you see those as the darker regions on this map. So those would be the places that, based on ballistics and trajectory analysis, would give you the highest probability to find something if we were to put people on the ground to search. You know, for comparisons, that first one you see there over the Nevada-Utah border, that's about a 300-square-mile box. It's still very large if you're looking for, say, a single piece of tile.

I guess I'll also point out that I keep mentioning a single tile. We don't necessarily know these are tiles. Our expectation is what we see coming off is something small.

Last thing I'll say on this picture. If you look over Texas, you see a very faint overlap area, just kind of a light gray; and towards the end of that light gray box is where Littlefield, Texas, is. That's where that Littlefield tile was found. And if you back up from there, our analysis shows that if that tile came off in that size, then it would have been shed somewhere in the Flare 1, Flare 2 area over eastern New Mexico.

Next page. Now, going back to Dr. Kent's radar tests, what this shows you is for the radar data that we have finished the analysis for. All of these circles show what the detection ranges are for each one of those radars. The large black circle would be the range of the radar in and of itself. The smaller dotted lines would be tuned to specific materials. The thing to note is the green circle out to the red circle, the relatively larger circles, those are all the leading edge components. The little light blue circle in the middle, that would be individual tiles or tile material. So the thing you would conclude from this, of course, is very low probability, at best, of us being able to detect tiles falling through any of these footprints.

You can see the ballistic footprint above these radars. Now, there are other radars that you see up here in red X's that we have not mapped. The analysis is still in work. I expect to have that in the next week or two. My expectation when we finish is there are only going to be a few cases where we have a possibility of detecting tile anywhere over the ballistic footprint, which was not happy news for us because it does give us less confidence that the radar threads that we're finding in many cases really could be tiles. They could still be some other leading edge type of component; but as you can see, it would have to be something relatively large.

On the next page I have a couple of different footprints. The thing I would like to point out is in the lower right you see the large black cross. I sent some folks back within the last few weeks to look through the thousands of reports that we have from witnesses that just saw something in the sky. These are reports that have gotten a lot less attention from us once we saw the video and we found we could calculate engineering data from the video.

We went back through all the reports and we tried to pull out the reports from people that saw things that could have been anywhere in any of our actual footprints. Of those, this one report was the one that stands out as the only one that's significant. This fellow was in a camping site 70 miles north of Las Vegas, saw the Orbiter fly overhead. Ten minutes later, looking due east, he saw something bright falling out of the sky, between him and a peak that was in front of him. This is where he was standing, overlaid on top of the Debris 1 footprint, a relatively old Debris 1 footprint. On the next page, similarly on a Debris 6 footprint. You see our high-probability box just to the east of where he was standing.

If you go to the next page, this is a close-up of one of those overlap footprints. That small green rectangle you see just east of where he was standing in Delamar Lake is Radar Search Box 8. We've already had NASA folks on the ground out there that put where he saw this object within a mile of our last radar return in Search Box 8. I haven't heard the results, but it was my understanding that by mid-week last week we had people on the ground, actively searching that area for this object.

MR. WHITTLE: We did, yes.

ADM. GEHMAN: Dave, you want to comment on that?

MR. WHITTLE: Yes, we do have people out there; and that box may be finished today. As of yet, we haven't found anything.

ADM. GEHMAN: Thank you.

MR. HILL: On the next page. I'm not going to read all these. What I'll tell you, though, is the radar search boxes or the NTSB search boxes that Dave mentioned, those are listed in this table and the next page. All of those overlap areas you saw on the overlap map, they all show up here. The Delamar Lake sighting shows up on here. What we have done is these two pages summarize the 21 search areas that we have out west, and that's a combination of our radar search boxes, witness sightings, or our trajectory footprints. They're in priority order, based on how good the data is, say, from radar, how close the radar thread or the witness sighting is to our high-probability areas, et cetera. The only other thing I would point out is you can see you don't have to go very low on this list and the areas you are talking about searching are enormous. The one that I have highest confidence in from a ballistics perspective would be that Priority Number 7, which I already mentioned is 300 square miles. The next one after that is 1200 square miles.

I have absolute certainty that our trajectory analysis is good and that the objects we see coming off in video are, in fact, in these areas; but as Dave and I were talking about a little while ago, sending people out to a 300-square-mile area or a 1,200-square-mile area to look for something that could be a tile is a tough job.

ADM. GEHMAN: All right.

MR. HILL: Skipping on to page 16. I'm not going to go into a lot of detail. I'd just like to explain this is the evolution of our generic footprints over Texas. So this would be our post-breakup debris footprint. Within an hour or two of the accident, the February 1st release was published; and that really was just a dark line that essentially was under the ground track. That was a really simplified analysis just to give us a place to start. Within three days that was expanded with Monte Carlo sims to that gray rectangle, giving you a larger footprint. By February 7th we had a better time on the estimated breakup. That moved that gray box up to the right, which gives you then that purple rectangle. That's a function of we continued the left roll, so we continued to get a little bit more lift. That moved then your debris footprint.

After two months of detailed analysis and adding in real weather and much more sophisticated Monte Carlo simulations, we ended up with that yellow feather-shaped footprint that you see there or the orange feather-shaped footprint. The yellow one is based on a breakup time, or an end of lifting, of 13:59:37, and then 25 seconds later we ran another case for lifting that continued and that gives you that second orange footprint.

You go on to the next page. This just shows you where those areas are over Texas and Louisiana.

On the next page, interestingly, the NASA 220 center line, this is the line that Dave Whittle and company used to search in East Texas and Louisiana. That center line was based predominantly on their observations of where debris was being found, and it matches up very closely to the center line for the orange footprint. You can see in the upper right, it's only about a mile off at the end from the center line of our 1,400 footprint, and also the difference in the center lines between the yellow and the orange footprint is about 4 miles on the east end and about 1 to 2 miles on the west.

On the next page, this just gives you an idea of where the significant items were. This isn't everything found; this is just from the significant items list. You can see how they're distributed relative to the footprints. You can also see up in the upper right where the SSME power heads were found, right on the center line of that orange footprint.

Then my last two charts. This is a combination of all the radar hits in the NTSB database from 13:59 to 14:10. You can notice the high concentration of those radar returns right in the middle of the footprint. A lot of the rest of what you're seeing is just standard noise.

If you go to the next page, this is a combination of the data from 14:30 to 14:40. You can read this essentially as background noise or clutter that you would typically see in this view.

If you go back one page again. Again you can see the high concentration, which gives us good confidence that we've definitely broken the code on how to generate these types of footprints.

I guess the last thing I would say is, were we to have to go through this exercise again, we have done enough work now that we could generate these footprints at this same level of accuracy within about two hours of the accident.

That's everything I have.

ADM. GEHMAN: Board members?

Mr. Hill, what do you think is remaining for your working group to do?

MR. HILL: Primarily processing the last handful of videos to calculate relative motion and good footprints on the remaining western debris and then summarize everything that we've done.

DR. WIDNALL: I'll ask my favorite question. What drag coefficient did you use?

MR. HILL: Drag coefficient. You know, I'm not positive. We used an L over D of zero to .15.

DR. WIDNALL: I saw that.

MR. HILL: And we actually measured the ballistic number from relative motion. So we didn't have to pick a drag coefficient.

DR. WIDNALL: Then in order to generate the footprint, you would have to -- I mean, if you were trying to estimate where the thing landed.

MR. HILL: Even with the footprint, we based that on ballistic numbers, independent of individual CDs of objects.

GEN. BARRY: Paul, have you given up on the Caliente, Nevada?

MR. HILL: I'll speak for myself. Personally, where the Caliente, Nevada, radar search boxes appear in our ballistic footprint gives me lower confidence that it's something that belongs to us, just because it's so far off our non-lifting box. So my confidence is not high that that is something that belongs to the Orbiter. I think it's good radar data; I just don't think it belongs to us necessarily.

DR. WIDNALL: I was intrigued basically by Greg Byrne's image analysis. Are you planning to use image analysis to try to estimate? I mean, if you actually had a ballistic coefficient of a piece of debris, based on, you know, you might be able to say that's a tile or that's a part of an RCC, because they're quite different.

MR. HILL: Well, what we have done is we've used the ballistic coefficients that we've measured to sort of bound which objects fall in the category of the ballistic numbers we're seeing in video. So typically the ballistic numbers we're measuring in relative motion range from about 0.5 to on the order of about 5 pounds per square feet, which, in fact, exactly brackets the full range of intact tiles. There are pieces of other external components, leading edge components that, if you were to break them down small enough, would also fit in that category. I guess another conclusion you could reach is because those are the ballistic coefficients we're measuring, we don't think we're seeing anything large coming off in video. I don't know if that answers your question.

DR. WIDNALL: Well, I guess my own view is that probably many of those debris are tiles. I mean, I literally cannot imagine 14 or 20 pieces coming off the Shuttle without the thing just melting. So I guess I have to believe a lot of them were tiles and I would assume that you could identify that from the trajectory, that these would decelerate much faster than structural elements.

MR. HILL: We can definitely show that the ballistic behavior we see of those objects is consistent with an intact tile or a tile fragment. It doesn't tell us for sure that it is, but it is consistent.

ADM. GEHMAN: All right. Well, thank you very much. Mr. Hill and Mr. Whittle, both of you represent the top of an iceberg of a lot of people -- particularly Mr. Whittle, who's got 30,000 people working for him on one day or another. Also, Mr. Hill, your group has done a lot of work to help us understand what happened; and we're very grateful. We're grateful to not only you two but also all the people that you represent. We'd like you to pass that on to everybody. You've done a great job, and we thank you for your candor and your willingness to discuss these things with us here at this hearing.

This hearing is closed, and we'll be having a press conference right here in this room in 34 minutes. Thank you very much.

(Hearing concluded at 12:24 p.m.)

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June 12, 2003
Washington, DC

Columbia Accident Investigation Board Public Hearing
Thursday, June 12, 2003

9:00 a.m.
National Transportation Safety Board
Conference Center
429 L' Enfant Plaza, SW
Washington, D.C.

Board Members Present:
Admiral Hal Gehman
Dr. John Logsdon
Mr. Steven Wallace
Dr. Douglas Osheroff
Major General John Barry
Dr. Sally Ride
Rear Admiral Stephen Turcotte

Witnesses Testifying:
Mr. Allen Li
Ms. Marcia S. Smith
Mr. Russell D. Turner
Mr. A. Thomas Young

ADMIRAL HAROLD GEHMAN: Good morning. This public hearing of the Columbia Accident Investigation Board is in session. I'd like first of all to thank Administrator – Chairman Ellen Engleman for allowing the Columbia Accident Investigation Board to use the NTSB conference and briefing facilities. They're magnificent, and we're very appreciative of them.

Today, we're going to review issues having to do with resources and management, and we have two panels of two very distinguished experts who are going to help us understand that. The first panel is made up of Mr. Allen Li and Marcia Smith.

Allen Li is the Director of Acquisition and Sourcing Management at the General Accounting Office. He is responsible in his position for several accounts, which include NASA as well as several Department of Defense accounts, like tactical aircraft. Previous to this, his duties included such things as energy and science, nuclear safety and the Department of Energy management issues, which turns out to have been something that we looked at, also. So, we very much appreciate the richness of your background and you're willing to help us here. Mr. Li has been selected to the Senior Executive Service in the GAO, and is a senior member of the American Institute of Aeronautics and Astronauts.

Marcia Smith is a senior level specialist in Aerospace and Telecommunication Policy at the Congressional Research Service of the Library of Congress and, as that, of course, she serves as a policy analyst for all the members and all the committees of the Congress on matters concerning U.S. and foreign military and civilian space activities, and on telecommunications issues.

Previously, she held the position of section head for Space and Defense Technologies, as well as Energy, Aerospace and Transportation Technologies in that division, which again bears directly on what we have looked at in this area. She is a Fellow of the American Institute of Aeronautics and Astronautics and the British Interplanetary Society, as well as the American Astronautical Society.

Before we start, I would like to request that the two panel members affirm to the Board that the information you are providing to the Board today will be accurate and complete to the best of your current knowledge and belief.

MS. MARCIA SMITH: I do.

MR. ALLEN LI: I affirm so.

ADMIRAL GEHMAN: Thank you very much.

I would ask the panel members to introduce themselves and add anything to their biographical sketch that I may have underplayed or gotten wrong, and if you would – first of all, I would like you both to introduce yourselves, and then we will get an opening statement. Marcia, you want to go first, please?

MS. SMITH: Well, thanks for the very nice introduction that you gave me already.

I would like to explain briefly what CRS is and does. CRS is a department of the Library of Congress but, unlike the rest of the library, which works for both the public and Congress, CRS works exclusively for the members and committees of Congress, providing them with objective, non-partisan research and analysis.

We do not take positions on issues. We don't make recommendations. Our job is to help the members and their staffs sort out the issues, look at the options that they have available to them, and help them understand the pros and cons of those actions. So, we don't have opinions. People often ask me what my opinion is, but I'm afraid that only my teddy bear knows what my opinion is. Everybody else gets pros and cons, and I apologize for that if you were hoping for some opinions this morning. And I have been a policy analyst at CRS since 1975, except for one year from 1985 to '86 when I served as Executive Director of the National Commission on Space that developed a long-term, 50-year plan for the space program.

ADMIRAL GEHMAN: Thank you very much.

Mr. Li?

MR. ALLEN LI: Thank you, Admiral.

My name is Allen Li. I've been with GAO now for almost 23 years. Been working on NASA issues for over five years. Have had the opportunity during that time to look at a lot of the programs that NASA's had, and had the opportunity to work with their top management in that regard.

The only thing I would like to add, similar to what Ms. Smith was talking about in terms of what CRS does, GAO does provide recommendations. When we do our particular reviews, we also are part of the legislative branch, and provide advice and information to the Congress. But, we do provide recommendations, as we do in different programs. If we see there are certain management issues that need to be brought to their attention, we do so.

The statement that I will provide in a few minutes is largely based on a report that we had provided, called the "Performance and Accountability Series" that we provide to the Congress every two years, and it's our snapshot of what is happening at the agency and what are some of the challenges that that agency faces.

Thank you.

ADMIRAL GEHMAN: Thank you very much, and I – we understand the caveats, but they don't – to me, that – they don't seem to be very inhibiting to what we need to get at, and we're sure that your report will be very, very useful, and we appreciate you – your willingness to help us with this investigation.

Which one of you is ready to go first, and the floor is yours, and if it's all right with you, we'd like to be able to dialogue and ask questions as you go along, if that's all right. You may have been told about the Board's tradition, that we – our tradition is that the briefer never gets past the first few graphs, so let's go ahead – if that's all right with you, we'd like to ask the questions as the issue comes up, because it's both fresh in your minds and fresh in our minds.
Thank you very much. The floor is yours.

MS. SMITH: Absolutely. I've watched all of your hearings, so I'm familiar with your tradition, and I'm hoping that I've left the most interesting slides till last so that I can get through the first few. If the folks in the slide room could bring up my presentation? There we go, and we can go to the next slide.

You asked that I speak to you today about the NASA and the Space Shuttle budgets over the past 10 years. I thought that it would help to first put the NASA budget into context because, of course, budgets have to do with setting priorities. And so, I think it's interesting and important to understand where NASA fits in the total federal budget. So, this shows you, for fiscal 2002, the last completed fiscal year, which I'm using as my benchmark for this 10-year look-back.

This is how the funding was split up in the $2.2 trillion federal budget. Mandatory spending was 56 percent, discretionary spending, which includes NASA, was 36 percent, and the interest on the national debt was 8 percent. And you can see on the slide where NASA fits into the non-defense discretionary account, which is 19 percent of the total federal budget. Next slide, please.

This shows how that 19 percent gets broken up, and how much of that ends up at NASA. The defense discretionary is on the right-hand side, and the non-defense discretionary is on the left-hand side. The agencies that are in the other category, by and large, were smaller in terms of dollar amounts than NASA, so these are sort of the largest of the various agencies that get funded in that account.

NASA is part of the Veteran's Affairs, Housing, Urban Development independent agency's appropriations bill, and I'm sure that everyone on the panel is very familiar with the federal budget process, but it might help if I just gave a 30-second review of how budgets happen in Washington.

ADMIRAL GEHMAN: Are you or Allen – are you going to talk about whether that 2 percent is going up or going down, or what's the historical trend there?

MS. SMITH: I have some trend charts in here.

ADMIRAL GEHMAN: Thank you.

MS. SMITH: In Washington, the way budgets happen is that agencies develop budgets through internal processes. They're submitted to the Office of Management and Budget, which is part of the White House, and so they're looking at various agencies' requests within the total context of the federal budget. That comes to Congress usually in February of each year as the President's request to Congress. It is Congress – under the Constitution, that has the responsibility to decide how this money is going to be spent.

Congress passes a budget resolution that sets the parameters within which the various Appropriations Committees have to decide how to spend the money. And these agencies are all divided up into 13 different Appropriations Committees on Capitol Hill, and NASA is part of the one that funds on Veterans, HUD, the Environmental Protection Agency. It used to fund FEMA, the Federal Emergency Management Agency, although that's now been shifted into the Department of Homeland Security. And so, there are a number of different agencies in the appropriations bill that funds NASA.

ADMIRAL GEHMAN: Now, if my understanding of the process is correct, if you take – if you take something like education, for example, that actually rolls up a whole lot of agencies and things like that into an education budget.

MS. SMITH: This education, 7 percent, is what OMB shows in its tables as the amount dedicated to, I believe, it's the Department of Education.

ADMIRAL GEHMAN: Okay. Well, let me rephrase that. In most of those other categories, you have up there a cabinet-level agency rolls up a whole lot of agencies and subordinate budgets into one submission. But, in the case of NASA, they don't – their budget – they don't have a cabinet officer, and they're not in a department. They're an independent agency. So, they're – so, there's a little difference there, isn't there?

MS. SMITH: Well, you may have a department like the Department of Commerce, and within the Department of Commerce you have the National Oceanic and Atmospheric Administration, and you have the Bureau of Export Administration. So, you have different offices within a cabinet department. NASA is a stand-alone independent agency, like EPA is.

ADMIRAL GEHMAN: That's what I was referring to. There's a step that NASA doesn't have to go through, in the sense that – take the FAA, for example. They submit a budget, but they submit a budget to a cabinet agency, which put – which does things with it, and – whereas NASA's being an independent agency doesn't have to go through that.

MS. SMITH: NASA has an internal process through which the Administrator of NASA submits a budget request to the White House, whereas FAA would submit a budget to the Department of Transportation, and then it would go to the White House, correct.

ADMIRAL GEHMAN: That's correct. Thank you.

MS. SMITH: So, the Appropriations Committees look at how much money they've been allocated, and they divide it up amongst the agencies within their jurisdiction. A budget gets passed. It goes back to the agency. The agency looks at what Congress did with their budget. They then decide if they're going to try and make a little bit of change here and there, and they notify Congress of those changes through something called an operating plan.

There are initial operating plans, intermediate operating plans, final operating plans. Congress also, after the initial appropriation, can pass supplemental appropriations. And so, throughout any given budget year, there are a number of steps that are going on that are deciding exactly what the final amount is that any agency is getting for any particular activity.

DR. JOHN LOGSDON: Marcia, one more question. Are there cabinet agencies with smaller budgets than NASA?

MS. SMITH: I don't know that off the top of my head. Do you know, Allan?

MR. LI: No, I don't know. I wouldn't think so. I don't think so.

DR. LOGSDON: There are a lot of agencies with smaller budgets that NASA, but not cabinet agencies.

MS. SMITH: Next slide. So, this is the NASA funding trend line over the history of the agency. The top line number is in 2003 dollars, the bottom line number is in current year dollars, and the first big spike you see, of course, is the Apollo program funding, and the next spike you see around 1987 is the funding for the replacement Orbiter after *Challenger*. So, those are the various trend lines. Next slide, please.

For the past 10 years, fiscal '93 to fiscal 2002, this shows how much the President requested for the NASA budget, and it shows how much Congress appropriated for it, and what the change was. Many people are interested to see how NASA's budget as a whole has changed over these years, which have been very difficult years, as President Bush – the first President Bush, President Clinton and Congress all were striving to reduce the federal deficit. And agencies, including NASA, were working under very austere conditions. So, this is how the NASA budget fares overall. Next slide, please.

For fiscal 2002 – again, that's the last fiscal year that's been completed – NASA's budget authority was $14.9 billion. And within NASA, these are the different activities that NASA is engaged in, so when you try to look at the Shuttle budget, which I'm getting to, this is what the Shuttle must compete with, if you will, in terms of resources, the resources that the head of NASA has to deal with when he's allocating them amongst the various activities. And you can see that the Space Shuttle was 23 percent of the NASA budget in fiscal 2002, which is the largest single percentage for any of these activities.

ADMIRAL GEHMAN: And manned space flight is just under half.

MS. SMITH: Human space flight is just under half. And there's been a lot of discussion about the replacement programs for the Shuttle. Those are funded from a different account. That's in the Office of Aerospace Technology, the X-33 program, X-34, the Space Launch Initiative. So, they are not, if you will, in direct competition with the human space flight side of the bar here. They're being funded within that account. But, of course, within the total NASA budget, there are always competing demands upon the total amount that's available to the agency. Next slide, please.

This shows just the Shuttle budget, and I decided to use as my base year – my benchmark year for this as 1981, the year of the first Shuttle flight. I thought it would be more useful to see the total trend line over that period of time rather than just the past 10 years initially.

MR. STEVEN WALLACE: May I interrupt, Marcia? We – unless you're going to describe it later, could you perhaps describe a little bit of the division between the Space Station budget and the Space Shuttle budget idea? In other words –.

ADMIRAL GEHMAN: – Go back one.

MS. SMITH: Go back one slide, please.

MR. WALLACE: Shuttle, you know, basically, how – are they funded entirely independently, since the programs are so intertwined and sort of rely on each other?

MS. SMITH: They are very interdependent. That was not true historically, but it has been true at least through most of the 1990s as the primary purpose of the Space Shuttle is to assemble and service the Space Station Program. So, they are very closely intertwined.

You can see changes throughout the years in how NASA has been organized – NASA headquarters has been organized in terms of managing those programs and bringing them under the rubric of the Office of Space Flight, and how the Associate Administrator for Space Flight has handled those programs and bringing them much more closely together. And as you'll see in some of the subsequent slides about Space Shuttle funding, some of the funding from the Space Shuttle Program has moved over into the Space Station account as the Space Station has encountered funding difficulties.

MR. WALLACE: There's sort of a continuing debate, I would assume, about who pays which parts of the bill on this between the programs?

MS. SMITH: Well, in one sense. When NASA talks about the costs of the Space Shuttle – of the space Station Program, for example, they do not include the cost of the Space Shuttle flights. So, when you see budget estimates for Space Station, that it's going to cost 17.4 billion or 25 billion or whatever it is, that does not include the cost of the Space Shuttle flights, even though you obviously can't assemble and operate the Station without the Shuttle. So, in that sense, the Space Station total funding is not taking into account the amount of funding required for the Shuttle launches.

MR. WALLACE: So, if you say there's – the Shuttle budget for 2002 is 3.3 billion, if we just – you might say that, what, three-quarters of that is more or less, or three-quarters of the program, or some percentage is in support directly of the Space Station.

MS. SMITH: Yes, it is. Next slide.

So again, this is the Shuttle budget since the first flight, again seeing a spike there for the replacement Orbiter.

ADMIRAL GEHMAN: Let me ask a question about graphsmanship or chartsmanship. I understand this, and I appreciate you putting it both in current year dollars and in any year dollars. A lot of times, I've seen this same chart in which, instead of using 2003 dollars, they use 1981 dollars. And, of course, if you did that, the yellow line would show, in real-term spending, Shuttle funding is going down.

MS. SMITH: Well, you can choose whatever base you're – you wanted to choose. I put it into the current year dollars because I thought that that would be most helpful to you. But, one can calculate these numbers in a variety of different ways.

ADMIRAL GEHMAN: That's right, okay. But, what I mean is, would you agree that, if the yellow year line were to be 19 – then-year dollars, 1981, then the yellow line would trend down?

MS. SMITH: I haven't done the calculations, so I couldn't presume what it would show.

ADMIRAL GEHMAN: Okay, all right.

MS. SMITH: Next slide, please. This is getting back to the 10-year time frame that you were interested in, and it's important to notice that this is the President's request up here. This is the request to Congress, what Congress did with it, what happened to it after that in terms of operating plan changes or supplementals that were done to it. What this does not include is the other step in the process, which is going from NASA to the White House, to the Office of Management and Budget. It doesn't show changes that were made from the agency's request to the White House.

They are also, obviously, a very important player in the whole budget ballet that goes on, the NASA number, the White House number, and the Congressional number. What comes to Congress is a White House number, and what happens prior to that process, the negotiations that go on between NASA and the White House, are very closely held, and I do not have data on the so-called pass-backs between OMB and NASA as they formulated these budgets.

ADMIRAL GEHMAN: Excuse me, go ahead, Jim.

DR. LOGSDON: First, a comment, Marcia. Neither do we. I think the observation that we should look at that chart is that Congress may re-shuffle, as you're gonna show in a while, the money within the Shuttle budget slightly, but that Congress has not made major changes in what the President has requested for the Shuttle, that the key decisions are what the White House requests.

MS. SMITH: There were some substantial decisions in the early 1990s regarding the advanced solid-rocket motor program –.

DR. LOGSDON: – Right, but in recent – in the more recent years –.

MS. SMITH: – But in the more recent years, a lot of the changes, if there were changes, would have happened prior to the President's request coming to Congress. But, I don't know whether there were changes or not.

ADMIRAL GEHMAN: Let me – would you help me with what the definition of "final amount" is? Is that –?

MS. SMITH: – That is what's in NASA's final operating plan. It's the amount at the end of the fiscal year as to how much actually ended up being allocated to the Shuttle after all the puts and takes throughout the year.

DR. LOGSDON: This is not the appropriation?

ADMIRAL GEHMAN: No, no, this is how – what actually was spent, obligated.

MS. SMITH: And the subsequent slides will show you the changes that were made to it, both by Congress and by NASA.

ADMIRAL GEHMAN: So – but this –?

MS. SMITH: – I have the other data, but I thought that I would be overwhelming you with too many budget charts, so I didn't put in all the tiny little numbers that showed you every step of the way. But, the – it was NASA that developed these numbers. They were developed in advance of the February 12th hearing, the joint Senate/House hearing, and these are NASA's numbers, and they do show a greater level of detail. So, if you need that, I can provide you with an estimate.

ADMIRAL GEHMAN: No, I just want to make sure I understand that –.

MS. SMITH: – This is a final amount that is a final final amount. It's not the Congressional appropriation.

ADMIRAL GEHMAN: No, no, I understand, after all the puts and takes and pushes and actions. But, when I read this chart then, at the yellow line, I should assume, then, that NASA actually spent, at each year, less than the President's budget?

MS. SMITH: They would have had the authority to spend less. This is budget authority. I don't believe it's outlays.

ADMIRAL GEHMAN: Well, that's why I was asking what "final amount" means, whether or not final amount – I got the impression that this was the final amount at the end of the year after – based on their operating budget.

MS. SMITH: Of budget authority.

ADMIRAL GEHMAN: Of budget authority, right. That's pretty close to saying that this is what they actually spent.

MS. SMITH: I do not know that these are outlay numbers.

ADMIRAL GEHMAN: Okay, they may not be outlay numbers.

MS. SMITH: Because, sometimes money can shift from one fiscal year to another fiscal year, so it would have been spent eventually. But, whether it was spent in this fiscal year, I don't know. I think this is budget authority.

MR. LI: They have things like carryovers that, when they don't, even though you obligate the funds and you don't spend them, then they are carried over.

DR. DOUGLAS OSHEROFF: But, what's true is, from '93 through '98, or something like that, there is, in fact, a constant offset between those – not constant offset, but, obviously, the amount that – the final amount is always lower. So, you're – I mean, you can't carry that over from year to year. You could have a whole pile of money left over.

MR. LI: And we had testified on that actually a few years ago, and some of the issues in what causes a carryover are things like, on the Space Station, when construction was – did not go as planned, and things were not put up in space on the scheduled as they thought, then that expenditure would not have been made during that year, and that causes carryover.

MS. SMITH: And I think the next slide is going to answer some of these questions, as well, because they go into the detail year-by-year as to what the puts and takes were as time applies. If I could have the next slide?

And I emphasize this is NASA's explanation. This all comes from NASA data. This is not something that CRS derived itself. And I think I have five slides that show these. I don't know if you want me to go through all of them. In the interest of time, if you want, I can just show you a couple, because I think what you're interested in is mostly the trend as opposed to specific cuts or additions that were made.

ADMIRAL GEHMAN: Right. Well, why don't you go through the first one, and then we'll see if we get the idea. We're slow learners, but we are solid learners in this.

MS. SMITH: Well, you can see that, in the appropriations process, Congress cut $50 million. They cut that because NASA had terminated one of the upgrade projects, the electric auxiliary power unit. They also added 20 million for upgrades, they added 25 million for repairs to the Vehicle Assembly Building at Kennedy Space Center. So, that's what happened in the appropriations process.

Subsequent to that, NASA transferred $7.6 million to fund other agency requirements, and they cut 1.2 million for a rescission requirement. So, all of that would have happened after the normal appropriations process, bringing the Shuttle budget to a net change of minus 13.8 million in that year.

ADMIRAL GEHMAN: Once again, we have that top line up there, where the President's budget requested 3.283 billion, and then we have that final number over there again.

MS. SMITH: Right. It's the final final operating plan.

ADMIRAL GEHMAN: It's from their operating plan?

MS. SMITH: Final NASA operating plan.

ADMIRAL GEHMAN: Right. So, their operating plan, again – once again, there were a lot of changes in between there, all kinds of puts and takes in between.

MS. SMITH: These are the changes.

ADMIRAL GEHMAN: Yeah, I know, but they're – okay.

MS. SMITH: This is – these five bullets are what get you from the three – 3.8 to the 3270. That is –.

ADMIRAL GEHMAN: – Some of it was done by Congress, some of it was done by NASA in the execution of their operating plan? They moved money –.

MS. SMITH: – Right, and some was done by Congress because of NASA actions or at NASA requests, and that's the trend that you're gonna see throughout all of these changes, is that, you know, Congress is making changes, NASA was making changes. It – the only part we don't know is what changes the White House might have been making prior to the budget submittal.

And so, for 2001, Congress cut 40 million at NASA's request for a Mars Initiative, and my recollection of that is that there was a joint project between the Human Space Flight part of NASA and the Space Science part of NASA on the Mars program, and the Human Space Flight part decided not to pursue the program, and they didn't want the Space Science side to take the hit – the budget hit, and so they moved the money over. So, this was cut for that reason, and NASA also cut 6.9 million because of a rescission. So that, again, takes you from the 3165.7 to the 3118.8.

DR. LOGSDON: These are really kind of changes that's a margin. I mean, there's, what, less than 1 percent of the total budget, right?

MS. SMITH: Right, in these years.

DR. LOGSDON: Marcia, what's a rescission requirement?

MS. SMITH: A rescission – Congress can, in the actual appropriation bill or in a supplemental appropriation bill, take back money that they had originally appropriated.

ADMIRAL GEHMAN: It's a tax. It's a tax.

MR. LI: But that's not agency-specific. It's throughout the government, isn't it?

MS. SMITH: Very often – this is – I do not believe that this is a tax that various parts of an agency are sometimes required to pay. I know this happens a lot in DOD, that each program gets a certain tax amount to it. This, I believe, is in response to a Congressional rescission where Congress has said all the agencies are gonna take a .0065 percent reduction in order to balance whatever budget amounts they had available to them.

DR. LOGSDON: Rescissions are also congressionally mandated?

MS. SMITH: Yes, Congress can mandate rescissions. We just had a rescission in the fiscal '03 budget. There was a .0065 percent rescission across all the agencies except Defense.

ADMIRAL GEHMAN: Okay. Would you – let's look at the next couple, please, 2000.

MS. SMITH: Next slide. So, you can see these – the things on here that you might find of interest are, under 2000, the two bullets that are in italics do not affect the total amount available to the Shuttle, but they do change how the money is being spent within the Shuttle Program.

And the last one on there was $40 million that was identified for what was called the R2 mission. The STS-107 mission was actually undertaken because of Congress's interest in continuing the ability for scientists to have access to orbit during the phase of Space Station assembly. The original idea was that NASA would fly Shuttle science missions, the Space Station would get ready, and the science would continue on the Station. But, as the Station schedule slipped, there was gonna be a long hiatus where scientists would not be able to conduct research.

So, first they allocated money for the STS-107 mission, and then they subsequently said they wanted a second dedicated science flight and, for that, they specified $40 million. They called it R2, for Research Two. Now, in actuality, in 2000 was when NASA was looking at whether or not they had pushed too far on the Shuttle budget. They had had the problems with STS-93, they had the McDonald Report, and NASA decided that they needed to put more money back into personnel.

And so, this 40 million, as far as I know, was ultimately spent on plussing up the personnel accounts in the Shuttle Program, and on Shuttle upgrades. And the R2 mission has been canceled. There is no R2 mission any more.

ADMIRAL GEHMAN: It's interesting. When you look at that net change, which is essentially zero, but then you look at all these 25 and $40 million chunks of money moving around, it's kind of fascinating. Congress adds 25 million for upgrades, and then 26 million gets taken away by – for one thing or another, so you wonder about whatever happened to the upgrades. And then, they transferred 25 million for upgrades from operations, so that's not new money, that's just moving money from one account to another, and so you wonder what happened there. And then, Congress specifies how 40 million is gonna get spent –.

MS. SMITH: – But did not add the 40 million.

ADMIRAL GEHMAN: But didn't add any money, but they specified how 40 million was gonna get spent, which restricts NASA's ability to use that.

MS. SMITH: Except that they ended up using it for the Shuttle, anyway, for upgrades and personnel.

ADMIRAL GEHMAN: And they used it anyway. So, yeah – all right, thank you. Now, in '99, the 60 million, that is a pretty – that's a pretty healthy cut.

MS. SMITH: Yes, and you can see that Congress cut 31 of that at NASA's request to fund other requirements that I think that were in the mission support category at the time.

ADMIRAL GEHMAN: Fund other NASA requirements?

MS. SMITH: Right. And NASA cut 32 million itself, transferring the money to the Space Station.

ADMIRAL GEHMAN: To the Space Station.

MS. SMITH: But, they also added back in two million that they had for Space Station, so it ended up a net of 30, basically.

MAJOR GENERAL JOHN BARRY: Marcia, I know you're gonna talk a little bit on the remaining slides here, but since '94, when they combined Space Station and the Shuttle in the Office of Space Flight, could you give us an indication of the dance of monies that have been moving between Space Station and the Shuttle? Can you characterize – ?

MS. SMITH: – Well, according to this NASA table from which this is all extracted, between – in that time frame, there's 330 million that was transferred from Shuttle to Station.

MAJOR GENERAL BARRY: Over what years?

MS. SMITH: From '96 to 2000, I think.

ADMIRAL GEHMAN: And no flow the other direction?

MS. SMITH: Correct.

MAJOR GENERAL BARRY: Now would it be safe to characterize also that the increase in the Space Station has resulted in funding, but the Space Shuttle has been held back to an inflation level growth pattern? We have one character reference that made reference to that, and I just want to see if you share the same point of view.

MS. SMITH: The goal for the Shuttle Program, especially since the initiation of the Space Flight Operations Contract, was to hold the line on Shuttle spending while not compromising safety. That was the goal.

MAJOR GENERAL BARRY: And that goal was stated when?

MS. SMITH: Well, it's been a goal for the Shuttle Program through the 1990s. And when they signed the Space Flight Operations Contract, SFOC, that – it was clear that that was the point that they were trying to make by getting more contractor workforce involved in the program. So, in essence, if you see a level budget for the Shuttle, that is exactly what they were shooting for, as long as it did not compromise safety.

They were always careful about this. And during these years when the Shuttle budget was very constrained, there was a lot of discussion about the fact that the budget was very constrained. There were hearings about it almost every year. I mean, there are hearings on the NASA budget every year. But, in addition to that, there were separate hearings on the Shuttle Program and on Shuttle safety almost every year during this 10-year time frame. There were reports from the Aerospace Safety Advisory Panel. The reports – you know, the McDonald Report that came out, GAO reports. So, it was well known that there were stresses on the Shuttle budget during this period of time.

ADMIRAL GEHMAN: We better let Marcia move on.

MS. SMITH: Okay, why don't you go to the next slide? These get shorter and shorter. Why don't we just go to the next one? Here, you start seeing bigger cuts. Why don't you go to the last one?

ADMIRAL GEHMAN: Wait a minute, go back one.

DR. OSHEROFF: We want to see where those cuts are coming from.

MS. SMITH: In '96, Congress cut 53 million for the Yellow Creek facility. This was a facility that was being built to support the Advanced Solid Rocket Motor program, the ASRM. The ASRM program was canceled, which you see on the next two slides, and there was interest, when they first canceled ASRM, in transferring some of the other

SRB work to Yellow Creek. So, they didn't cancel the facility at the same time they canceled the rest of the program. But, when they got to '96, they did finally cancel that facility, as well. So, that's all part of the decision to terminate the ASRM program.

ADMIRAL GEHMAN: In the – '96, there was a transfer to – of Shuttle – from Shuttle to the Space Station.

MS. SMITH: That's right.

ADMIRAL GEHMAN: In '95, there – that's just a cut.

MS. SMITH: That was just an across the – that was a cut, and you'll see a note on there. My colleagues, Dan Morgan and Amanda Jacobs, went back through appropriations reports to try and look at all of these numbers, and we have our own report about what the House did and what the Senate did, and what the final appropriation was. And we couldn't find this one in the appropriations reports. It's not that we doubt that this is what happened, because the NASA people are very meticulous about these things. But, we just put a note on there that we couldn't find that. We did find 141 million in cuts in the appropriations conference report.

ADMIRAL GEHMAN: Okay, thank you.

MS. SMITH: Next slide. And here, you see the termination of ASRM in '94. In '93, Congress had actually tried saving ASRM. The last year of the first Bush Administration had decided to terminate the program, but Congress thought that it still should proceed. And so, in '93, they were saving ASRM, but by '94 they were convinced that it was no longer necessary, considering the slips to the Space Station assembly schedule. And part of the reason for ASRM was to increase safety, and they were feeling comfortable that the Re-Designed Solid Rocket Motor, the RSRM, had demonstrated sufficient safety that they didn't need to go to the ASRM for that.

And that is my last slide –.

ADMIRAL GEHMAN: – Thank you.

MS. SMITH: I think the overall message of all this is that, you know, people want to know who cut what, and the answer is we know that Congress made cuts and NASA made cuts, and we don't know whether or not the White House made cuts. And so, there have – it's been a give and take, and that's what the budget process is, by and large. And one can argue that, and there's certainly people that argue that the Shuttle budget has been cut too deeply, and that there may have been negative impacts on the Shuttle Program because of those budget cuts.

It's very difficult, I think, to, perhaps, tie this into a specific situation like the *Columbia* tragedy and trying to say that, because of budget cuts, the *Columbia* tragedy happened. I know that you still remain flexible as to what the actual cause of the *Columbia* tragedy was. You haven't come out and made a statement but, obviously, a lot of people are thinking that it was foam hitting the Orbiter. And if NASA did not fully appreciate the dangers associated with foam hitting the Orbiter, it's not clear that an increased budget would have helped that situation.

So, everyone's, you know, looking to budget issues, trying to determine whether or not budget cuts were responsible, and it may well be that you'll conclude, as others have before you, that the Shuttle budget was cut too far, but it will be interesting to see whether or not you can tie that directly to this catastrophe.

ADMIRAL GEHMAN: One last question, then we'll let Allen get on stage here. But, if you look at '93, '94, '95, my – the big numbers were back in the mid-90s. If you look at '98, '99, 2000, the – either the cuts or the transfers are little numbers, 13 million, one million, 40 million. But, when you get up to the 400 millions and the 180 millions and the – things like that, 190 million, those are back in the '95, '96, '97. So, my – what I take away is that the really big transfers or cuts were in the late 90s and not so much recently.

MS. SMITH: Yes. They were back when the focus was on cutting the deficit, and all the federal agencies, including NASA, were suffering cutbacks in order to reduce the deficit.

ADMIRAL GEHMAN: And also, there were – this was the first couple of years of the SFOC contract.

MS. SMITH: No, it was '96.

ADMIRAL GEHMAN: That's what I said, '95, '96, '97, '98, and so there were perceived savings there. Whether they were real or not, we don't – we'll get to. Okay.

MS. SMITH: And NASA had metrics to show that the restraints on Shuttle funding were not affecting safety. They had charts showing that there were fewer in-flight anomalies despite the cutbacks in personnel.

ADMIRAL GEHMAN: Last question, Mr. Wallace.

MR. WALLACE: Well, just as sort of a process question, we've heard a lot in that – the history of the program about various compromises in the design of the Shuttle, that – sort of part of making the whole thing go, you know, military requirements or cross-range requirements or payload base size requirements, and things like that. And I'm curious.

So, we hear about compromises which may have resulted in designing a vehicle which was not optimized for the mission it ended up flying, or perhaps not even optimized for safety, and I'm wondering – I have sort of a two part question – are those compromises typically before the submission comes to Congress in the White House/OMB/NASA part of the process, or are they after the submission to Congress?

That's the first part of my question, and the second part is, does Congress have a process to sort of technically vet these things, experts they rely on to, sort of, understand the technical consequences of these decisions?

MS. SMITH: In terms of the original design of the Shuttle and decisions on how much money was going to be spent on the Shuttle, and how they finally got down to that $5.15 billion for research and development, that was all done before the President approved the program. So, that was what started the program, which then led to the annual budget request to fund it. So, those negotiations as to how big it was gonna be and whether it was gonna have – be fully reusable or partially reusable, or anything, those were discussions that happened prior to President Nixon's 1972 approval of the program.

And Congress does have a mechanism to vet any agency request. They have a hearing process where they will call upon a variety of witnesses. GAO often does studies for Congress and testifies about them. They can always come to CRS so – but we don't have the investigative powers that GAO has. And they rely on outside experts in industry, they – the Aerospace Safety Advisory Panel testifies to Congress, so they have a number of people that they can rely on in formal settings, and they also can discuss with people in informal settings if they're concerned.

ADMIRAL GEHMAN: Thank you very much, Ms. Smith, and Mr. Li?

MR. LI: Before I start my summary of my presentation, I wanted to address one of those issues, because I think it's important.

A few years ago, when I testified before the Senate, we were talking about the issue of upgrades, and this is an issue that I think permeates some of the discussion here. It's very important to know what your end state and goal is before you make whatever decisions you have to make. And the thing that, Mr. Wallace, I wanted to bring to your attention, because I know you're familiar with the commercial side of the aviation industry and not so much on the space side.

The analogy that I used that I thought was effective in conveying my feelings was I was talking about my 1986 Volvo, and I had to make a decision – it's like making a decision whether or not you want to replace the – what components, are there some things that are less expensive? Is the water pump worth fixing this year, or do I want to do a full-blown ring change for the engine? That – my decision is based upon whether or not I'm gonna keep it for one year or five years.

And the issue that I would like to raise to the Board is that I believe that, at the time that Ms. Smith was talking about those cuts and whatever, that was never well-focused in terms of how long are we going to keep the Shuttle. And I think that that uncertainty has a lot to do with, well, how much money should we spend? It's a lot easier to make an argument to OMB or to somebody else and say, "Look, I'm gonna keep this vehicle for X amount of time and, therefore, I need to make this investment."

When that changes from year to year – and luckily now, I think we have – or, at least before the tragedy – we had a good idea that it was going to be now 2020, but at least they put the line in the sand. They drew it. We knew what we had to do and, therefore, they came up with the – a sort of slight extension program. But, before that time, that particular line in the sand had not been drawn. So, I just wanted to raise that to your attention, that's a consideration that they should have.

Okay, let me move onto my prepared statement. Chairman Gehman and members of the Columbia Accident Investigation Board, thank you for asking GAO to highlight its work at NASA. We recognize the Board's daunting task of not only establishing the cause of the *Columbia* accident, but also in understanding the agency's environment in which management decisions are made. We believe our body of work at NASA can help the Board in this area.

In January of this year, we identified four challenges facing NASA, namely one; strengthening strategic human capital management; two, correcting weaknesses in contract management; three, controlling the costs of the international Space Station; and four, reducing launch costs. I will highlight these four challenges, and then provide an observation on the effectiveness of knowledge sharing at NASA, an issue I understand is of high interest to the Board.

The first challenge is for NASA to strengthen strategic human capital management. It may sound like a cliché, but an agency's most important asset is its people. NASA, like many federal agencies, faces ongoing difficulty in attracting and retaining a highly skilled workforce. This was no more evident than when we reviewed the Shuttle workforce.

In January of 2001, we reported that the Shuttle workforce had declined significantly to the point of reducing NASA's ability to safely support the Shuttle Program. Recognizing the need to revitalize the Shuttle Program's workforce, NASA discontinued downsizing plans and initiated efforts to hire new staff. As we reported in January of 2003, this problem has yet to be fully resolved. Staffing shortages in many key skill areas of the Shuttle Program, such as electrical engineering, remain a problem despite the new hires. New shortage areas have recently emerged, such as subsystems engineering and flight software engineering.

NASA believes that similar workforce problems affect the entire agency. Currently, the average age of NASA's workforce is over 45, and 15 percent of NASA's science and engineering employees are eligible to retire. Looking down the road, 25 percent will be eligible to retire in five years. At the same time, the agency is finding it difficult to hire people with science, engineering and information technology skills. Several bills have been introduced in this Congress to provide NASA with requested flexibilities for attracting, retaining and developing its skilled workforce.

NASA's second challenge is to correct weaknesses in contract management. Much of NASA's success depends on the success of its contractors. I'm sure you've heard that. These contractors receive more than 85 percent of NASA's funding in fiscal year '02. However, this reliance does not come without problems.

ADMIRAL GEHMAN: Excuse me for interrupting. Could we ask – I think if we want to ask a workforce question, this is probably – you're about to go onto contracting.

MR. LI: Yes.

ADMIRAL GEHMAN: In your statement, you said that, in your January 2001 report, that the report – and I've read all these – the report states that NASA's workforce has declined to the point of reducing NASA's ability to safely support the Shuttle Program.

MR. LI: Yes, and I wanted – I'm glad you mentioned that, and the issue and the point that we were making was not that it had declined to the point where it was unsafe to fly. It was within the context of what was happening in the near future, that increased flight rates were about to happen to support the Space Station. And what our concern was was that, if this trend of downsizing were to continue, and with the increase in the number of Shuttle flights that was to happen, then we saw some problems. But, you're absolutely right, Mr. Chairman. We were not saying that it was unsafe to fly.

ADMIRAL GEHMAN: Right, but it was declining?

MR. LI: That's correct.

ADMIRAL GEHMAN: In the January of 2003 report, you say that the challenges have not been mitigated.

MR. LI: Yes, and by that, I'm saying that all the new hires, in terms of having a critical skill that had, like, a backup, that that had not been fixed, that people are still very thin as far as expertise.

ADMIRAL GEHMAN: Right. Good. Thank you very much. Now – and we are also reviewing those things, and will come to our own conclusion on that, but we thank you for that.

One of the areas that we are focused on – and you listed a whole number of areas, training programs not attended because the people are working too hard, leave – not – annual leave not taken, the amount of overtime, advanced degrees not pursued because there's no time to give sabbaticals, and – I mean, all kind.

One of the areas that we have been looking at is the area of – and maybe I'm – this kind of balances toward your next section of contracting, but one of the areas that I'm concerned is the area of cases in which NASA no longer manages subsystems. In other words, the subsystem manager is a contractor. Did you – do you have any insights into that, and maybe – and I don't – it may be that – it may be not so much a workforce problem as it is a – the level at which the line between contractor and government oversight is maybe moving up and down. And can you –?

MR. LI: – I think that, you know, rather than just talking specifically about Shuttle, I think you can extrapolate that to the entire government. The issue is that there is always pressure to reduce the number of government employees associated with any program. That said, the terminology in NASA that is often used to describe the situation that you were describing, Admiral, is one of oversight and insight. And that really came – it was really clear when I was looking at the X-33 program.

In the X-33 program, it was one of those instances where they decided a very minimal government participation was going to occur. It was primarily one in which the contractor was developing the X-33 demonstrator. The feeling was that the government insight, which is working alongside with the contractor, was going to provide them as good information as doing oversight, which is checking things, that they had a certain milestone, certain product delivery that they had to provide, and then they would check that.

There is a lot of controversy associated with that insight and oversight. As a matter of fact, when we brought that issue up, I believe that when they restructured after X-33 and they did the strategic launch initiative, there was additional government oversight associated with a lot of those contents.

ADMIRAL GEHMAN: Yes, Dr. Osheroff?

DR. OSHEROFF: Well, I'd just like to bring up one specific example. The constant shedding of foam from the External Tank, whether it caused the accident or not, is irrelevant. It cost NASA a tremendous amount of money in maintenance costs for the Orbiters. And yet, it seemed like rather little was being done to understand the properties of the foam and why it shed. Now, the question is, whose responsibility was it to actually do this work?

MR. LI: I'm afraid I can't answer that particular – your question, sir. I believe that, had they known that that was a problem, I think that NASA would have stepped up to the plate and said we need to do something about this.

DR. OSHEROFF: I beg your pardon. I think that they knew that it was costing them a lot of money. That's not an issue. I think that was very clear. And yet, my statement still stands.

ADMIRAL GEHMAN: Let me rephrase the question, or let me add my question onto Dr. Osheroff's question. In the manpower review that you did, did you analyze, or did you consider the issue of whether or not, in a unique technical enterprise like human space flight, which nobody else does this, whether or not a rich and robust U.S. government technology workforce is necessary for a whole number of reasons, including career progression, including for the government to exercise property fiduciary contract management, and if – I could name seven or eight reasons why a robust, rich technology workforce should be paid for by the government.

One of the reasons is to be able to have the technical competence to answer Dr. Osheroff's question, but I can think of seven or eight other reasons. To kind of boil my question down to the issue of did you consider, or did you look at, whether or not it should be just a cost of doing business to fund a rich technological workforce as just a cost of doing business?

MR. LI: No, we did not, but I understand. I think it's a matter of philosophy. When I was talking to some of the engineers, and this is an important issue that, I think, in looking at the future, when I talked to engineers at NASA, they say, "Hey, I came to NASA to design aircraft, to design spaceships. I did not come to be a project manager/ contract overseer," and I think that gets to your point.

ADMIRAL GEHMAN: Well, I think that – what we're concerned about – and I don't want to speak for Dr. Osheroff, but we're concerned about – there are a number of reasons why the government should have government technologists and government expertise. There's other reasons – there's other places where the government doesn't need to do this, where if it's duplicated in industry or academia, fine, go hire them. But, we are wondering whether or not, if you're gonna send men into space and nobody else does this, whether or not the government should just have to bear this cost as part of doing business.

MR. LI: I think there's some merit to that philosophy. One side of me, in terms of being – having had the engineering background, tells me yes, in order for me to be able to oversee something, I need to understand that process, and I need to be able to understand whether or not somebody is doing something wrong. That is correct.

But, I also am reminded of a saying, and when I'm asked the question of my own staff at GAO, when people are saying, "Well, how can you go ahead and review these programs when you're not engineers?" And I'm reminded of a saying that says, "You don't have to be a chicken to smell a rotten egg."

DR. OSHEROFF: I think the full issue is very complicated. Let me make one more point if I can, and that is that the people that produce the External Tanks, that apply the foam, had rather little to gain by investing in research to solve a problem which NASA was not complaining about. USA was repairing the tiles repeatedly and, presumably, they had every reason to do that, but it wasn't in their venue. The one organization that would profit by solving this problem was the parent organization.

ADMIRAL GEHMAN: Dr. Ride?

DR. SALLY RIDE: I wanted to ask a question related to the workforce, as well. In your 2001 report, as you said, you identified constraints on the workforce, that you didn't identify as safety of flight problems at the time, but as something that needed attention, and rather desperately. And I was wondering if you could comment on how you related that to the flight rate, and to the work that was foreseen to be coming in the next few years, because I think that that – you know, the level of people related to the level of anticipated work, I suspect, was a major issue.

MR. LI: My recollection, Dr. Ride, was that, when the flight rate increases above the eight per year, that's when you – things are getting really dicey in terms of that workforce and how much they're going to be stretched. But, I believe it was in that general area between eight and 10, and there was talk at that point in time, as you perhaps recall, that at the peak of construction, they were planning to – almost a dozen flights were being planned out in the future. So, that was of concern, and I – to NASA's credit, they recognized that particular weakness and, as I said, they did stop their downsizing and start hiring again.

DR. RIDE: Did you look at that same issue in your 2003 – January 2003 report, in relation to the projected flight rate?

MR. LI: We updated – no, no, that part we did not. But, however, as you probably know, at that point in time, the decision had already been made to completely change the Space Station. When we did that original work in 2001, there was no talk about core complete, and things like that.

Now, we're in the situation where the Space Station is truly not an apples and oranges type of thing. The – as you know, the original Space Station was one where we were supposed to have seven crew, and now we're talking about something much smaller and, as a result, the number of flights would probably be more in the four per year, four to five per year.

DR. RIDE: I think it was said to be around six left to core complete.

ADMIRAL GEHMAN: Dr. Logsdon?

DR. JOHN LOGSDON: Allen, in your testimony, you say the agency is finding it difficult to hire people. Why? Have you done any reflection – I mean, is it not competitive with other federal high technology agencies, or is it not competitive with private sector opportunities?

MR. LI: I've had – I've had many conversations, actually, with the agency, and with the – NASA's chief Human Capital Officer, Vicky Novak, and they have some aggressive things that they are planning right now. The issue is one – and this is in their justification for the legislative relief that they're asking for, and has translated into those three bills that I mentioned. The issue is that, yes, there is difficulty throughout the country in terms of hiring science and engineering. The Aerospace Commission recently mentioned the same thing. So, NASA's not alone.

That said, it's incumbent, and the responsible thing for the Administrator of NASA, is to find ways in which his particular agency can weather this particular issue and, as a result, they have made those proposals. The types of flexibilities that they have asked for, Dr. Logsdon, include things like retention bonuses for the people that are there already, but in terms of recruiting, they are trying to go now at the – even the base level, at the – from the kindergarten on up, they're trying to enhance their participation in many programs such that there would be greater interest in NASA.

ADMIRAL GEHMAN: I think we better move on. I actually have a couple more questions, but let's move on. I'll save them for later.

MR. LI: Since 1990, we have identified NASA's contract management function as an area of high risk due to ineffective systems and processes for overseeing contract – contractor activities. I think that rings a bell there. Specifically, NASA has lacked accurate and reliable information on contract spending, and has placed little emphasis on end results, product performance and cost control.

While NASA has addressed several of its acquisition-related weaknesses, key tasks remain, including completing the design and implementation of its planned financial management system. As the agency implements this system, it will need to ensure that its systems and processes provide the right data to oversee its program and contractors. Data must allow comparisons of actual costs to estimates, and provide an early warning of cost overruns or other related difficulties.

NASA's third challenge is to control the costs of the International Space Station. We had a perfect example of that when Ms. Smith was talking about some of those changes. As the Board knows, the estimated cost of the Station has mushroomed, and expected completion has been pushed out several years. These weaknesses have affected the utility of the Station with substantial cutbacks in construction, the number of crewmembers and scientific research.

The grounding of the Shuttle fleet has a significant impact on the continued assembly and operation of the Station. The Station is not only the primary vehicle for transferring crew and equipment to and from the Station, but it is also used to periodically re-boost the Station into a higher orbit. While controlling costs and schedule and retaining proper workforce levels has been difficult in the past, the grounding of the Shuttle fleet will likely exacerbate those challenges. Because the return to flight date for the Shuttle fleet is unknown, and manifest changes are likely, the final cost and schedule impact on the Station is undefined at this time.

The fourth challenge is –.

ADMIRAL GEHMAN: – Let me – let's stop here a second. General Barry?

MAJOR GENERAL BARRY: I'd like to ask a question about contracts. Let me run this by you. The Board is looking at the issue of whether the Space Shuttle is an operational vehicle or a flight test vehicle, and we're debating that rather vigorously. When you look at contracts, right now, NASA, particularly for this Shuttle, when you look at the SFOC contracts and the other contracts for the other components of the Space Shuttle system, NASA uses primarily the cost plus award fee contract formula, with the incentive fees, performance fees and so forth.

Is it your opinion that that focus on that kind of a contract, rather than maybe fix the – or as the SPC used to be before the SFOC before 1996, would it be fair to say that maybe this award fee/performance fee focus does not encourage technical competence? Is there any issue there in your mind, insofar as what the motivations are insofar as a contractor? You said 85 percent of the budget goes –.

MR. LI: – Right, to contractors.

MAJOR GENERAL BARRY: For contractors. So, can you give us some impression – some of your views on whether this award fee/performance fee focus is the right one for a flight test vehicle as opposed to an operational vehicle?

MR. LI: Let me – let me answer this this way, and I preface this by saying that we have not done a thorough review of the SFOC contract, and so I am not as familiar with that contract as the Board is.

However, in – with regards to your question as to what type of contract would be applicable for a vehicle that was either not in full operational use and one that's in an experimental, I don't believe that the contract – there is that sort of relationship where I would change a contract in order to reflect what state the aircraft or spacecraft was. I don't believe that that is the salient point.

I also don't believe that the incentives, or some of the discussion that I've read in the media about what the Board has been asking questions about with regards to whether or not USA had the proper incentives, and perhaps disincentives, to, you know – with regards to trying to meet a schedule as opposed to ensuring safety. I don't believe – and I've had many, many interactions with the USA folks – and, regardless of whether or not they're contractors or government people, some of those people at one time used to be NASA people.

And I think that, while I understand that – and I heard some comment in one of your hearings where they said the heart is there, but that does not necessarily mean that safety was – could be ensured. I really believe that their heart was there. I have had many interactions with USA staff up and down, and I don't believe that they ever had any such malicious intent.

ADMIRAL GEHMAN: Okay, thank you very much. Let me – I'm sorry, but let me – you said that you had not looked in depth at the SFOC contract itself. Really, this section on contracting is really a section on financial management and –.

MR. LI: – It's on financial management, but it's – also reflects the work that – and the cost overruns problems that we have found. For example, I mentioned earlier that the Shuttle is being used to re-boost the Station. That was not the original intent. The original intent a few years ago was that they would have a propulsion module that was attached to the Space Station to do that. That propulsion module was canceled, and part of the problem was this fantastic cost overrun associated with the propulsion module.

They've had cost overruns on many, many other things, some of the things on the upgrades that were canceled. For example, the CLCS that the Board is well aware of had that problem. And the issue that we're raising here, Admiral Gehman, is they did – they do not have that financial management system that provides them the real-time, accurate information that they can project this is where I am and, therefore, the next few months we're gonna be in trouble.

ADMIRAL GEHMAN: Right.

MR. LI: But that was the issue.

DR. LOGSDON: Could I – quick follow-up?

ADMIRAL GEHMAN: Yes, absolutely.

DR. LOGSDON: You, GAO, had been looking at the almost billion-dollar investment that NASA's making in new financial management systems. What level of confidence have you developed about the success of this, since it's the third time – third try?

MR. LI: I think the issue – the – right now, the feeling is a mixed one. We just issued a report that was released just last week, and the issue there was that we do believe that the current core financial module, which is one of the components of the IFMP, is providing NASA, for the first time, with information that's consistent across all centers.

Up until this time, they've had separate accounting systems pretty much throughout all their centers, and that's the reason why they were never able to provide their top management with accurate information. They had to go through heroics in terms of manual spreadsheets in bringing that all together. So, from that standpoint, that's positive.

We, however, as we identified in our report, we have several concerns associated with how they are testing the core financial module. We're worried about – that some of the issues associated with providing the project managers and cost estimators with information, that that is not going to be provided just yet because they had not consulted with them early on in the program. So, we had – to answer you, Dr. Logsdon, it's mixed.

The fourth challenge is for NASA to reduce launch cost. In our earlier identification of cost to build the Station, we listed Shuttle launch cost as being a substantial cost component, almost $50 billion. NASA recognized the need to reduce such costs as it considered alternatives to the Shuttle. A key goal of the agency's earlier effort to develop a Shuttle replacement vehicle was to reduce launch costs from $10,000 per pound on the Shuttle to $1,000 per pound by using such a vehicle.

As we testified in June of 2001, NASA's X-33 program, an unsuccessful attempt to develop and demonstrate advanced technologies needed for future vehicles, ended when the agency chose not to fund continued development of the demonstrator. Subsequently, NASA initiated a five-year, $4.8 billion program to build a new generation of space vehicles to replace the Shuttle. In September of 2002, we reported that NASA was incurring a high level of risk in pursuing its plans to select potential designs for the new vehicle without first making other critical decisions such as identifying the overall direction of its integrated space transportation plan. NASA agreed with our findings.

In November of 2002, the Administration submitted to Congress an amendment to NASA's fiscal year 2003 budget request to implement a new plan. The new plan makes investments to extend the Shuttle's operational life for continued safe operation, and refocuses the earlier effort to develop an orbital space plane and conduct development of next-generation launch technology.

As I indicated at the onset, I will now comment on the effectiveness of knowledge sharing at NASA. In January of 2002, we reported on the results of a survey we conducted of NASA program and project managers. The survey revealed that lessons are not routinely identified, collected or shared. Respondents reported that they were unfamiliar with lessons generated by other centers or programs.

Many indicated that they were dissatisfied with NASA's lessons-learning processes and systems. Managers identified challenges or cultural barriers to the sharing of lessons learned, such as the lack of time to capture or submit lessons, and their perception of intolerance for mistakes. They further offered suggestions for areas of improvement, including implementing mentoring, storytelling and after-action reviews as additional mechanisms for lessons learning.

In closing, I will conclude with the following observation: to successfully implement its programs, NASA will need sustained commitment from its top management to focus attention on strategic planning, organizational alignment, the human capital strategy, performance management and other elements necessary for transformation success. The challenge ahead for NASA is to impart top management's commitment and vision to the rest of NASA by establishing the framework necessary for its successful implementation.

Chairman Gehman, this concludes my verbal statement. Thank you again for requesting my participation in today's public hearing. Be happy to answer any questions that you or other members may have at this time.

ADMIRAL GEHMAN: Thank you very much, Mr. Li.

MAJOR GENERAL BARRY: Just one real quick question. Do you think NASA as an agency is platform-centric, or does it have, in your view, a focus on a strategic plan and where it wants to go?

MR. LI: That is a very difficult question. I think that the strategic plan that the Administrator put forth establishes that the agency is no longer one in which it is platform-centric. It is the science, and there's no longer a destination-specific mission. It's one in which there are certain goals that have to be achieved. So, to answer you from that perspective, I believe that they are not platform-centric.

MAJOR GENERAL BARRY: Thank you.

MR. LI: To use a DOD term, kind of remembering that platform-centric versus network-centric.

ADMIRAL GEHMAN: Mr. Li, in your – you have a tremendous amount of experience in this area. I'm talking about reducing launch costs and launch reliability and things like that, which you've done at least two studies on this.

If you look at the Space Station, for example, you have reports in here, and I won't quote them, and I'm not gonna get hung up on statistics here. But, you submit a report on this ISS that it's behind schedule and over budget, and then a year later, you submit another report in which everything is doubled. I mean, just in one year, the costs double and you get another year behind, and then another year goes by and you submit another report, and, you know – and costs have gone up, and it's behind again.

Which – I'm not being – I mean, it may sound like I'm being critical, but I'm not really being critical. This is the nature of exploration and doing things that mankind has never done before. To me, we should expect that. Now, maybe we could do a better job of cost accounting and things like that, but I don't find that the process of going places and building things that mankind has never done before, I don't find that there's a lot of slop and error in there, and a lot of unexpected kinds of things in there, but that's my own view.

Okay. So now, we talk about space launch initiatives, and we set a set of requirements like reducing the – you know, you used the number that cost to launch a pound is $10,000. We actually calculate it to be way higher than that, but that depends on how you calculate it. And so, now we have – let's get it down to $1,000 and have a fully reusable vehicle that doesn't take six or 700 man-years in between each flight, etc., etc., and all that kind of stuff. And then, we spend $1 billion and we find out we can't do it.

And then – so then, we launch another initiative, and – do you find that – do you agree with me, or would you characterize in your own words whether or not we continually fall into the same trap of setting goals which are unachievable, underestimating their costs, and then not following through? And we seem to repeat this – we as a nation. I'm not talking about NASA here, because there are a lot of parties involved in this. We seem to repeat this pattern, and then punish ourselves when we realize that space exploration is so hard. And consequently, we find ourselves today without a replacement vehicle for the Shuttle.

Am I way off base here, or could you – could you relate that in your own terms, based on your experiences?

MR. LI: As I've faced General Barry, I have to be careful because, you know, I just recently testified on the FA22, and that is not a spacecraft, and that has also had many cost increases associated with it.

The issue is that, yes, technology is making it very difficult for some things to happen, and people do underestimate the complexity. Again, the – like, on the FA22, the software complexity of integrating all different avionics into this aircraft is causing tremendous problems, and you would think that by now our technology would be such that we can do that, but it doesn't happen that way.

On the issue of the reusable launch vehicles, and especially on the International Space Station, one thing that perhaps you have found in your analysis is that NASA has been trying to force-fit projects within their budget. And one of the criticisms of the International Space Station and why we've had these overruns and why does it – suddenly somebody says, "Hey, by the way, we have a $5 billion overrun." How does that happen?

Well, it happens because the focus is primarily on the budget year and trying to fit everything within that budget year. They are not looking at the cost to complete a project. If they had that particular perspective, and I believe that they are now, we perhaps would not be achieving those – and seeing those sorts of cost overruns.

ADMIRAL GEHMAN: Do you believe that this tendency, or this characteristic – we're not just talking about NASA here. We're talking – I mean, this is shared among several agencies and branches of the U.S. government. I mean, NASA has to work in a certain environment. NASA has several bosses, and they get this characteristic of focusing on the budget year with some help.

MR. LI: And you're right, and just not to – kind of tooting GAO's horn, but one of the issues, in terms of when we're talking about weapons systems development, and one way to control cost overruns is to make sure that you have mature technologies before you go to production.

Now, I understand that, from the standpoint of NASA, that is not a good similarity. But, the issue still is that, in the budget process and trying to get that particular political support for something, there is a tendency to try to establish a cost that everybody is going to be agreeable to supporting. And unfortunately, as more things are known and as technologies are found to not be as mature as they are, then cost overruns will happen. I don't have a solution to that.

I think, in terms of the X-33 that I spoke of, and I think you were implying with – talking about the $1 billion, that wasn't an instance where it was hoped that, even if there was increases, Admiral Gehman, that private industry would have been willing to pick up that slack because of that brass ring that was going to happen at the end, which is the Venture Star, which Lockheed Martin thought that they were going to build and capture all that for our – from a commercial perspective.

ADMIRAL GEHMAN: Except in that particular case, we have a built-in set of checks and balances that, once a commercial entity realizes that there is no brass ring out there, they back away. I mean, there's an automatic check and balance here, whereas in space exploration sponsored by the U.S. government, sometimes if you really want to get it done. You just have to keep – you just have to – you have to overpower the problems, and I don't know a better solution, but it does seem to be – it seems that the process has left us here with a Space Shuttle Program which is entering its third decade. People are talking about it having to enter a fourth decade, and we do not have a viable replacement vehicle because of a couple of false starts and things like that. We seem to be repeating this process.

Dr. Logsdon?

DR. LOGSDON: I have a question for Marcia Smith. If we could get her presentation back to chart number seven, I want to try to ask you, Marcia, to talk a little bit about what was going on in the program, see whether that works.

MS. SMITH: Could you bring up my slide number seven?

DR. LOGSDON: If you look at that and look at the current dollars, you see that, it's between fiscal '92 and fiscal '95 that the Shuttle budget went rather dramatically down, and it's been more or less level since then. What was going on in the program in those three years? How much of that is ASRM cancellation? How much is –?

MS. SMITH: – And budget deficit reduction, you know, the general reductions that were made at the agency's discretion, which are some of those larger numbers that you saw on the later slides.

But, during the early 1990s, there were still plans to build the ASRM, the orbital maneuvering vehicle was still being planned at that time, another project that was ultimately cancelled. And so, there were funds being spent on ways to upgrade the Shuttle, basically, and the decision was made not to proceed with those, and that, coupled with the struggle to reduce the federal budget deficit, brought the numbers down by the mid-1990s.

Also, at the time after they'd had the Chris Kraft report in 1995 that suggested going to something like SFOC, and then in '96 they went to SFOC to try and level out those Shuttle budget numbers so that it was not consuming such a large percentage of the NASA budget.

DR. LOGSDON: But this chart shows that, from SFOC on, the budget hasn't – there hasn't been big cost savings because of SFOC, or maybe there have been savings that have been offset by upgrade investments. I mean, you know, what –?

MS. SMITH: – Well, I think it's that, if you had not had SFOC, then the line would not have been able to stay stable. That's my understanding of it, that although, you know, it goes down to a number and it stayed pretty level, that if you hadn't had SFOC in there, it wouldn't have gotten down there and it might have kept going up. But, that – SFOC has saved significant money for NASA compared to where the program would be had there never been an SFOC.

ADMIRAL GEHMAN: Or cost avoidance systems. I mean, it's avoided having the program go up in cost.

MS. SMITH: Right.

ADMIRAL GEHMAN: All right. Dr. Ride?

DR. RIDE: Let me just make a point that's going back to, I think, a point that Mr. Li made right at the very beginning of your discussion, in fact, before your prepared remarks, in that is related to this discussion of the repeated tendencies to start an initiative to replace the Space Shuttle or to develop a new vehicle which then overruns in cost, turns out to be harder than everyone thought it was going to be, and is ultimately canceled.

One of the – one of the ramifications of that continued tendency has been that we're always ten years or less away from what we think is going to be the replacement to the Shuttle. As a result, we don't put a lot of investment into upgrading the Shuttle, and giving it the ability to last beyond those 10 years. So, we've been kind of trading off those investments, investing in new vehicles versus the upgrades to the Shuttle. Could you just comment on that?

MR. LI: What you're saying is absolutely true, and the starts and stops have affected it, and with the hope that the Shuttle would not have to go beyond the 2012 at one time frame. Whether or not these were, in hindsight, not the right things to do, the X-33 was a technologically complex program. It was – they had decided to do a single stage to orbit as opposed to a two stage. They were trade-offs. It was going to be less costly, and that's the other aspect, Dr. Ride, that we should remember is that, not only were they looking for something that was going to replace the Shuttle, but they wanted something that was going to reduce launch costs of significant magnitude, and that's a very difficult nut to crack.

ADMIRAL GEHMAN: Admiral Turcotte?

REAR ADMIRAL STEPHEN TURCOTTE: I might piggyback up on that a little bit, but let's talk a little bit about the effects of budget and indecision, I guess, on infrastructure.

Looking at a lot of the facilities that – specifically, the Cape comes to mind. A lot of indecision on where the program is going over the years has caused us a lot of delays, and there literally are a lot of facilities that are crumbling. Could you comment on that?

MR. LI: Yeah. When I was at Kennedy just a few months ago, I did notice that, and you're right. As far as the investment in those particular structures, many of those structures were built for the Apollo projects and, as a result, things are starting to fall apart. I was there when the crawler had that problem, which is the transport mechanism that takes the Shuttle to the pad. And for the want of a giant $10,000 shock absorber, that crawler was immobile there for a while.

And you're right. It's that sort of investment, but I am not prepared to criticize NASA management for not having made those investments, because I recognize the fact that they had a lot of other priorities. And just as I have to manage my home budget, I realize how difficult it is, and you make trade-offs. And I think the trade-offs were made, and when those particular problems were not one that was immediately on the screen, they did not make those.

But, in retrospect, they should have, and I'm hopeful, Admiral, that the current extension program and the monies that they're going to be putting in the Shuttle is going to also address that, because I understand they're going to put money in the infrastructure, as far as the Shuttle extension program.

MS. SMITH: If I could just add one data point that you might find interesting, I didn't put in a slide showing how much money had been spent on upgrades over the past few years. But, since upgrades have been separately identified in the budget, which began in 1995, NASA spent $4 billion on upgrades from '95 to '02. So, there was an investment being put into upgrades. It was starting to tail off there towards the end, I think a lot, because of the uncertainty as to how long it was they were going to be keeping the Shuttle around. But, they did invest 4 billion in upgrades during that period of time.

ADMIRAL GEHMAN: Mr. Li, no one's asked any questions on the last part of your report, which is the knowledge sharing kind of a section of the report. And I have not – I actually have not read that particular report, or that particular work by the GAO. But, in your statement, you make some comments about cultural resistance and the requirement for various centers and stovepipes and things like that to work better together on lessons learned and knowledge sharing, and things like that. You have some relatively strong words in there. Do you feel that this is a relatively significant issue that NASA needs to address?

MR. LI: Absolutely.

ADMIRAL GEHMAN: Stovepipes and –?

MR. LI: – Absolutely, and I think that, to Administrator O'Keefe's credit, he recognizes that his program of one NASA is one that addresses that particular issue. Some of the infrastructure sorts of things that they're doing, I mentioned the integrated financial management program is one that I believe is going to provide that sort of consistency. But, I – more important than that is this issue of, yes, the individual centers at one time were very competitive with one another and trying to bring them together and make them feel that this is a team effort is something that's very difficult. And lessons learning is one in which right now I think you're asking me do I think it's important.

Yes, I think it's important because people like myself are gonna be retiring pretty soon. We're gonna take away a lot of knowledge that our institution should have. Now is the time for NASA to be investing in that and ensuring that that knowledge is transferred to the younger people. And if I can say so, you know, one of the things that has concerned me through this whole process of – and especially in reading in the media about the bureaucracy that perhaps NASA's had, and the slowness of its decision-making, I want to – if there's anything that I wanted to convey to the young people of America is that – please don't look at this as additional vindication that government service and public service is not one that's important. If any time we need good, young people to come into the workforce, now is the time because, you know, I remember when I wanted to be in the federal service, after I got my degree in Aerospace Engineering. I remembered those words of our President. That said, we don't do things because they're easy – we do them because they're hard. And with what's happening with NASA right now, we need young people to come in. So, I think it's extremely important that people be able to disassociate the fact that yes, NASA has had problems, but this is an opportunity for them to make a difference with us.

ADMIRAL GEHMAN: I certainly – speaking for myself, I certainly agree with your statement, that almost everything NASA attempts to do is very hard. I know that for myself, until I began to understand a little bit more about this, I didn't realize how difficult it is to put an object in orbit in space. It was difficult when we first did it in the 60s, and it hasn't gotten any easier. I mean, we've still got a few laws of physics that are not going to change, no matter how hard we try to circumvent them. That's what we're trying to do here. So, this is still very, very hard, challenging work. And I agree with your comments that this is an exciting and worthwhile, national endeavor, that anybody should be excited to join.

My last question gets back to this personnel capital – this personnel business. And in your report, you mentioned NASA hiring initiatives and special pay initiatives, and special initiatives and things like that. Did you have an opportunity to look very deeply at a different mechanism? And that is, internal career development, promotions processes that – were you able to – and I understand the business about hiring and things like that and it's all a good idea. But, is hiring necessarily the fix to this? Or are there – did you look at the internal promotion and upward mobility kinds of aspects –.

MR. LI: Not in detail. But the thing that is important to understand, is that NASA's human capital strategy is not only for hiring. It's for hiring and retaining and being able to secure the expertise that they need. Some of the initiatives that they're proposing or that are being proposed in legislation have to do with securing people that have the expertise and being able – have more flexibility in being able to get them to come into NASA. And the retention bonuses are for people that have that expertise, but are on their – would like to retire. And we're trying to convince them that hey, please don't retire just yet, we need your expertise. There is one aspect that you're mentioning, that I think it's the mentoring issues that really have to come to fruition at NASA, that there has to be some innovative ways in which we can provide that transfer of information from the experienced people to those that are coming into the workforce.

ADMIRAL GEHMAN: Thank you very much.

MR. LI: Thank you.

ADMIRAL GEHMAN: One more.

MAJOR GENERAL BARRY: This is not unique to NASA, as you well know – that we are short on scientists and engineers and in the military and all that other stuff. Did you find any of the benchmarking being done to figure out any transfer of lessons learned between, like DOD and NASA or any other parts of the federal government?

MR. LI: No, we have not. But, however, as you're probably aware, GAO has identified human capital management as a high risk area for the entire government. And I know that at DOD, Secretary Rumsfeld has introduced and has been wanting to make specific changes associated with that. I think that is the trend. Everybody's recognizing now that with the impending loss of a lot of knowledge on the part of people, like my age, and they just need to do something and they need to be able to manage that workforce better than they have in the past.

ADMIRAL GEHMAN: I would like to thank you both, Ms. Smith, and Mr. Li, on the behalf of the Board, for your candor and your willingness to dialogue with us and work with us as we try to understand things that you're experts on and we're not.

We all have the same goal here, which is to return – safely return man to traveling in and out of space. And we want to do it as quickly as possible. And we are hoping that our work will help do that. And your willingness to help us along that line is greatly appreciated. We will take about a 10 minute break here to set the next panel. Please, not more than 10 minutes and we'll go back to work.

[Break]

ADMIRAL GEHMAN: Okay, alright, we're ready to resume. I'd like everybody in the hall here to take a seat and stop talking, so we can proceed, and with the second half of our public hearing. We might call this, moving from the way it ought to be, to the way it really is, or something like that. We are though, very happy and pleased to have two very, very experienced managers and directors that really know how things run.

Russ Turner. Russ Turner, until just May, was the CEO of the United Space Alliance and he was – he had a very, very long tour as the CEO of the United Space Alliance. Probably longer than he planned to when he got there. We'll let him tell that story himself. But Russ is a business man and has a long and rich history in the Space Shuttle business, going back, I believe, to Rocketdyne and has been in this business a long time.

Tom Young, is a former NASA Center Director, former space industry CEO and he serves, it seems like, a professional task force, a professional board, a professional advisor, on all matters of space and shuttle programs, not only to NASA, but to the US government at large. As is the process of the Board, before we begin, let me ask both of you gentlemen to affirm the information you provide the Board today, will be accurate and complete, to the best of your current knowledge and belief.

PANELISTS: I do so.

ADMIRAL GEHMAN: Thank you very much.

I'd like, starting with Mr. Turner, if you would introduce yourself and say something to either amend my remarks or say anything you want about your background and expertise. And then we'll ask Mr. Young to do the same thing. And then I'll ask you to make some introductory comments.

MR. RUSSELL TURNER: Yes, I'm Russ Turner.

For the last five years, until recently, I was the Chief Executive Officer and President of United Space Alliance. I was also involved at the very beginning of the formation of the United Space Alliance. I was on assignment for about six months, with Kent Black, who was the originating CEO. So, I have a perspective, both from the start, and where it is today.

ADMIRAL GEHMAN: Thank you very much.

Tom.

MR. THOMAS YOUNG: My name is Tom Young. I'm an engineer.

And as the Chairman mentioned, the first 20 years of my professional life I worked for NASA, concluding with being Director of Goddard Space Flight Center, almost totally in the automated side of the NASA activity. The next 13 years for Martin Marietta, where I was President and Chief Operating Officer, retired in '95, currently on several boards of directors, and as mentioned, involved in various advisory activities. I might mention just two or three of those. I did chair an independent review of the Mars program, after the Mars '98 failures. I did chair the

International Space Station Management and Cost Task Force. And I'm currently chairing a review of the DOD Space program, looking at cost and schedule related issues. Thank you.

ADMIRAL GEHMAN: Thank you. Thank you very much.

And I believe, Mr. Turner, are you prepared to go first?

MR. TURNER: Yes, sir, I am. If we could put my first chart up, please. I'm here to give the contractor's perspective on the Space Flight Operation Contract or SFOC, as we call it.

And I'm going to just spend a chart on the origins, to give you sort of a common basis for understanding the SFOC. I'm going to talk about what was different about SFOC and what had gone before, talk about how that affected performance and what the performance has been under SFOC. Talk about the things that SFOC achieved that ought to be retained, and then draw some conclusions. Next chart.

As you know, the SFOC was created by NASA in 1996, and it was viewed to being the next logical step in improving Shuttle contractor performance. NASA had done internal reviews and they had brought Chris Kraft and a team of folks in, to provide an independent external review of where Shuttle was and what was possible for the next step. And the consensus was, the existing approach, which I think you saw in Marcia Smith's testimony, was a series of budget reductions over a period of time. I think if you look over the period about 1992 to 2002, it was more than a 40 percent decrease in the number of contractor employees and the number of government employees supporting the Shuttle Program and mostly driven through budget reductions. And so, the consensus was that if we're going to continue to reduce costs and do so safely, we needed a different approach, and the SFOC created a prime contractor for Shuttle, where there had been to that point, lots of separate contracts. Not only separate contractors, but even within a contractor, multiple contracts, creating a very complex situation that I'll come back and talk about a little bit more.

So, the SFOC created this prime contractor. I have noted, in the media there's been some confusion over the fact that SFOC was not an outsourcing initiative. The contractors were already performing the vast majority of all this work. We'll talk about the accountability shift that took place. But this was not taking folks who had been performing work as government employees and shifting them into being contractor employees. This was simply reorganizing how the contractors approached their work. The initial work scope of SFOC was 100 percent existing –.

ADMIRAL GEHMAN: – Mr. Turner, if it's okay with you, we should ask questions as we go along because – so you don't have to jump back and forth.

And I – my understanding of that last comment that you made, that the SFOC contract was a collection of independent contracts and subcontracts, and was not a privatization or an outsourcing – I mean, that is my understanding. Nevertheless, during the same time frame, there was a significant reduction in government employees at NASA.

MR. TURNER: Well, as I indicated actually, that reduction in employees started back around 1992. The total reduction – I have it in front of me – was 56 percent. And if you look at the curve and you look at the beginning of SFOC, I think you conclude there really wasn't much of a change, in terms of the rate of decline in government employees that could be attributable to SFOC. There were some shifting accountabilities that enabled NASA to move some folks around – absolutely.

ADMIRAL GEHMAN: That's correct. Now, I think we're saying the same thing. I am not attributing any of the government personnel cuts to SFOC. However, at the time this was going on, there was a steady – it started before this and it continued after this – a reduction – a pretty – as you say, over a number of years, 50 percent reduction in the number of government employees.

MR. TURNER: Agreed. And, by the way, the same reduction was going on, on the contractor side – almost the same percentage. And if you look at the slope of this chart, it actually accelerates a bit around the time of SFOC, and that was because of SFOC and I'll talk about that a little bit.

ADMIRAL GEHMAN: And also at this time, there was some pushing and pulling of responsibilities and functions, between the government and the contractors. And I don't know if you're going to get into that or not.

MR. TURNER: Yes, we will. We will talk about that.

ADMIRAL GEHMAN: Thank you.

MR. TURNER: Although, I prefer to think of it as a planned transition, as opposed to pushing and pulling, but we'll talk about that.

ADMIRAL GEHMAN: I'll use your terms.

MR. TURNER: The initial scope of the SFOC was 100 percent existing Rockwell and Lockheed Martin contracts. There were subcontractors to those contracts, but on day one, that was the work-scope that was included. The United Space Alliance was created by Rockwell and now that, of course, is owned by Boeing, and Lockheed Martin, specifically to compete for the SFOC. When we understood the government's intent, we looked at what the best way to respond to that was, and concluded that a joint company that had the best skills from both, would best serve the government.

And NASA ended up sole sourcing then the SFOC to USA, after evaluating industry capability statements. They held a suppliers conference, where they talked about what SFOC was going to be and asked for 25-page capability statements to be submitted by interested parties. We submitted such a statement and ultimately, were awarded the contract, sole source. Next chart please.

ADMIRAL GEHMAN: So your understanding is, that this was a sole source of award?

MR. TURNER: Yes.

ADMIRAL GEHMAN: And if this is not an appropriate question, you just tell me if it's not appropriate. But, can you tell me what the contractor investment in the new entity was, dollars?

MR. TURNER: I can't tell you dollars off the top of my head. That number's available. We can provide it. Each of the companies, Rockwell and Lockheed Martin, contributed a certain amount of capital. And then they contributed a significant amount of human capital in their key engineering and manufacturing organizations. And so, we can provide you with that total investment amount.

ADMIRAL GEHMAN: I have read in some sources that the investment capital provided by each of the two entities was a nominal amount, I mean, like a million bucks or something like that.

MR. TURNER: No.

ADMIRAL GEHMAN: Okay.

MR. TURNER: It would be substantially more than that. Because, they each did have, when they put the companies together, existing capital that went into the companies. And so, machinery, computers, facilities, anything that belonged to the two companies that were associated with operating the system, went in. I think the million dollar reference –.

ADMIRAL GEHMAN: – But that wasn't new.

MR. TURNER: No, but it was property they owned that they gave up to USA, that otherwise they could have retained for their shareholders. I think the million dollar reference is talking about money they put up, day one, to just be able to form a company –.

ADMIRAL GEHMAN: – Probably right.

MR. TURNER: Right. But in order to do SFOC, they had to put all that other capital in first. As you know, the SFOC emphasized contractor performance accountability and it was a shift in approach and a gradual shift in actual accountability. NASA's accountable for establishing the goals and objectives as it has always been. And really very important here is that NASA continued on the requirements. There was no shift of requirements ownership. In the Shuttle Program, requirements determines how you process the vehicle and how you fly the vehicle, and they retained authority of that and have it still today.

The contractor's mostly accountable for what and how you achieve those goals and objectives, with those requirements. Certainly accountable for the technical performance, for the scope of its contract. Certainly responsible for its own systems and processes, except where they're controlled. So, for example, a non-conformant system which would be used to track the hardware performance and any discrepancies with it, that would be a controlled information system. If there were going to be any changes to that, that would end up having to be approved by NASA. But, if we wanted to make a change to an internal risk management tracking system or a human resource system, those are within our purview to make changes as appropriate. And of course, the contractor was accountable for – is accountable for total cost. And I'll come back to what the implication of that total cost accountability is.

DR. LOGSDON: Russ, just to make this clear in my own mind. What function did NASA perform before SFOC, like safety and mission assurance, that they either have many fewer people or no people performing after SFOC, where the function and responsibility was transferred to the company?

MR. TURNER: Well, actually, you just asked two different questions. In instances where there was a person doing a task at NASA and USA picked it up, and therefore NASA no longer had any function to perform for that person, related to Shuttle and reassigned them, I suspect, John, it's a relatively low number. And I'll get that number for you. What happened most often, was a very conscious shifting of accountability. So, Admiral, I believe you brought up the issue of subsystem managers. The subsystem manager before SFOC was a NASA person. That NASA person would have the accountable contractors, who in many cases probably had most of the technical expertise, and they would meet in boards and panels, and the NASA person would chair the boards and panels, because they're a subsystem manager. After SFOC, the subsystem manager role transitioned to the contractor, where the technical expertise was, but the NASA folks would still participate in the panels and boards, and therefore, there wasn't a shifting of a – while there's a shifting of accountability, there wasn't a shifting of a job. It was a change in how the process was done.

DR. LOGSDON: But USA would chair this board?

MR. TURNER: Absolutely. USA would take over chairing those boards. And I have a list, which we can also provide you, which gives you top view of where the shifts took in accountabilities on various boards and panels. So, as I'll get to on a chart that's coming, this is a significant change in accountability.

Prior to this contract, these were more like level of effort activities, where the contractors provided all the actual hands and feet and technical expertise, but there would be a lot of day-to-day direction from the government. And the transition to saying, you're providing the technical expertise, you're accountable for your performance. We're still going to participate with you. You still need our approval for any changes. But we're going to give you more end-to-end accountability. That was the change.

ADMIRAL GEHMAN: One of the – this is not a quiz and so I'll ask you a specific question. But, from my reading on this subject, one of those subsystems that shifted from government responsibility or government oversight, to contract oversight, was the Thermal Protection System.

MR. TURNER: Absolutely.

ADMIRAL GEHMAN: And now we have a contractor who is the subsystem manager –.

MR. TURNER: – Correct.

ADMIRAL GEHMAN: – Of a Thermal Protection System.

MR. TURNER: We might want to expand then, how the process works. For the various elements of the Shuttle system, NASA provides the TMR, Technical Management Representative. And the TMR has ultimate authority over that system. And that accountability did not shift to the contractor. So, the way the process would work – and we use TPS as an example, is if there's an issue relative to TPS, the technical teams would do the detailed work and the chairperson of that activity, the accountable person for making sure that the right people are on that team and they're answering the right question and they're doing a good technical job, is the subsystem manager. That team result is then taken up through NASA to the TMR, to the TMR's board for reviewing whether or not that, indeed, is adequate.

Now, that's what it looks like on paper. In reality, this is a day to day communication activity that goes on. And so, the TMR would have been very connected to what, in fact, probably was the origin of the request. And then, if there was an issue that needed to go further, then the TMR would be able to take it forward to, for example, the PRCB, which would be the Shuttle Program manager, NASA position, that ultimately would approve any changes of a certain level.

ADMIRAL GEHMAN: Thank you.

MR. TURNER: So, a change that takes place is that NASA now is in the position of evaluating contractor performance, relative to this change in accountability. Dr. Li talked about this phrase, oversight versus insight, and the discussion we had around accountability just now probably helps illustrate what that was. NASA was responsible for watching USA, how we performed, what our processes were, the robustness of our processes, and the quality of our products. But the accountability for actual execution of all that resided with USA. And that was a shift.

There was and is in SFOC, an increase in objective performance – measurement criteria. I submitted to the Board, a set of the 200-plus metrics that NASA tracked as part of their insight activity, to validate our performance. And then the contract shifted, from what in most cases had been cost plus award fee, to a much more sophisticated cost plus award fee, performance fee, and cost incentive.

And associated with that, were safety gates, to insure the proper focus. And I heard this come up earlier. The way this was structured, NASA did a traditional NASA award fee evaluation of USA, how are we doing against a set of criteria, and that would have been rated by each of the TMRs and then summarized up to the Shuttle Program Manager. A separate rating would be given to USA for its safety performance. And that was led by the Quality TMR and how well we're responding to NASA's safety priorities. A separate evaluation was done on how well we achieved the performance criteria for properly processing the vehicle and properly launching it and returning it.

If the safety score were good or lower, we would lose the opportunity to earn in any of the cost incentive during that same period. So, the safety performance, had as a minimum threshold, very good. So it had to be in the very good to excellent range, or there was no cost incentive.

MR. WALLACE: May I ask you a question, Mr. Turner?

MR. TURNER: Sure.

MR. WALLACE: Has that happened in the history of your contract?

MR. TURNER: No.

MR. WALLACE: So, you've never fallen below that threshold?

MR. TURNER: We've never fallen below very good, and we're at the excellent level – USA is at the excellent level currently.

MR. WALLACE: This presents an issue that we wrestle with a lot within NASA, and also in terms of these contract award criteria. So, philosophically, how do you write a contract where since there really is a high objective, maybe a top objective, without ever creating an incentive to under-report safety problems? I mean, we see, occasionally, proudly displayed reductions in, let's say, in-flight anomalies, in some phase the program, yet we also see, in parts we look at really closely, certain discrepancies, which seem to have been IFAs before, that later on aren't IFAs. What are your thoughts on that?

MR. TURNER: First, the process you just described is a NASA process. So, that isn't an opportunity for the contractor to define what is and isn't an IFA. So, I wanted to put that aside, because you're asking a more general question, which is around, does the government have enough insight into our metrics to know that the performance we're reporting is the accurate performance. They do. And from a contractor's perspective, and I'll come back to this, it is very clear that all of the contingencies are around safety performance.

Yet there are scheduled contingencies, there are issues around making sure that we form well the budget, but there isn't a business, if you don't have the ultimate level of safety. And in fact, I'll go to a side now – one I was going to make later. When you evaluate this and make recommendations, I encourage you to be empiricist about it. That is, in addition to the philosophy issues you've raised, you need to spend some time on the facts and ask yourself, how did the contractor behave under this arrangement? And what you're going to find out is that USA spent a lot of money on safety activities that they did not have to spend, by the nature of the scope of the contract, and which meant that they did not get cost incentives, that they could have gotten, by not spending the money. And I'll give you an example.

We initiated a bonus for the employees – an annual bonus, based on the company's safety performance. Every employee in the company could get a check at the end of the year for $750, if we met all the objective performance goals around safety. And the employees did very well against that. I don't know if they maxed-out every year, but they were –.

MR. WALLACE: But was a lot of that occupational safety, missed work –?

MR. TURNER: – It's a whole bunch of things; occupational safety, in-flight anomalies, processing escapes, damage to hardware, it's the full range of things related to safety. And we negotiated that set of categories with NASA in advance, so that they could agree that that constituted a good measure of how the organization was performing. So, 10,000, $750 an employee, that's about $7.5 million a year, in money that USA committed to spend, that it didn't need to, in order to respond to the contract. That's one example.

Second example, a big investment in a new safety system, risk associated trouble spots, which was put in place to get employees to identify more issues, and rewards employees for identifying more issues. And it's a closed loop system, so once identified, a manager is assigned to it and the manager has to close it out. Now that cost money to put in place, not only to administer it, but then you have to respond to all the issues. Not required to be done under the contract, but done because safety is the primary focus of the contract. Trained every manager in the company a several day training program on how to lead for safety. Integrated – implemented VPP "Star" status at every facility. Implemented the new quality system. So you go through this list of things, and what you'll learn as an empiricist is, this list of things didn't have to be done. The total amount of money USA spend on activities that it was not required to do under the contract, over this first six years, is about $190 million. If instead of spending that money, they had just counted that as savings, they would have gotten 35 percent of that.

So, the nature of this safety gates and the nature of the business we're in and the nature of the culture we're in, had a result. And I encourage you to look at the result in terms of the systems and the money that USA spent, and the result is what NASA was trying to achieve. Which was a balance between, first and foremost, a focus on safety, and then given that, how do you reduce cost and how do you make sure you meet the manifest and meet the mission objectives. And I'll show you some performance figures later on.

I also will later tell you, from a contractor's perspective, it doesn't matter whether there's a cost incentive or not, it's based on what the government wants to achieve. So, I'm not lobbying you to keep a cost incentive in, I'm just encouraging you, before you draw conclusions about its effect, to make sure you look at what the effect actually was in terms of contractor behavior. Next chart.

I didn't mention on the safety gates, in addition to having to have at least a level of very good in the safety score, you had to have at least a level of good in the overall award fee score. So there are actually two safety gates, and you had to hit both of them in order for the cost incentive to be available.

ADMIRAL GEHMAN: While we're on that subject, this is not exactly on the subject, but what does the contract say about loss of vehicle and loss of crew?

MR. TURNER: Loss of vehicle, loss of crew, we lose all the cost incentive, the performance incentive, a bunch of other stuff. In the instance of the *Columbia* tragedy, I think it ends up being a $70 million impact, to USA. And some of that – a bunch of that, is money that we actually have to pay back to the government – money that we had earned in the prior periods, and based on the – if the loss of the vehicle were a result of a USA action or a failure to act by USA, we would need to refund that money.

MR. WALLACE: You sort of anticipated my question. If this is an element you would lose certain awards, and then as far as there being sort of a penalty would require some kind of finding by who, I don't know –.

MR. TURNER: – Well, the way it's worked, it's actually very straight forward in that the contracting officer for the SFOC contract makes these calls. Now, how NASA behind the scenes works, I wouldn't have visibility. But the contracting officer would send us a letter saying we've made the determination that USA's accountable for the following, and then the terms and conditions of the contract dictate not only loss of the opportunity to earn, but also refunding money earned in prior periods.

ADMIRAL GEHMAN: Yes, sir, go ahead.

DR. OSHEROFF: Well, I just – let me ask a question, slightly different, that I asked in the last round. Which is, issues that were not necessarily safety issues, but maintenance issues, such as the impacts of foam shedded from the External Tank, whose responsibility was it for dealing with those? And I mean, I guess I really want to know why there was no concerted effort put forward to understand the problem and to eliminate it?

MR. TURNER: That's very clearly, a NASA Space Shuttle Program call. USA was getting the brunt of that. And you commented earlier that it cost a lot of money. It cost USA that money. We didn't get any relief as a result of having to make those repairs and I'm on a cost incentivized contract. So, the additional work necessary to repair that tile was coming out of my funding. And so I was motivated to find a way to have that foam not come off anymore, because it was creating a turnaround issue for us. But, the SFOC is only a limited part of the total Shuttle Program. So, the External Tank, the Space Shuttle Main Engines, and the Solid Rocket Motors are not in the scope of SFOC. If the tank had been in the scope of SFOC, the question you just asked me would be an appropriate question for me, why didn't you get after it. And we would have been motivated to do that, but the External Tank isn't part of the Space Flight Operations Contract.

DR. OSHEROFF: But still, if you felt – how much was it costing, by the way? Do you know, roughly speaking?

MR. TURNER: Not off the top of my head. It, you know, every one of those tiles has to be inspected. There are rigid standards for how you repair or replace it. And clearly, like during the period when we were getting the popcorning, there were more tile damage and those tiles had to be replaced. We certainly – with feedback through the system, what those costs were and expressed the desire to see where we reduce the damage to the tile, but that's the extent of our ability to influence the outcome.

DR. OSHEROFF: But, if you had thought that you could save money by actually undertaking a research program to understand the problem, you would have done so?

MR. TURNER: Well, not with the External Tank being – there is no mechanism by which I could undertake a research program on the tank program. And that's what you'd have to do. The research would be on how would you change the foam so it performs differently, and since I'm not the contractor for the External Tank, I don't have a mechanism to insert myself and say, hey guys, I'm gonna do a research project, give me your experts, let me have access to your hardware, and I'll let you know that outcome.

DR. OSHEROFF: Who is – who does have the contract for the External Tank?

MR. TURNER: It's a Marshall contract, so it would be through that Marshall chain of command.

DR. OSHEROFF: What is the subcontractor?

MR. TURNER: The contractor is Lockheed Martin on the external –.

DR. OSHEROFF: – Okay. Lockheed Martin certainly has a part – I mean, this is very funny. I mean, obviously, you are – USA is partly owned by Lockheed Martin, correct?

MR. TURNER: USA, yes, USA is a limited liability company. And we have two shareholders, 50-50, Lockheed Martin and Boeing. But the nature of a limited liability company is there is a very, very limited governance relationship. And for all intensive purposes, the Lockheed Martin External Tank people are the same to us as Thiokol or any other non-USA company. There isn't any relationship. I can talk to people, but I can talk to people at Thiokol as well. The Shuttle community, the contractors, talk to folks. And so I can tell them that it'd sure be better if the tank performed differently, but I'm not in a position to direct them to do anything.

ADMIRAL GEHMAN: Even though you can't quote a dollar amount, would you give us a subjective evaluation of whether or not TPS repair was kind of the driving factor in turnarounds or frequently was the driving factor?

MR. TURNER: For a long time, TPS was the – in terms of cycles, was the long pole. Now, that's, as you know, TPS damage comes from a lot of sources, not just foam. In fact, in prior years, it wasn't foam from the tank at all. The most damage, I think, in the early years, was from the Solid Rocket Boosters. You get damage on liftoff from stuff around the pad. You get a lot of damage when you land, because of stuff that gets kicked up when the vehicle touches down. And so, the impact of all of those things together, had for some time – and I think in recent years it was less of a long pole, but it was still a significant element. And partly because of the process to repair and replace tile. Anything else on that?

ADMIRAL GEHMAN: Go ahead, General Barry.

MAJOR GENERAL BARRY: To go back to the trend of keeping on one slide here, but the bottom line is on this cost plus award fee performance fee cost incentive. Let me ask you a couple of quick questions. Is the Shuttle an operational vehicle?

MR. TURNER: I heard you talking about that earlier. I think you need to – I think we need to do some definition of terms. There is – I think what folks mean by operational – there's the way you fly a 737 airplane. That's what an operational vehicle is. That's one end of a spectrum, I think. The Shuttle is not like operating a 737. It's much more demanding that – and Admiral Gehman referred to how hard it is to go to space. There's some real physics limits to what we're doing. So it's not like a 737. It is however, operational in the sense that the purpose of the vehicle is not testing. It's not a test vehicle. The purpose of the vehicle is to perform a mission that's independent of the vehicle itself. And most recently, of course, deploying Space Station. So, it has an operational role. And there really is very little that is going on in a planned way, around collecting more data and doing experiments about how does the vehicle perform. The vehicle performs in an envelope that's well-defined and understood, in order to perform those operational missions.

Having said that, it is not a 737, and there are two kinds of issues here. One kind of issue is finding out things that don't perform as well as expected, because of design limitations, we'll say. And that's like, I had a relatively new car and the water pump went out on it recently. And that's not – there's no new science in that. Water pumps go out. Turns out the design, probably has the Serpentine belt putting too much side load on the water pump and the water pump failed. We have failures like that in the Shuttle, for example, the wiring problems that we had.

What I think brings people to talk about being a test vehicle is there's a second category of issues, which we have in the Shuttle, which I call new science. And an example of that, I think, is the cracks in the flow liners. Nobody else operates a reusable cryogenic system. And in operating it, we discovered that there's some kind of back pressure phenomenon from the SSMEs that creates some kind of environment with those ultra-cold temperatures that causes these micro-cracks. That's new information about how such a thing operates. You're not getting that with 737s. That happened earlier, you know, in the development of the jet aircraft and you learned a whole bunch of that stuff. But because Shuttle is at the cutting edge and nobody else is doing it, we are still getting those kinds of new findings that affect our ability to operate.

MAJOR GENERAL BARRY: I know I'm jumping ahead a little bit on your slides, but with that understanding, is the incentive part of the SFOC conducive with this kind of an operation, in your opinion?

MR. TURNER: Yeah, I'll go back to my empiricism, I think yes. Because if you look at the behavior of the contractor, we're focused on the things you want us to be focused on. We're spending money where you want the focus on. I think it's more appropriate to ask, was the program properly funded. As Marcia was discussing, given that it's that kind of vehicle. Should you be putting more into studying those new things that you're finding out, out of the vehicle? Should you invest more in the vehicle? Should you have a better I,V, and D planned improvement program for the vehicle, because you know it's not as mature as the 737 and therefore, you plan that you're going to be operating it that way.

MAJOR GENERAL BARRY: Final question. If you were going to redesign this contract again, would you have it lean more towards the fixed fee elements or the incentive side?

MR. TURNER: Will you let me wait till my last chart, where I make recommendations? Because I do want to talk about that, but I have a little ground work I'd like to lay before I get to it.

This is the dreaded 15 minute chart that we all think about when we do briefings. It's been up there a long time. The SFOC – we're talking about what changed with SFOC. The SFOC contract made a significant simplification in NASA interfaces. The scope of work we're talking about were nine separate contractors, I think, 28 separate contracts, all with interfaces in technical business contracts – fairly complicated. Under the SFOC, we had a single contract, a single contract manager. The first thing that enabled us to do, which was probably the biggest single element of savings on the program, was to eliminate duplicate business organizations, duplicate information technology organizations, duplicate human resources organizations, and eliminate tiering of corporate flow-downs and corporate fee. So, if you go back and look at the numbers, there was a huge savings as soon as the contract was signed, out of just eliminating what was completely non-value added redundancy, that was a result of the prior contracting mechanisms. This is not reducing a single engineer on the program. This is simply getting rid of the support structure that was unnecessarily burdensome. Second thing we got out of the simplification was more unified technical requirements flow down. And I view that as an important safety and quality issue. When you had complicated flow-downs that were different by contract, inside the same contract or Rockwell, you would have a different way and philosophy around technical requirements, then the contractor was forced to figure out how to integrate those.

And the nature of this contract is a single employee in the old days, would be charging to two or three different contracts. And therefore, as they shifted work, would be shifting where their technical flow-down came from. And under the SFOC, we had a single flow-down for the requirements.

Now, the sub-bullet there that however, under the SFOC, the NASA Center differences were still in place. An example of that that I use I think that makes it very clear, is in Florida, there's a Marshall facility. There used to be a fence around it. And that's the solid rocket booster processing facility. If a USA employee is inside that facility, now they're responding to a technical requirements flow-down to come from Marshall, with differences in the quality system. And a real concrete example I use, is how often do you certify the calibration of a torque wrench? And let's say, at that facility, it's 30 days. That same employee, working over on the KSC side of the facility, with that same piece of hardware, but now as part of the integrated Shuttle staff, will be under the KSC technical requirements flow-down, and that same type of torque wrench might have a 90-day re-calibration certification requirement.

So there are two issues here, one is that ends up being complicated for the workforce. And the other is one of those two numbers is probably wrong. It's probably either 30 or it's 90. If it's 30, then we're not doing it as well as we could. If it's 90, then we're spending a lot of time doing calibration that isn't adding any value. So, that didn't change under the SFOC.

And we still have the TMR, the Technical Management Representative structure aligned to the historical NASA structure, which I think is best characterized as around how the vehicle was originally developed. And so, you'd have a TMR for Orbiter, a TMR for the External Tank, a TMR –. And, and so, that also didn't simplify it as much as you might have. But still, much simpler structure that really helped us to address both cost and technical issues.

The contract had a broadly written scope – and still does – to avoid continual change traffic. I've come back to that as one of the benefits that enable the contractor to do what was right, without having to get into a contracts loop that drove a lot of time. And then the Center accountability change in the contract to be primarily base support, not program performance. The exception, until recently, being JSC, which was designated as the lead Center for Human Space Flight. So, JSC did have a very strong program performance accountability. But that's now gone. And the Centers now have this support role that the program is aligned within USA, to the Shuttle Program. Next chart.

Now, a number of those changes enabled significant improvement. Let me talk, how do you measure improvement? Everybody's improving, so I can talk about improvement in a number of different ways. The SFOC enabled safety, quality, and cost performance improvements. And I'll talk more about why, but first, what improved and what can we compare it to. There is a very clear absolute improvement in performance over historical levels. So that's very straight forward to look at. I'll show you some numbers and you, I think, have access to more detailed data. It also improved, compared to a contemporaneous heritage company performance. There are still elements of Rockwell now boiling in Lockheed Martin.

And if you compare the USA's performance to what they've done over the same time period, and you look at these various metrics, you see that USA's performance is improved over that.

Where we could, we've done formal benchmarking. And this is very limited. Because it's hard to find an operational system – you can look at the Concorde, which we've looked at. We've benchmarked with Delta and some other airlines folks, to look for points where you can – how do we compare to the best in industry? Places it's easiest to compare are in places like information technology, procurement, things that all companies do. We do those comparisons and USA's performance is in the top quartile, compared to the best comparable companies in the country.

We also compare ourselves to Thiokol and other non-SFOC NASA Shuttle contractors. And in doing those comparisons, our safety and quality at USA is as good or better. And we have a lower cost structure. We didn't talk about this earlier, but one of the advantages to the government and USA is of a single purpose company, so it doesn't have the flow-downs of a larger corporate entity, it has a very low G and A rate, it's focused on the single purpose and it's not a design and development organization, so it doesn't have the IR and D budgets, it doesn't have the Science Center costs, and so it ends up being a very low cost provider.

Now, how do we get those improvements? The focused contractor accountability allowed us to go after optimizing the system, instead of the subsets of the system, and putting in integrated information systems, common training approaches, common certifications, those kinds of things. That was a help. The simplified structure is probably the most important things here, because it enabled us to eliminate a lot of handoffs. And any of you that are interested in the science of variation know, that every time you have a handoff, every time there's a communication, every time there's a task that moves from one organization to another, there's an opportunity for a mistake.

So when you simplify that structure and decrease the number of handoffs, you get a system that's much more robust and that accounts for a bunch of the improvement. Much improved communication with one organization and we have one set of goals and objectives. We have one set of policies and procedures and it makes it much easier to work together.

And then I mentioned, we have the advantage, therefore, of being able to look at the system. And so, if we're going to implement a better way of doing work authorization documents, we don't do it one way for flight crew equipment and another way for another element of the company that doesn't come together and doesn't enable us to, in fact, achieve all the benefits. We're able to implement it across the company. Next chart.

Now, I didn't – you have the book of the several-couple of hundred metrics. And I only put these up to be illustrative. We track a lot of detailed metrics and it may roll them up for me. We look at hardware inspection yield and that's sort of the classic, are parts that you're making, meeting the quality requirements. It's high and continuing high and I want you to note this starts in '99. The reason it starts in '99 is, in late '96, early '97, when USA was created, there weren't any common ways to measure any of these quality of performance. Because these were nine different companies and 28 different contracts. So we now have a unified set of measures around our quality and safety performance that we can report. We measure our overall product quality across all the different kinds of products we produce, software and paper products.

We've implemented a first-time quality surveillance approach that we call process surveillance, where we go out to a site where an activity is going to be performed, like a tile installation. And an independent auditor evaluates everything related to that operation. Is the tech certified, are the right parts there, is the right work authorization document there? Are the proper lock-out, tag-out things in place, is it FOD-free? So they look at an entire list of things. And the goal is to have the answer to be yes, to 100 percent of what's on there. And that's measuring the percentage of everything being correct, and how that's gone up over time. And then we also do inspection, of course, of our subcontractor hardware as it come in. And that shows how that's improved over time. Next chart.

I've included our industrial safety performance data here, which you're familiar with, and I've included it, because I think it's back to this issue of how does the contract terms and conditions influence the behavior of the organization. Industrial safety performance measures employee behavior. It measures the behavior of the employee on the floor. Are they putting on the right protective equipment when they're supposed to? Are they taking the extra time to do lock-out tag-out, so they don't have a risk of getting electric shock? In general, are they paying very close attention to what they're doing? And on all these charts, smaller is better. And if you look at the number of lost time occurrences, it's down to .04 across the organization. This shows that the work force is paying very close attention in performing their work, and similarly in those other measures of industrial safety. Next chart.

Another benefit in the SFOC is it that the cost and performance incentives that you've been asking a lot of questions about, motivated stakeholder behavior by USA. And what I mean by stakeholder is, the company felt accountability, responsibility and ownership for the Shuttle system. And that was reflected in their behavior in a number of ways. It motivated the company to take a longer-term view.

The 10-year horizon on the Shuttle is very important to USA. We had a six-year based contract and two, two-year options, and that allowed us to have the perspective to say, I can make investments today, in improving safety, improving the Shuttle hardware, and I won't see the benefit of that for three or four years, but that's worth it to me. And I'll give you some examples of that. And that the overall optimization was more important than the year-to-year sub-optimization, because we needed to live with this vehicle, because we were going to be the contractor for this vehicle for a considerable period.

I mentioned earlier, when we got into this subject, because of the cost incentive, USA had the discretion to reinvest savings for system improvements. I put re-invest in quotes, because this is NASA money, but it's money that we have saved, and the dollar amount today is $190 million, that we could have declared as savings and then gotten 35 cents on the dollar on. Instead, USA made a decision to change a piece of hardware on the Shuttle. In some cases, USA funded hardware changes. Decided to fix, or do an improvement to some ground infrastructure, that could have delayed further, but it was the right thing to do. I talked about the various things in investments in safety training programs, bonuses to employees, new information systems, whatever.

Now, why would you make that $190 million investment? Because, if you're the stakeholder, you're going to be operating this for 10 years and making the same kind of investment that anybody would make in order to make sure the system's healthy, and so you're not inheriting problems downstream that are going to cost you money or safety issues.

DR. LOGSDON: Russell –.

MR. TURNER: – Let me finish. I'm not claiming some new kind of altruism. I'm just saying, because you're a stakeholder, you make those proper, long-term investments. Dr. Logsdon?

DR. LOGSDON: At the time of the initiation of SFOC, there was a lot of rhetoric about savings of a billion dollars a year. You say there have been savings declared. What's kind of the sum total over the first six years, of declared savings?

MR. TURNER: I'll answer that crisply, but you're going to need a pencil because it's complicated. First, a billion dollars a year was based on the assumption that USA was given, as was originally intended, total prime contractor responsibility, including the External Tank, the SSME, and the RSRM. That never happened. It wasn't implemented the way originally intended, so that billion dollars, you know, doesn't track, because we didn't do what was recommended.

To talk about what USA saved, I'm going to give you a set of numbers. Because you have to talk about saved, relative to what. So the original base line for savings was the NASA POP, the Program Operating Plan, that showed what it was gonna cost to operate the Shuttle without a prime contract. Then a group of smart folks got together and said, what should a prime contractor – we call that the A-line, the starting point. What should a prime contractor save? If you did SFOC, how much money should it save? And they came up with a number. It was a couple of hundred million dollars less than the A-line. Then they got into negotiations with USA and said listen, since we're having to sole source this to you, we're going to give you a big cost challenge, to make sure that the government is getting best value. I think that was another $388 million, they negotiated out, which we call the C-line. And then USA's performance is saving another couple of –.

DR. LOGSDON: – Is that per year or over a period –?

MR. TURNER: – Total six years. I'm answering you six years. And then, USA has saved another two to 240 million dollars below that C-line. And then the government has tracked some savings in terms of less oversight than was required on their part. The number that we have agreed with NASA, is the total of the difference between the first six year's performance and the A-line pop, was about $1 billion. Okay.

And last item on stakeholder behavior, is the result of us at USA saying hey, we're really accountable for this thing. We formed a counsel that had never existed before, with all the other contractors, not only the SSME and RSRM and ET, but also with next-tier contractors that were supporting the major subsystems, to discuss things like quality issues, how do we maintain a healthy supply chain, the common problems that all of us were facing in terms of supporting the Shuttle. Next chart.

Now, having said there were some good contractual features that supported the intent, there were some contractual features that were not aligned to NASA's goals and culture. And this gets to the part of the SFOC that didn't work as well. This cost incentive provision really didn't benefit the Space Shuttle Program. When we saved that money, if we declared it as saved, it didn't come back to us to spend on upgrades, you know, improving the workforce, or whatever. That money went somewhere and I wouldn't have visibility, but from the perspective of the Shuttle Program, it didn't necessarily go to the Shuttle Program to invest in other items. And so, it made it a two-edged sword. Saving the money for the Shuttle Program's a good thing to be able to talk about, but it ultimately, what it was doing was eroding what the total budget was for the Shuttle. You had a misalignment between contract terms and conditions. And I'll come back to that.

The performance incentives provisions were not entirely consistent with the manifest priorities. The performance incentive motivated USA to get a vehicle ready on time. It'd be ready to launch. It would be launched successfully, and return safely.

But the way the contract was set up, if we knew that a sequence of launches, one, two, three, were coming up, and the vehicle on number one were going to be late, this terms and conditions actually encouraged us to quit working on number one and go work on number two, so it wouldn't be late, so I wouldn't have a waterfall effect. We didn't behave that way, and that circumstance actually occurred after the wiring problem. We worked on them in order. But that was despite the fact that the contract would have encouraged us to do otherwise. Something certainly was changed, going forward.

I bragged about the fact that we had these objective forward performance measurements that NASA was doing on us, but they were not used to determine our award fee performance. So the award fee performance was a separate evaluation, and our objective performance measurement system was excellent all along, and our award fee might or might not correlate to that, period-to-period. So, again, my only point here is it's a misalignment between what we were doing on the contract and the way the system actually works.

And then we talked about the change in the NASA Center role, that was in the contract, but that wasn't aligned to the NASA culture. That was very difficult for the NASA Centers. It did not want that diminishing a role. And that put a variety of stresses on the system, in terms of well then, what is their role and what's their accountability. Next chart.

And, in general, NASA was not aligned with its various elements, to the SFOC. We were all on the same page on safety. That's where everybody started. But then there were a lot of differences on what came next. Clearly, the Administration wanted savings, as Dr. Logsdon pointed out, the SFOC was sold on the basis of how much money it was going to save safely. And the Administration was focused on that. The Office of Space Flight was really much more focused on meeting the manifest. Because if you look what Station was all about, you had to look at optimizing the total cost of Shuttle and Station. And saving money on Shuttle didn't necessarily translate into a total cost savings. And so, they were more focused on meet that manifest, and of course, you have to stay within budget. Budget is always a constraint.

The Lead Center, when we had it, which was JSC, was very much focused on manifest to meet the budget, but then was also very focused on how can you get the savings spent on upgrades, so that the lead center was very interested in reinvestment in the Shuttle. Not reflected necessarily in the levels above the Lead Center.

I've already mentioned the Centers wanted to retain contract authority, budget and management accountability, so that put tension on the system. And when we were talking about, bringing, for example, the External Tank and other elements in the USA, Marshall Center, as an example, is very concerned about how, if that were done, the money would still flow through the Center, so there wouldn't be sort of a loss of their perceived role in the program.

The Administration for some considerable time promoted that what we call, phase two, which is bringing those other elements under USA, so that the External Tank problem would have been a USA problem. But that was resisted almost uniformly by everybody else except the Administration. It happened partially. We moved a few elements, and then it stopped.

And then you've heard some about the NASA budget process. The NASA budget process clearly drives short-term thinking. It's an annual focus. An example I'll give you of that is in '98, we ended up laying off 700 USA employees, not because there was a cost incentive on the contract, because there was a NASA budget shortfall. And then a year later, NASA was recommending to us that we needed to hire more employees into USA.

And the difference between those two years was an issue of budget. They had the money one year and they didn't have it the other. And you would like, if you're going to take a system view, you'd like not to be ratcheting year to year, never mind the implication in terms of the number of folks, just think what it does to the organization to go through and have a layoff and then try to rehire and to do that on a cycle.

So, that lack of alignment means, despite the great performance SFOC has had, it could have been better if there had been better alignment within the agency. Next chart.

Having said that, what would I recommend would be retained as you folks look at making some kind of conclusion about SFOC. Next chart. The biggest thing to me, is the safety and quality issue and that's retaining the reduced organizational complexity. Going back to nine contractors and 28 contracts for a scope of work that really has some very clearly integrated process flow, is a bad idea. You should retain the alignment to process and system. This is process 101. You organize around what process has to get accomplished and you try to minimize interfaces. We recommend you do not re-separate the aligned work content.

And a step that needs to go further across the whole Shuttle Program is to ensure process commonality for core processes. We talked a little bit about supply chain. There will be a supplier out there – a lot of suppliers out there, who provide products and services to all the contractors on the Shuttle Program. And we all have separate contracts with them. And our contracts have different provisions. And our contracts will have different technical requirements flow downs and different quality systems flow downs. So that contractor is having to behave in four different ways, all of us as Shuttle contractors.
Now, if this were a active production program for an airplane, that might not be as big a deal. But the volume is very low for these contractors. And it's very hard to keep a viable supply chain with a low volume. And if we're hard to do business with and if we aren't looking at optimizing their performance and reducing their interfaces, we're putting the supply chain at risk. Shuttle Program needs a single, integrated supply chain activity that unifies the support that these sub-tier suppliers are providing. And I encourage some of that has gone on that needs to be retained and expanded.

I won't go into as much detail, but it's the same thing in terms of quality system, non-conformance and problem reporting and corrective action. Those should be integrated systems across the Shuttle Program. They aren't. They're complicated with handoffs now. I think you folks yourself have looked into the bracket issue. And those kind of systems ought to be Shuttle systems and applied to this reduced complexity. And for each company, there ought to be single contract instruments for the work scope, again, so there's a unified set of requirements, unified technical flow down, and supports unified requirements. So, keep it simple.

Organization will create – complexity creates an upper limit for Shuttle safety and quality performance. Please don't recommend 29 handoffs in order to get a task done. Next chart.

I think the increased contractor accountability has been very successful. I know you're gonna have thoughts on that, whether it went too far, but please, when you look at that, we do not want to go back to the level of effort where the contractor actually doesn't feel – the employed, individual employee turning the wrench or doing the calculation, doesn't feel completely accountable, because it's what's called a government accountable function. We still have those today. About 10 percent of my work force is government accountable. That's what it's called. Now what does it mean to the employee? It means the government's calling the shots. If it's right or wrong, it's the government's call. We want these very bright people bringing their minds to work. And the contractor accountability makes it clear to each of those folks, they're accountable for the technical correctness of their products. So, there needs to be clear, unambiguous contractor accountability, even if you shift what some of this oversight insight is.

This gets, I think, to a question one of you asked that I asked to defer. I think it was maybe you, Dr. Logsdon. The terms and conditions of the contract should be aligned to NASA's goals and priorities. So, if you lay out clearly that NASA's goals and priorities are around safety, meet the manifest, ensure supportability, and improve the system, then yes, redesign the terms and conditions in a way that support that.

Now don't assume that a cost incentive is bad. But, the contractor wants terms and conditions that are aligned to the government's priorities. That's the best contract you can have, is one that when you perform well, the customer's happy. The two of you are in alignment. And so, to the extent this contract wasn't aligned – and I had a chart on that, either NASA needs to align itself around the goals and objectives, or you need to change the contract in order to reflect that.

This reinvestment that we were able to do under the SFOC was a very positive thing. So, if you eliminate the cost incentives and pull out our contracts people, you need to figure out a provision that allows the contractor to continue to have this accountability for doing the best thing to keep the program going, that doesn't get you into a continual negotiation with the government, which ultimately slows the thing down and prevents progress. I mentioned align the terms and conditions to goals and priorities of NASA, but also make sure that they reward excellent performance. You do want the contractor motivated to really do a great job.

And John, I think you asked about fixed fee – I'm sorry, General Barry. Make sure that you don't have in place something in which the contractor's range of achievement is too – is a narrow band. You want the band to be broad. In SFOC the brand was very broad. If we did very, very well, we could do reasonably well. Not as well as a development program, but well for an operations contract. And if we didn't do well, we not only could not earn anything, we could end up owing money. So, you do want to keep the contractor motivated by having enough differentiation around performance, that they put the kind of effort that we did, into performing well.

And I hope you continue the emphasis on objective measures of performance. In my career, I had the opportunity to work on DOD programs. And really this SFOC change was moving more in the direction of the way the DOD operates programs, and using CPARS, having really clearly defined what is goodness, how we're going to measure that and having both parties agree to that and then actually track and reward relative to those objective measures, is very positive for the system. It's hard work, because you have to know what is good. But you ought to know what's good going in. Next chart.

So, to finish up. After working on it for – at the very beginning, and then for five years, it was the next logical step in changing how NASA operated the Shuttle. And with the goal towards improving contractor performance, contractor performance improved under the SFOC, whether it was optimum or not, it definitely improved, relative to prior arrangements, and it improved in the important areas; safety, quality, and in cost.

I've talked about the key structural and accountability features that ought to be retained. You notice I didn't give you that long a list of what ought to be retained. There's lots of degrees of freedom to change this thing, if you retain those core elements.

And then finally there really needs to be an alignment process. And I've got to compliment Sean O'Keefe. He caught on to this alignment issue early on, and he has this one NASA initiative going inside NASA right now, that is doing exactly what I'm recommending here. Which is, getting NASA more culturally aligned to be a single organization. If he's successful in that, that will really help to address the fine line.

ADMIRAL GEHMAN: Thank you very much.

Mr. Young.

MR. YOUNG: Mr. Chairman, I do not have any charts. I have a couple of comments I would like to make, that were stimulated largely from the previous discussion. And it really has on the role of government and the role of the contractor and insight versus oversight. I have enormous respect for the capabilities of the Aerospace industry. I have enormous respect for the difficulties of space flight, whether it's human or automated. And I am a firm believer that the government has a significant value-added function involved in the execution of space programs. And let me see if I can say a little bit about what I'm trying to imply there.

First thing, if you'll allow me the discussion of oversight versus insight for space activities. If I could figure out how to remove that from the space dictionary, I would do so. It applies to defense electronics. It applies, probably, to the Hum-V. It probably applies to some of NASA facilities activities, but in my view, not to space flight. And the reason I say that is that space flight, again, whether it's automated or whether it's human involved, is really a one strike and you're out business. And there are not many things in the world that are that way. But, it really is a circumstance where thousands of people can do things perfectly, and one well-intended individual can make a human mistake, be it in workmanship or judgment or analysis.

And so, in my view, the reason that we achieve the degree of success that we achieve, is because we have a system of independent verification. And independent verification in my mind, starts, the best is testing and that is, if you can test as you fly and then fly as you test, things usually do well. In some instances you can't do that, like some software systems. And in that instance, I believe that the technique of IV and D is an important element. And in some instances, you can do neither. Such as for the rocket engine, or solid rocket, which is – you can verify the design, you can do analysis, but you – many of the functions you can't really verify on the system. And in that regard, I think the technique that we use is inspection. And that is that somebody watches what somebody else does.

So, what I'm really coming around to is, I believe that the government, NASA in this instance – let me say it a little bit differently. I am with Russ. I am an advocate for the contractor having the accountability. I don't know the right choice of the words. Maybe what I'm saying is that NASA has – maybe the contractor has the accountability, but NASA has the responsibility.

And what I really mean by that, is that I don't believe that the government is simply in a role of funding and properly executing contracts. I'm a little bit where I heard you describe or ask a question. I do think that there is a government overhead – and I don't mean that in its classical sense, involvement in these programs, because it's a one strike and you're out kind of business. I think that there are every day activities that go on in the execution of a space program, where the government really has to play a significant role. NASA has enormous capabilities at its research centers and those research centers should be used, where they can effectively look into problems that are so complex, that they should not be trusted to a single string kind of a solution.

So, I don't know whether I've helped on that, but my observation is that I said space is unique. One strike and you're out. And that says you've really got to have the best of the government and the best of industry, to have a high probability of these succeeding. And I do not believe that the government can pass that on to industry, though I have an enormous respect for the capability of industry.

The only other observation I would make during some of the review process is that you referenced is, we made some significant changes in the way we do business throughout the acquisition process in the '90s. And for a lot of reasons that we could spend a lot of time on. Much of it was moving things from the direction of the government to industry. And I, for one, believe that we went well too far in that regard. That there are functions that only the industry can do and there are functions that only government should do, and we need to work hard to have that common balance and to assure ourselves that we respect the risk associated with space flight, the risk of the human – single human mistake that's going to happen no matter what we do. That can be mission catastrophic, that we've got to have a safety net under that process that minimizes – it won't eliminate, but minimizes those occurrences. I'll stop at that point and be delighted to answer any questions that you might have.

ADMIRAL GEHMAN: Thank you very much for your views here.

I'll ask the first question and then I'm sure my colleagues here are ready to jump in here. You use the term independent verification. As one of the things we might call the government's value-added or the government's proper function. And you said that an example, for example, where it starts would be with testing, for example. And I assume you mean independent, objective testing of things, provided by contractors or suppliers or something like that?

MR. YOUNG: Well, I really mean both the – I don't mean to exclude the contractor. I think that the best form of independent verification for industry and government, is a quality test program. And all I'm really saying is that I think it – my observations would be that when we test as we fly and then we fly as we test – if I could use that kind of cliché, we maximize the probability of things succeeding. And when we operate outside of that window, again, in my view, we're not taking risks, we're gambling. Because you don't understand how a system is going to perform that hasn't been tested. So, I didn't mean to imply that the government has to duplicate industry's testing. I mean, I have a lot of respect, as I said, for industry's ability to do that, but the test program does have to be as complete as it can be. And when you're operating in a mode where it's not complete, then I said, I think you've moved into the realm of gambling, as opposed to taking risks. Simply because you don't know.

ADMIRAL GEHMAN: Right. Thank you for that. And you just used another term that I'd like you to qualify a little bit more and that's understanding. The Board is attempting to agree among itself as to how much of Shuttle missions is test or developmental flight – how much is exploration and how much is running a trucking line. And there's – obviously, there's some of each in here. But, we hear and a matter of fact, Mr. Turner used the phrase already – we hear as we talk to people, oh we understand all this. In other words, we're flying in a regime and we repeat the same regime every time and we understand this. And yet, we keep getting bitten by things we "understand". So, my question is, would you put some kind of a value statement on what is the government's role in constantly attempting to find the unknown unknowns or to really understand things that are happening?

MR. YOUNG: Yes, let me, if I might, a couple of comments. And you folded two or three things in there that I probably shouldn't go back to, but I was intrigued hearing twice, General Barry and now you ask, you know, is the Shuttle operational. And to be honest, I never quite thought about it that way. So, I was sitting here as you were talking to Russ, thinking about it.

Within the context of space, I'd say the Shuttle is operational. However, I'd say there's nothing that's involved in space that's operational, as we define it. And I don't know whether that means anything or whether I'm being redundant. But I mean, I don't think we do anything in space today that you can clarify by what is our traditional definition of operational, as being operational. But the Shuttle, relatively fits into that, but to the understanding. I, you know, again, I think that there is a constant issue with, you know, with trying to understand the circumstances associated in the systems that we're involved with. So, I don't know if I'm exactly answering your question, but I don't think we, with any space system, and particularly Shuttle, we have not reached the point that which we should say we understand it, if that's the answer that warrants your question.

I think we should reach the point of saying that we have got to constantly be worried about those items that, you know, can creep into the system that can be mission catastrophic. We probably understand, you know, the performance or the design, but that's a lot different than understanding the performance of the total system, which heavily involves the people who are operationally involved with the system.

ADMIRAL GEHMAN: Well then, how would you – in your – how would you formulate a government program, which would enable us to – which would enable this program to be inquisitive or dubiously curious? How would you fund – does personal—do you think NASA has a robust, rich program, and where does the money come from to kind of ask those tough questions and to understand what's happening?

MR. YOUNG: I don't know if I can answer the latter question. Now, let me go back to the question you lead in with. Again, personal observation, I think in the kinds of things that we're mostly involved in with space, be it Shuttle or be it Titan or be it a Delta or be it a Mars vehicle, I think that they benefit enormously by what I'm loosely going to call a third set of eyes. And what I mean by a third set of eyes, having watched it practiced and watched it not practiced, I'm not convinced it's a lot of money. I mean, so maybe we're not on the same wavelength there.

But, I think that again, the way of implementing a space program – and Russ went through a lot of it, as I mentioned is, there's got to be the mainstream activity and then there's got to be the independent verification. I'm an advocate of, I don't know what – I'm going to call it a small group. A group of people involved in programs, that don't have a responsibility. And if again, if I could say a little bit more about, you know, what I mean by that – if I go to kind of some personal experience. I know with launch vehicles, you know, I have the experience of having half a dozen or so people, who, you know, were at the Cape. And what they do is they walk around and they talk to people and the – I'll call it the less experienced engineers had the opportunity to go and say look, I saw some troubling aspects in the test we ran last night, and I don't quite understand it, but the data didn't look like it looked like to me, the last three times we've had a vehicle down here processing. It's somebody for them to go talk to. That's this third set of eyes that I'm talking about.

My observation is that's not big money. I mean, that's having a few people who have a lot of experience, and who have an intuition for, you know, for functioning in this capacity. That as I said, don't have a line responsibility, but they probably work, you know, two shifts a day. But they're not filling out forms. They're not running a test. They're available to follow their intellectual and safety curiosity and be responsive to the mass of people who do have these formalized jobs.

ADMIRAL GEHMAN: Thank you.

Dr. Osheroff.

DR. OSHEROFF: Well, I guess I'm, I mean what you're suggesting, I think, is a great thing to do. And I suspect that many organizations, not just Aerospace organizations, should have people that fill this role. But, let's say that something looks wrong when the test gets run. Don't you need a research organization then that can understand this?

MR. YOUNG: Yeah. I think the answer to that is yes. And again, if I extrapolate and maybe follow what I think you're asking, I do believe that, you know, this, what I'm going to call a third set of eyes, and maybe that's not a good choice of words, but this third set of eyes, I think if they cannot satisfy themselves, I think they would be – I don't mean uniquely. Everybody should view it. But I think they would go stimulate the research organization. My answer is yes, you need the research organization, you know, and NASA has that. I mean, NASA has extraordinary capabilities to go analyze a broad spectrum of problems. You know, much of it is utilized at the Center, such as Langley or Ames or Glenn, you know, not the Space Flight Centers. Not that they don't also have capabilities. So yeah, I am with you in that regard, but I think that what I'm trying to suggest is like the third set of eyes.

And I don't want to diminish the responsibility of the program. But I think that, you know, it's a technique for pushing things to root cause maybe. Which is what you're talking about. Which say that you have to turn on a larger group of people. You know, but I again, my observation is that NASA has that capability to do it.

DR. OSHEROFF: When you use the term, which I've heard before, test as you fly and fly as you test, that sort of suggest to me that we may – we need to be looking more at the Orbiter and in fact, the Shuttle system, on particularly launch and return, instrumenting it the way it was instrumented back in the early days, in order to – I mean, I find it amazing that the only "data" that existed up until very recent, of the temperature profiles through the foam and in through the bipod ramp, was in fact, a calculation, it wasn't actual data. And so, you can't do that just at Langley, of course, it's going to have to involve USA as well, I would guess.

MR. YOUNG: Certainly.

ADMIRAL GEHMAN: Go ahead, John.

MR. LOGSDON: Tom, one of your engagements that hasn't been mentioned is that you chaired the Space Flight Advisory Committee for NASA for some period of time, looking at the whole Human Space Flight activity. And you talked earlier about the need for a good safety net. This is kind of a two-part question. One is, do you have some comments about the current state of that within NASA?

And second, you talk about the third eyes as being not very expensive. And what about the function performed for DOD by the Aerospace Corporation? Is that just an extreme version of third eyes?

MR. YOUNG: Let me answer the last one and then I'll come back to the Space Flight Advisory Committee. I think Aerospace provides a third set of eyes, but I think they also – my observation of Aerospace is that they augment the DOD capability in assuring that appropriate programmatic reviews are done and assuring that, kind of the right, what I'm using, this independent verification take place. So, I would say part of – they do have – they do, do the third set of eyes, but I'd say most of their activity is probably – I didn't mean to get into this terminology, but it's in the second set of eyes role.

Let me come to the Space Flight Advisory Committee. And I need to probably should give just a tad of background into the group. NASA set up, a long time ago, a International Space Station Advisory group. Actually, Academy Engineers did it first, and then it moved to NASA because it was more operational. And I chaired that for several years. The Associate Administrator for Office of Space Flight, about mid-2000, recognized that they did not have an advisory group, as much of the other enterprises in NASA do, such as Space Science or Earth Science. So, he said – Joe Rothenberg is who I'm speaking of, by the way. Joe said, you know I think we should take the Space Station Advisory Group and expand it and – Space Station – and expand it or all of the Office of Space Flight. And asked if I would chair it, which I did. And that was done in mid 2000. We operated, effectively for a year, then we kind of stood down while we did the Space Station Review that I talked about earlier. And then we came back and we had another meeting in mid '02 and then NASA abolished the Committee. So it's not currently existence.

We really looked at – we spent a bulk of our time – now we looked across everything that was done in the Office of Space Flight, but we did spend time on Shuttle. And we really spent a fair amount of time on two issues. One was upgrades. And the second was infrastructure. That was discussed. And then a little bit on the next stage of privatization that Russ mentioned. And I don't know, if you would like, I'd say a little bit about the upgrade process, because at least that illustrates to me, a little bit of the style of operating. Would that be of value to do?

ADMIRAL GEHMAN: Yes, go ahead.

MR. YOUNG: What we did – there's been various stages of upgrades. The first observation I would make, a little bit following Marcia's discussion, is that NASA's interaction with OMB is, in my view, had a stronger influence on the budget activities than the interaction with the Congress. And a lot of policy is really established by OMB, in that budget process. But NASA, finally was able to get some upgrade funding in mid-2000, or I guess probably it was FY 2000. I think it was $1.6 billion at that time. And OMB required there be an independent look at it, and we did that. So, we spent a fair amount of time on that issue.

And if I would – some observations from that, probably irrelevant to what you're doing, when we first looked at it, this was led by Johnson Space Center, though all the Human Flight Centers had been involved, and basically what was presented to us was a collection of items that were: improve the cockpit, an electric APU, an APU for thrust vector control for the solids, and instrumentation for the engines, and then a few smaller items. The problem that we had as a group with that was that is was really – it was budget driven, clearly. And it was also – let me make sure I use the right words. It was probabilistic risk assessment driven. And so, what NASA had done, is they basically took – and they have an extensive PRA system for Shuttle, and they had looked at it and they had kind of taken those items that had the highest probability of making a positive improvement in the Shuttle, that fit within the budget. Okay, our criticism of it was, forget the budget. Show us the list of all of the items. You know, what would have the biggest impact and probably the biggest at the top would be a crew escape capability. Clearly bigger than $1.6 billion, and maybe not a practical item, but what we were interested in was show us the list of items, kind of from top to bottom, and then after we understand that, then you do have to apply the budget restrictions, but make sure everybody knows that the upgrades are not going from the top priority. They're going from what you can afford. We had a terrible time ever making that happen. And I'd say, probably never did.

The second thing that we were concerned about in that process was that – a little bit going back to the earlier discussion, my personal view and I think our group's view, was that for mature systems, things again, often fail because of the human interaction. And that's either because of a workmanship error, judgment error, an analysis error. The PRA doesn't have any of that in it, you know. So, we kept trying to say, you know, aren't there items that should be in a Shuttle upgrade that are not PRA hardware related? I don't mean – I think PRA is good and the hardware is good.

But aren't there items that you could change in Shuttle that make it more testable, you know, between flights? Make it easier. And Russ would know a lot more about this than I, to refurbish. My understanding is that, as an example, to refurbish the hydraulics system or do something to the hydraulics system, you have to remove the engines, you know. Is there a way that you could, you know, make some changes that you don't have to remove the engines to do that, to minimize even a – we were never able to get that into the process.

So I'm sided, a little bit of, you know, of a way of operating that, you know, was – there was no question this process was budget driven. Even though I think the items were good that they had. And I think it's no question that what the scope of what was being considered was not as broad as it could be, to assure that whatever funding we had, we made the maximum impact on Shuttle. I don't know whether that was useful or not, John. But it's background.

ADMIRAL GEHMAN: Anybody? Nobody else? Yeah, go ahead, Dr. Ride.

DR. RIDE: I'll just ask one, you know, in one of your many oversight roles, you spent quite a bit of time reviewing the Space Station and implementation of the Station and I presume, at some level, its interaction with the Space Shuttle. And the two have really become very closely intertwined over the last few years, so that it's very difficult to evaluate the Shuttle Program without talking about the Station and the Station's effect on it. And I just wonder if you could maybe reflect a little bit on the stresses – not so much the budget stresses, because I think we've heard a little bit about that. Marcia touched on that a little bit. But, maybe some of the other stresses that the Station and the Station manifest, for example, or Station requirements might have placed on the Shuttle Program. And Russ, you may have a view on that too.

MR. YOUNG: Well, two or three items. I mean, there's no question about what, you know, the fact if we look forward, I guess, all of the anticipated missions are, as we can see a manifest, for the Shuttle, is to support the Station, with the exception of two. And one is Hubbell upgrade and two is a Hubbell return mission. So, as you said, they are, you know, highly, highly related.

First off, if I just go back and just add something to the budget, even though you kind of excluded it. You know, I don't think there's any question, if you look at – I don't have the visibility that Marcia did. I was really impressed listening to all her activity on some of the budget numbers. But if you look at the Station and Shuttle together, you know, the combination was inadequately funded. And you know, one measure is that we take Station from FY '94 to – you take the first six years, I guess, from FY '04 to FY – from '04 to – from '94 to '02, I'll get my numbers right. The Station actually moved to the right four and a half years, you know. So, that is a measure of, you know, a program that was significantly under funded. So that circumstance existed.

There were continuing the Station requirements largely for supplies, grew. And so Shuttle missions had to be added in that regard. But, I think – and again, Russ would be more knowledgeable than I, but I think that demands of Shuttle flights per year, you know, were perfectly reasonable. You know, I don't think they were at all out of bounds. And I don't know that the Station demands on Shuttle moved it into an area of concern. I would say it was probably – they handled that in quite a responsible way.

MR. TURNER: Yeah, I would agree. Actually, as an operator of the system, it's better when we're flying. It's better for the workforce when we're flying. It's better for the processes and the systems when we're using them. And we were comfortable at that higher flight rate and enjoyed flying at that higher flight rate. Given that we have the adequate resources available, it's actually not putting stress on the system.

On the contrary, it's keeping the system well-oiled and working well. So, we were comfortable with that. You may not know, Dr. Ride, actually the part of it we all worried about was the EVA component of it, which was a huge load on the astronaut corps. But in terms of operating the Shuttle, we were comfortable with that and would be happy flying at that rate.

I have a comment about the test program, as long as I've started talking again. I agree with Tom. And in fact, as you look, going forward, I believe Dr. Li brought up this issue, how long are you gonna fly the Shuttle? And so, we've had this rolling period, well, we're going to stop flying it in a little while, for the last six years. Which has prevented us, I believe, from making the longer term decisions that I talked about when I was talking about being a stake holder. And one of those decisions is robust test program. And I don't mean just in instrumenting the vehicle. I mean, why don't we have a ground test program? The SSME has a pretty good ground test program.

The solid rocket motors has a pretty good ground test program, where they're getting new data about new options by testing it on the ground. But what about all the systems on the Orbiter? We talked about the cracks in the flow liner. Why isn't there a main propulsion system test article, where we're really thrashing it out and having it be the fleet leader instead of the vehicle that flies the most the fleet leader.

Well, when you don't know how much longer you're going to fly the vehicle, you probably don't make that investment. One of the things that ought to come out of this is, we should be doing more testing. And then that ties into your question about then, the research and development activity that can go on in parallel with that within NASA, which is absolutely their role. To get the data from that, use it for helping to make the Shuttle more robust, and by the way, the design of the next RLV, cause one of the things that folks are missing here is if we don't get the learning out of the Shuttle into the next RLV, it's gonna be different but not better. And so, why wouldn't it have cracks in the flow liners? So, I do think that a lot more can be done with testing and the result of that testing will be a much more robust Shuttle Program.

ADMIRAL GEHMAN: I'll ask the last question. We thank you very much for your patience. Mr. Young, again, I wrote down something you said here about the Centers should back up the programs – the Centers should be able to back up the programs. One of the things that this Board has done, in order to understand how high reliability organizations attempt to achieve high reliability, there's a couple of characteristics which we have found in common among them. And one of those characteristics is independent verification. We mentioned for example, in the case of the Department of Defense, almost all of the launch vehicles are contracted for, but then they pay Aerospace to do independent verification – second set of eyes. We find that in other industries too. I won't mention them but we have found a number of cases in which there is a very, very strong, independently funded, not in the program, independently funded –.

MR. YOUNG: – Critically important.

ADMIRAL GEHMAN: Set of eyes, who don't care anything about schedules, don't have any interest in budgets, and they independently verify what's happening. And every time they have an itch or a scratch, they have the funds to go look at it. And they are not stealing money from the program and they are not slowing the schedule down, unless they raise their hand and say, wait a minute, cease and desist.

My understanding of the process that we have here is that that second set of eyes – the first set of eyes is the contractor. The second set of eyes, NASA has decided that they are the second set of eyes, because there's nobody else doing it.
But what I'm concerned about is that my understanding of the way the process works is that if you go to any one of these Centers, particularly Human Space Flight Centers, that the engineering sections that work on Shuttle are funded by the program. And I'm wondering whether or not you agree with me or whether or not you would describe it that way, that what we should really look for, if we believe these characteristics that I talk about, they ought to be independent, independently funded set of people who don't care about the schedule, don't care about the budget, that whatever this entity is, you either hire it or contract for it. You ought to get Aerospace to do it for you, you ought to get somebody else to do it for you, or it ought to be, if you want it to be in the Centers, it can't be funded through the program. It's got to be funded in some other way and whether or not the full cost accounting issues are an issue that we see, well now we've got something butting heads here. Do we have two concepts butting heads here?

MR. YOUNG: Let me see if I can really respond to your question. It's my belief, and I might not be close enough to it. But it's my belief right now, if one of the programmatic Centers or Space Flight Centers called up Ames today and said we've got a real issue. You know, you understand plasma flow problems, you know, better than any of our people. I think today, Ames could, you know, turn forth, put a group of people on that and go work that problem. And maybe I'm mistaken. So, as I said, because I'm, you know, a little removed, but I don't think that would be a budget item. And I think that kind of capability. So, I think NASA has a lot of that capability.

Now, it does require – one of the things that you're maybe talking about is that the scenario I just went through required somebody to call up and ask. And that's an important consideration. But, and so maybe this third set of eyes I'm talking about, you know, in NASA's instance, maybe has to be broader in that regard. Because I don't know that Ames would recognize the need for that, unless, you know, unless they'd been asked.

So, I'm stumbling through, but my experience, which again, may not be today correct, but I think it probably is, is it's not so much a budget issue, as it is whether or not the Ames of this regard would know about the issue. So, if you're trying to say we need to worry about how do we couple this, what I think is an enormous capability that NASA has, more closely that their curiosity can be stimulated, as well as the program's, that's probably a valid thing to, you know, thing to pursue.

I think the other item – and I'm trying to figure out how to say this without it coming across adversely. My observations – and I want to emphasize, my observations, is that the Human Space Flight area doesn't ask for help very often. And you know, if you interpret that as, you know, being a critical comment, I meant it, you know, that way. I'm not trying to over-emphasize.

But again, my observation is that there is a Human Space Flight culture, you know, that exists, is you don't get a lot of – and you all probably have the statistics, which maybe prove me wrong, but my observation is, do not frequently ask for help. Which maybe again, comes back to the importance of what you're saying, is that maybe this third set of eyes has to have some relationship beyond Human Space Flight or that that culture has to be somewhat changed.

ADMIRAL GEHMAN: Well, gentlemen, thank you very much on behalf of the entire Board. We could do this for hours, because your depth of knowledge is really impressive and your willingness to share it with us is deeply appreciated. I want you to know that. I wish we could can it and take it with us. But, it's been extraordinarily helpful to us, as I expected it would be. And we all certainly share the same goal here and that's to learn from this tragedy as much as we can, and get back to flying again. And we thank you very much.

And we have a short break here and we're going to re-set up for a 1:00 press conference right here. Thank you very much.

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