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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