

Interviewee: Dr. Robert C. Bless

Interviewer: Dr. Robert Smith

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DR. ROBERT SMITH: The last time we spoke on tape was late '86, and I guess at that point the issue about the Second Thermal Vacuum Test was something that was [Tape goes blank. Smith then says, "Okay, we're back"].

DR. ROBERT BLESS: So many problems had been discovered after the First Thermal Vacuum Test that many of us felt it would be extremely useful to do a second one because there were a lot of modifications made to thermal control systems. In particular, it was discovered that temperature sensors weren't in the right place, and some things needed to be insulated more. All sorts of problems of that ilk surfaced. Practically every box in the ST had been taken out and worked on and put back in, and it seemed to many of us that it was taking unnecessary risk not to do another thermal vac.

Of course, against that is the usual ST anti-project constraints: time and money. There was a long debate about it and a fair amount of -- [tape going off and on]. As I recall, Jean Olivier convinced himself that it was not necessary. I think [Domenick] Tenerelli felt it was necessary. There was, as I recall, considerable pressure on Tenerelli not to think it was necessary, pressure from Lockheed. [Fred] Wojtalik was, from my perspective, never too enthusiastic about testing. Anyway, it turned out that there would be no Thermal-Vac. It was deemed unnecessary. As it worked out, I guess they would say they were correct not to do a second Thermal-Vac, but if we hadn't had all of the problems and cost overruns and so on, I still think it would have been wise to do it.

Interestingly enough, one of the things that we had pushed very hard, all the time we were in Lockheed, was to try either HSP [High Speed Photometer] or more particularly STAR in all of the bays because every instrument had difficulty being installed the first time. We thought if we could fit everything, you would have something on the ground which you could use as a standard, but that got absolutely nowhere. That's come back to bite them because now they're not quite sure what fits. I'm getting off the subject.

SMITH: You're talking about the pressure that was on Tenerelli.

BLESS: Oh, yes. Obviously I'm not the best source of this, but in general the Science Working Group felt we should test, and the project was against it for financial and schedule reasons. I think Lockheed was being told by the project, "Don't do it." Tenerelli felt that we should test. He was quite concerned. Anyway, it worked out that we didn't have the second test and everything, the thermal control system's working very well, so they can correctly say, "See, it wasn't necessary." I think it was still an undue risk, but I can understand why the decision was made the way it was. That was one of the big things going on at the time. Let's see what else.

SMITH: There was a change of project manager, I guess a little later. There was a meeting coming up on Christmas, I think. Coming up towards Christmas, and [Jerry] Richardson at the time was project manager.

BLESS: That's right, and he was not on very long before he died. As far as I could tell, his main HST experience had been with Perkin-Elmer, so he was experienced from that point of view.

Everybody was scrambling very hard to get the commanding necessary to operate the spacecraft. Also during this period all the instruments had to cut back on the OV and SV programs because they were taking too long as people understood more and more just how difficult and how time consuming some of the tests would be and how hard to implement. There were pressures on us to cut back on SV. I've forgotten, but we had, I think, three major revisions of our OV/SV programs all in the direction of making them shorter. There was a lot of testing that went by the boards because of that, so we were all concerned about that. There were other things: getting things ready for launch, getting the commanding in place. The phases of the launch and initial checkouts were worked very, very hard. They had many rehearsals for the first few days after launch. For the first month, that sort of thing. But there never was a real run-through. The term got over used, but our goal was an end-to-end test. There never really was an end-to-end test in the terms that we understood it.

SMITH: Just for the sake of the tape, how did you understand it?

BLESS: What we wanted to do was to pretend that we were actually up and operating and have a science proposal go in to the [Space Telescope Science] Institute, have them process it, deliver it to the control center to be transmitted to the space-craft, to have it executed on the spacecraft, data returned back to the control center, up to the Institute, processed and results coming out. There were some links that couldn't be closed. I think we couldn't see TDRS at that time from Lockheed. I remember they tried several times, but there's part of the link that couldn't be matched. But apart from that we wanted to exercise the whole system from end to end. That never really happened, and so we had literally, as far as what the instrument people, the Science Working Group people and so on considered real testing, it was measured in hours, not days. The project concentrated very heavily on the first few days after launch. The argument is obvious. If you lose it then, why, none of the rest matters. But we never really did much in the way of testing the operations.

The tests that we worked out at Lockheed, came from an engineer Pat Eddy, a wonderful woman. She was the one who got those small -- those test were sort of repeated in various versions. That was about as close as we came.

So we went into launch thinking -- -- launch this thing finally. On the other hand, we felt, we're not in the greatest shape to launch and operate ST because of the lack of testing, the lack of people. I mean, it was very success oriented. They had a whole series of contingency plans, and they had done some testing of those, and they worked through plans. Those are useful because it makes you think about things that you ordinarily wouldn't think about, but I'm not sure, after the fact, that they were very useful because the real conditions are always different. But it was a good thing to do. They had done that. As far as the health and safety of the spacecraft goes, it was probably in reasonable shape. As far as the operations go, I don't think it was in such good shape.

Of course, the first attempt of launch took place in the midst of a real media circus. Literally we were in a tent, a circus tent. [Chuckles]

SMITH: This was for the actual launch?

BLESS: This was for the actual launch itself. Where we all were trooped down to [Cape] Kennedy for this. There had been something before that at Goddard. Oh yes, we all went to Charm School. All the PI's went to Charm School.

SMITH: What's that?

BLESS: It varied a bit. But they hired some outfit here in Washington to do this. What they would do is to try, first of all, to prepare us for the kinds of questions the media might ask, the obvious ones like the 'have you stopped beating your wife' kind of question. They warned us

about things like not repeating a negative question because that simply reinforces the question in the mind of the listener. How to handle hostile questions. How to stand in front of a camera so that you don't look too awkward and uncomfortable. Things of that sort. As I recall, they did it from about 9 to 3, but there were a lot of breaks and interruptions. It was moderately useful, I guess, to see yourself. They'd have an interview, and they'd tape it, and then they played it back to you, and you could see yourself and gasp in disbelief at how bad you looked. That part was interesting, but everybody there had talked with the press on several occasions. It was an indication of how seriously NASA was taking this whole business of meeting the press.

SMITH: Was there any kind of pressure, subtle or unsubtle, about the kinds of answers that you were supposed to give?

BLESS: Oh, I think there it was not any overt pressure, but obviously they wanted always the best face possible on things. Oh yes, yes. We had this press briefing at Goddard. It was still cold, I remember, so it was probably around February. We were herded into a temporary trailer, and [Charles] Pellerin gave us a pep talk. This was completely as predicted. Again NASA wanted to make sure that all of our disagreements and bitches were behind us now. We were entering a new phase, and we were all going to be happy. It was predictable, and again you could understand it from his point of view, but it was a little bit amusing.

In the PR room, the Visitor's Center at Goddard, they had various people up there to be interviewed by the press and taped, and we sat behind desks. Barbara Mikulski was there, and of course, she got a lot of attention. And [Leonard] Fiske was there and Pellerin and [Ed] Weiler. The PI's were asked to describe one or two real nifty observations we were going to make and all that kinds of stuff. I guess it went okay. But I'm not the one to judge that.

This is a completely irrelevant, but amusing point of mine. We were in seated behind desks on chairs with rollers. The platform had no back, and I discovered -- I tend to move around when I talk -- at the end of the press conference that one wheel of my chair was a fraction of an inch from the edge, and had I just pushed once more I would have made the biggest impact than I've ever made on the project. In front of the cameras, I would have gone over on my rear end. I thought that was sort of amusing for me.

SMITH: How did you feel about this kind of activity? I guess it carries on during prelaunch. I mean it's not quite performing seals, but --

BLESS: Yes, I would not like to characterize ourselves that way. [Chuckles] But you're right, there was an element of that. After all the years and the travail and so on, they wanted ST to get off with a good start. It's been a big problem, but all those problems are behind us now, and we're on the threshold of a glorious new era, blah, blah, blah, blah. After a couple billion bucks you can understand that they wanted to make sure they were going to get some proper attention.

I just don't remember. There must be a tape of that press conference someplace, and it might be interesting to look at it and see what things were said. Yes, we felt a little uncomfortable, not so much about talking with the press because, again, all of us had done that. But we've never been in anything that was as organized and orchestrated as this was, and that made us a little uncomfortable. Then afterwards or during the morning -- oh yes, it lasted all day -- reporters would corner us and interview us, and that's where they got a lot of this footage that they would use later, you know for background, and that was interesting. Then they had a very fancy lunch. Were you there?

SMITH: No, I must have missed this.

BLESS: They had a very fancy lunch, which really was worth the price of admission. And then they went on. They got everybody and his mother in the act. In the morning it had been NASA Headquarters, the higher up project people, and the PI's. But in the afternoon they got everybody

else. Practically every subsystem had its day in court. It went on and on and on until about 6 o'clock that evening. We didn't hang around for it, but the brave souls who did said it was just terribly long and tedious, and they overdid it enormously. Reporters were leaving; they were getting a little surly. I mean, a whole day of this and being talked at about how great everything was, and they were given detail that none of them were interested in. That's my impression of it.

SMITH: Had you had any attention to this public affairs program, that somebody somewhere had devised this?

BLESS: Somebody in NASA Headquarters, one of the PR people, whether it was [Charles] Redmond or not, I don't know, but some of the PR at NASA Headquarters were doing this. And when we found out what the whole program was, we thought, "My God, that's just far too much." Only the most technically minded person from *Aviation Week* would be interested in a lot of the stuff in the afternoon. But as far as the general press goes, they were not interested. So that went on a very, very long time. Far too much detail, we all thought.

Then there was the big festival at the Cape. That was the next big PR thing, and for that they had a special tent. We were all supposed to make posters or instruments and, again, zippy observations, and all the major companies were there with handouts. Appointment books they T-shirts, and all this sort of thing. That was a big festival that lasted a couple days. We were supposed to be there. Again, there were interviews along the way with individuals.

SMITH: What would have happened if you had said, "I'm not interested in this. I'm not staying. I'm staying in Madison."

BLESS: They would have insisted that somebody represent HSP, and they would have thought I was very being very uncooperative. This was the first time I'd ever been to the Cape, so I enjoyed previous launch. At launch attempts I was always at Goddard. I'd never been actually to the Cape before, so I thought that was interesting.

I thought some of the PR stuff was hilarious. They got representatives of the Hubble family, for heaven's sake, there. I talked to a nice lady who was one of the Hubbleites, and she'd never met Edwin. She didn't know a thing about him, didn't know why she was invited. [Chuckles]. They apparently have a reunion every so many years, and it's open to anybody whose name is spelled Hubble in any way you want. There are thousands of people in the country. Anyway, they were all there. So it was of overkill.

Then, as I said, the industry, the industrial people were there with exhibits. And, oh, NASA had not just ST, they had another tent with all their other missions, jazzy missions. It was big. They were really milking this for all it was worth. They had lots of interviews for CBS, NBC, ABC. News stories and all.

One thing that was interesting. It became clear that some of the reporters and news people simply were trying to stir up controversy which I guess from their point of view makes life interesting. And somebody from "Nightline" knew about this article I wrote.

SMITH: The one from the National Academy?

BLESS: Yes. They were getting Pellerin on, and they wanted somebody to debate Pellerin about ST. When it was clear that they really wanted to have a fight, I said, "No, no, no. This is ridiculous. I mean, the differences that I've had are known. But here we are on the eve of launch, and to bring up all this stuff about how it should have been done differently, come on. It's pointless." That part was, I think, something of a revelation. I guess we were pretty naive in that sense. A lot of these people were going around really trying to stir up controversy, trying to get people to fight with each other or at least, to disagree. At that point in the history of the program,

that was, of silly. That wasn't a very useful thing from our point of view. So nobody really wanted to get involved in that sort of stuff.

One of the greatest pieces of organization on the ST project was performed by Kathy Stittleburg (HSP) on the day of the launch because there must have been a couple of hundred people who met through her efforts, met at Denny's at 4 o'clock in the morning. She organized the caravan, and it was many, many cars, and we all had to go down on the highway.

SMITH: We're talking about the first attempt?

BLESS: The first launch attempt, yes. There were too many cars to get organized in the parking lot, so we had drive down the highway, pull off on the side of the road, and wait until we were sure the last car had gotten there. Kathy was going to be the last car, and she made sure we couldn't miss her. She came barreling down, honking her horn. We were quite a sight. We all had red and white ribbons to identify us when it was light enough to see the antennas. Red and white are the colors of Wisconsin, and we just loved giving these to Caltech and to Princeton and various others.

Anyway, that went very well and we lost -- through sort of an error on their part, not because of anything else -- lost only one or two cars out of this 100 cars or so that we had. And we all ended up the same place on the causeway. This was mostly HSP and Wide-Field Camera plus friends and so on because the others had, I think, opted to go to the VIP area. We thought it was more fun to have everybody be at the other end. And Kathy had gotten gallons of coffee and rolls and milk and all kinds of things and all sorts of -- she did an incredible job. It worked very well, got down to T-minus three minutes -- it was something like that -- and I believe it was APU [Auxiliary Power Unit] did not work properly, and they had to scrub. That was a huge let down.

Kathy organized the next launch attempt Jim Westphal but Jim brought his own Caltech ribbons. [Laughs]

SMITH: What was your sense at that point?

BLESS: Oh Jesus, here's another ST [Laughing], another glitch in the life of ST, and we were pretty down. We had planned to have a big party at Denny's, our headquarters, the motel there. I felt everybody needed the boost, so we went ahead and had the party anyhow and invited everybody in sight. We had all the wide-field people there, HSP people, some of the other people came in. We had well over a hundred people at the party, so that's how it ended.

There were all kinds of rumors floating around about how they were going to try again tomorrow. Could they do that and so on? Obviously a big rumor mill at this point. But it was clear after a couple days that they finally said, "Nope, we're not going to do it." It's interesting, again I had never been there before, but apparently most of the motels in the area tune into NASA Select in their motel televisions, so you can sit there and watch all of the interviews and get the latest news and so on. It was convenient.

We all went back -- what was it, two weeks later? Something like that.

SMITH: Yes, I think it was roughly two weeks.

BLESS: About two weeks later they were going to try again. Of course, this time a much smaller contingent went because we couldn't afford to pay for people twice, so only a few of us went down. We did the same thing, although with a much smaller group, got out on the causeway, and this time it worked. Have you ever been down to a launch?

SMITH: I've never been to a launch.

BLESS: A shuttle launch, it's impressive! It is impressive. Of course, you're several miles away, and you see it long before you hear it. What the television doesn't show, because it usually follows the orbiter, is this enormous cloud of steam that forms around the launch pad because of all the water they put in to dampen the flames. And there's this enormous cloud of steam from which the shuttle rises. There happened to be a cloud, a thick cloud bank, just perhaps a thousand feet up, and the shuttle went through that and came out very dramatic, early morning and so on. It was a spectacular sight! Diane, my wife, was gasping for breath she was so impressed. Then the noise gets to you, and it's a physical sensation. I mean you feel this thump, thump, thump against your chest. It's a very, very impressive experience. We all felt happy and relieved. Ed Groth had brought a case of champagne to the causeway. The champagne made a nice little party there, and then we had another party back in Denny's.

Oh, there was something else. One of the ABC reporters, he's very familiar -- I've forgotten, I'll think of the name -- followed [Jim] Westphal around on the second launch from before launch, up through the launch, and afterward and so on. They were literally following him around with a camera. I'm sure Jim remembers who it was. That was to get the real instant impressions. So it was great. And for a day or so, let's see -- oh, then they started deploying the spacecraft and we all, the HSP people all met, along with a few friends in my room and watched it for a while.

SMITH: So you were still in Florida when they were trying to deploy the solar arrays?

BLESS: Yes, yes, and we were leaving just about the time when they were starting to have some troubles. So we kept tuned to the radio because we had reservations to go back, and of course, there wasn't anything we could do from where we were. So we went back thinking that everything was going to be okay.

SMITH: So you didn't see the solar array problems as major?

BLESS: They're worrisome, but at least I assume everybody had the feeling, sure they're going to meet some difficulties, there may be some problems, but they'll be overcome. One thing that nobody every dreamed of was that the mirror was going to be a problem. That was not at all in anybody's mind.

SMITH: When you say, thought "there may be some problems," you're saying software?

BLESS: Yes, operational problems. Something wouldn't deploy. The antenna didn't deploy. One of the high gain antennas, you remember, didn't deploy. The wires wrapped around, the cables wrapped around. That's the kind of thing. That's the problem, but you work around it, as they did. So it was that sort of thing that you thought would happen. You weren't surprised that this happened. It always does. And then they started getting these images, and as soon as they focus they'll really be great, but look how great they are right now, and so on. Of course, it became clear that they weren't going to get focused.

SMITH: Now leading up to that, there was this long period, I guess, where people who did the calculations of what the focus should be were not to present to the public a four opt second image star, so there's a kind of calculation, I guess, that went to actually take the first image --

BLESS: Yes, that had gotten complicated with some PR. During the big festival that preceded the first launch, [Leonard] Fiske allowed himself to get put into a corner by some newsman, I've forgotten who. The news media really liked, the Voyager flyby seeing things in real time and putting the microphone down somebody's throat right then when they slip in the image to get his reaction. And Fiske tried to explain that isn't the way ST works. But they were badgering him pretty badly about this, and again I've forgotten the flavor of it, but there may have been something, what are you trying to hide or something like this. -- So he allowed himself to agree,

"Yes, on 'first light' the press can be there." We were watching this interview from the circus tent, a whole bunch of the instrument people, and everyone said, "Oh my God, how did he do that."

This 'first light' thing had become practically mystical. You know, it's a term that's used by astronomers to indicate the first time you're taking a peek at something with a telescope. It doesn't mean that it's working. It's just that now you can begin the process of seeing how it works, focusing it, and adjusting it and so on. So after 'first light' for a sizeable telescope, there may be a six month period of testing before you really start to get some good data.

They used the term a lot with ST, and the reporters wanted to be there at 'first light.' It sounds good, you know, 'first light.' All kinds of suggestions and implications. The reporters wanted to be there to see that, and that's what Fiske allowed himself to get boxed into. So there was well documented on tape, Jim Westphal describing this image, you remember, when they first turned the ST to a star. Of course, the image is not very good, but everybody thought that was because the thing isn't focused properly.

SMITH: Where were you when they were doing this?

BLESS: I was in Madison because the press conference was being held at Goddard, again at the Visitors Center, and I could watch it from (?) because we had NASA Select, I was not involved in this at all, so I just sat there with everybody else in the conference room at SAL. They got us through. Actually pointed to a star and got it. That was pretty good. That meant they were pointing within a few arc minutes. After all, that's pretty good. Of course, the images didn't look all that great, but everybody said, "Well, it's not focused." They were concerned about this because they didn't want to present the press a fuzz ball.

SMITH: When you say these are the guys who were down at Goddard who were talking about the image?

BLESS: It was primarily Jim who was doing the talking. Again, Headquarters people were there. It was on a Sunday, I think, when it took place. Everybody felt, "Sure, the images are not great, but they'll come into focus." Of course, there was this long, long period when it became clearer and clearer that something was wrong, and it wasn't going to be in focus. Then people started looking at the image more carefully, and it had all the earmarks of spherical aberration. That, I guess, was sort of officially announced to the world in June, was it?

SMITH: On the 26th, and in the afternoon there was a press conference.

BLESS: I was on my way here, and I've forgotten whether Joe Dolan called me up, or I heard it in his office, but at that Science Working Group, we heard the news, and we had to get up and say the effect on our science. Again, it's understandable that people would want to know this in Headquarters and, the project. On the other hand, we literally just found out about it, most of us. Maybe Wide-Field had an inkling of what was going on. They probably knew what was going on because they were using their data.

It was a pretty wild session. We weren't sure what was going to happen. It was clear that the through-put was going to be affected. As it turned out, the spectrograph suffered the least. All they suffered was loss of through-put. That's bad enough because they're talking about big losses in through-put. The cameras, of course, lost detail, and we lost a lot of our ability to do accurate photometry because of the large image.

SMITH: Maybe if I just backtrack a little bit. You said that it was becoming clearer that there was a problem. Do you remember how you became aware of that? There was, for example, I think an Astronomical Society Meeting.

BLESS: Oh yes, Albuquerque. Yes, right. Everybody was still in a self-congratulatory phase, and we all got up and said how far we had gotten in our check outs, instruments were turned on, high volts were on, or whatever. The engineers, Jean Olivier gave an account. I think he mentioned where there might be problems we get with the mirror, but I think there were people who were thinking, "When is this thing going to get focused? It's taking a long time." Everything was success oriented, and that isn't the way the real world operates, so nobody knew the magnitude of the problem that was going to be at that point. So that was another possible problem.

At the Albuquerque meeting I don't recall hearing rumors. I'd have to see if there was anything in my notes about that, but I don't recall hearing rumors. That was what, early, late?

SMITH: Around about the second week.

BLESS: That's right, the second week. I think probably the next week must have been the Science Working Group meeting, something like that or maybe ten days later.

SMITH: The Science Working Group was on the 26th or 27th.

BLESS: Yes, that's right, and that was when it was official. Again, I'd have to look at my E-mail to see if there were rumors or rumblings about that ahead of time. Jim, since wide-field data was being used so much, would probably know. Chris Burrows would know, people like that. I don't really remember. But the real thunderbolt was the Science Working Group.

SMITH: You said that you had to get up with basically next to no notice. You'd heard for sure that there was this problem, what, a day or two before the meeting?

BLESS: Hours. At least I had because I was traveling during part of the time when the news was getting around, so I had no time to think about it or talk to anybody about it. All we knew was that the image was a fuzz ball. I guess there was some idea of what the point spread function looked like at that time. But I'm not sure how much of that we knew either. So yes, we had to get up and say something. And yes, it was all pretty grim.

Nobody expected this. See, the people like [Dan] Schroeder and [Bill] Fastie, who had supposedly been following the development of the mirror, had been given all this wonderful test data. They had looked at it and analyzed it, and the test data looked like everything was just peachy keen, and they reported that to us. We all have great respect for Schroeder, for example, and so we thought, "Well, that part, at least they did that right." Perkin-Elmer took forever. But Schroeder and Fastie did not, of course, take part in any of the actual testing, so they only could work on the data they were given.

TAPE 1, SIDE 2

SMITH: Were you ever aware of the fact that they weren't going in to see the actual testing done or so on.

BLESS: Yes, I think we knew that, but nobody made much of it. You know that there were difficulties with Perkin-Elmer because of the military business. We had a Science Working Group there or at some meeting there, I've forgotten what. It was a big pain because you weren't even supposed to go to the bathroom unless you were escorted, for example. So we thought, "It's just more of the same, so they're not involved." Also, you would have had to be involved on a day-to-day basis for a long time, and the program was simply not structured that way, did not allow for that. So we never dreamt that the mirror would be anything but as good as everybody said it was.

SMITH: What was the perception of Perkin-Elmer at that time when they were doing the mirrors?

BLESS: Oh, I think that everybody thought that managerially they were wildly incompetent. They weren't all that great in some of their engineering either. We got these impressions at Quarterly Reviews. All their mechanical stuff was a bit flaky. They kept on having troubles with electronics. Pretty second rate outfit was the impression that we all had. As I say, managerially they weren't even second rate. They were wildly incompetent. But we thought their optical work was maybe the one thing they can do. That's the reputation they had, which by the way wasn't held by the astronomers. This was supposed to be this big prestigious optical house, but there are a lot of horror stories that go way back about the optics that Perkin-Elmer manufactured for astronomers, so they never had that great reputation with astronomers. But we thought, "Surely they can make a mirror, for heaven's sakes. Even the mirror with the tolerances of this one." So we could believe that, while they were pretty incompetent in most ways, we did not, unfortunately, extend that lack of confidence to their mirror manufacturing. Again, partly it was that everybody had other things to worry about and that there was nobody who was really able to get in there.

Dan Schroeder was going to a meeting in Europe and was going to talk about the performance of HST before launch. The optical characteristics. There were -- you probably remember this -- people at Headquarters who said, "No, you can't do that because this will tell all the baddies in the world the marvelous capabilities of U.S. optical houses and therefore tell them something about one spy satellites and whatever." We all thought that was the silliest thing we had ever heard since the general capabilities had been trumpeted to the world for years. We thought it was ridiculous and made some serious noises of quitting if they didn't let him go to the meeting. So they caved in and he went. But there was that attitude.

SMITH: So there was always that kind of air around Perkin-Elmer, but you weren't supposed to ask too many questions of Perkin-Elmer.

BLESS: Yes, it was hard to find out really what was going on there. At STOPAT we never decided that we should look at the mirror. We had other things that we understood better. In addition, these other guys were looking at the mirror, Schroeder and Fastie so nobody in the Science Working Group had any inclination or feeling that there was something wrong there. At least nobody expressed it, if they did. So it was a big shock, a big surprise. I'm sure I told you the story, but I was asked to go up to the House Subcommittee on Science.

SMITH: That was then, just for the sake of the tape, Chairman Roe's subcommittee, or was the earlier testimony in '85 when you talked about AB?

BLESS: No, no this was the second time. That wasn't the guy's name --Trexler?

SMITH: Trexler's the House Appropriations Subcommittee. I know there was one hearing when you actually testified along with John Bachall. That was before the Authorization Subcommittee, that was when Roe was the chairman.

BLESS: Roe, Roe, I thought you said Rose, and that didn't ring a bell. Roe, yes, yes, that was the guy.

I called up Schroeder to find out what he knew about the mirror mess, and he said that he calculated that if this little widget had been off-set by 1.3 millimeters or something -- I'd forgotten what the number is anyway -- that would have gotten the effect, that would have produced fringes that they actually saw, and it would have been wrong by about what was thought to be the right amount.

That very day --Westphal sent me a copy of a technical report from the European Southern Observatory, reporting on the mirror, their new technology telescope, which has a thin mirror that can be bent. That had been made by a German optical house -- who was it? Leiss, Anyway, a very reputable German optical house. They had spherical aberration, and from what I could tell, about the same amount as the ST mirror. They had done an analysis, gone back

through the records to find out where things had gone wrong, and it was exactly the place where Schroeder had speculated and by the same amount. It was spooky learning about both those things on the same day, just before I went down to DC. ESO could get around it because they could bend the mirror, and that was one of the options, looked at: could we move the actuators enough to fix the problem? It turned out that we could fix only ten percent of the problem. As I said at that day, I think when you say there should have been more testing mission you are totally neglecting the atmosphere and the attitudes at the time. The project was so far behind schedule and so much over budget that the notion of having a major test to test something that of course was okay, I mean, how could you doubt it?

SMITH: And this was just months after the program was being rejittered, extra money being put in, and so on.

BLESS: This wasn't the March. This was the second time -- oh wait, wait, I'm sorry --

SMITH: I'm thinking that the mirror --

BLESS: Oh yes, yes.

SMITH: -- late '80.

BLESS: I see what you mean, I misunderstood. Yes, that's right.

SMITH: They were already falling behind, but having been given this big help and months later they're already behind and here they're having to say, "We've got to go back."

BLESS: That's right and test it, not deliver because they were late on delivery to Lockheed, and it was just totally out of the question.

SMITH: So the 26th of June comes and the Science Working Group, and you said the mood was pretty wild.

BLESS: Yes, I mean everybody realized it was true, and we were starting to think about what it meant. Not everybody understood the implications, of course. Some people said glibly, "Well, you can do deconvolution." But the point spread function turns out to vary over the face of a wide-field chip, so you have to have different point spread functions -- many different point spread functions -- over one chip. It's much, much more difficult than people had thought. So it was just another low point in the history of ST.

SMITH: Did you have any thought at that point that maybe they'd just turn it off? Or was that a possibility?

BLESS: No. There had been too much press, too much money, too many promises. At least as far as we were told -- I don't know what people at Headquarters actually discussed -- but at least as far as we were told, or what we knew, there was not a question about that. They were going to fix it somehow or other. Of course, that was when [Leonard] Fiske was saying some pretty silly things to Congress about how simple it was to fix: that they'd just replace the wide field camera and that was it. End of problem.

SMITH: Yes, I was looking through my notes of that 26th of June meeting. There you, in fact, raised a point about overselling what could be done.

BLESS: So we never had the impression that they weren't going to try to fix it. Of course, we're still in the throes of that, trying to understand. By the way, I asked today at the meeting when the collimation of the telescope will be completed and the best focus, best in some sense or other, I'm not sure what, established. That will not be until maybe early next year. So that's nearly two

years after launch before things collimated properly. You see, the astrometry people have not been able to even begin their OV rescue program because things have been changing all the time. The characteristics of the telescope, plate scale, distortions, and so on have been changing. It's a good example of how wildly overoptimistic the people were at the beginning and success oriented. For the most part, the instruments are working pretty well. It's the usual thing: you sit there and cry about what could have been.

SMITH: Again, just going back. Maybe we can skip track forward and look at the change in perceptions of how things were going. But on the 26th of June I knew the things you mentioned were that the target acquisitions were much more complex, that you thought perhaps about half or perhaps more than half of the HSP programs might be doable, but you were definitely out of the business with some things like [unclear].

BLESS: And the target acquisition was a nightmare. First of all, the FGSs did not work properly, partly because of the aberration -- they were dealing with a much bigger image -- partly because it was a complicated business of getting them all working together properly. And in order to do photometry, we had to center and recenter our images and our apertures to a precision that we never dreamed of doing before launch and which, until recently, I thought we would not be able to achieve because we're sensing the position of a big one second arc fuzzball, with a one second read beam. And yet, not through work I did but work people in the HSP group did, we can now center things to within a few hundredths of an arc second. That's why we're behind although we're catching up rapidly, interestingly enough, because all the instruments are just sitting there, not doing much. That's why we got so far behind, because we had to get this pointing problem out of the way. The cameras don't care. From the point of view of pointing, they don't care because they're in big fields, and when the spectrographs find just one or two apertures, then they're happy. So that was a big problem, but we think we know how to do that now. And the FGSs are working properly now.

There was a time in January of this year when it became clear the FGSs could not drop a star reliably within the ASP ten arc second finding aperture. After we had blown many hours of spacecraft time with seeing nothing, I really got annoyed and said, "This is silly, and we're not going to do any more HSP testing until this is fixed." I think that was [Joe] Rothenberg's first Science Working Group meeting, and he, interestingly enough, was quite surprised. He had not been told that the pointing was as bad as this. He got very exercised about that. I mean, the emperor really didn't have any new clothes, so things started to happen more seriously. People started to work on that harder. I mean, other people also made a noise, but my saying that we weren't going to test anymore, that really caught people's attention.

SMITH: When you say people, who do you mean?

BLESS: Science Working Group, the usual cast of suspects. So it was Rothenberg, the project manager, the Science Institute people, and so on. These guys knew what was going on. [Rodger] Doxsey knew what was going on. But there wasn't the general perception in the project; there were other problems in aberration that they were having to worry about. So there was a feeling that the acquisitions were going okay. Well, they weren't going okay.

There'd be different spins put on it. When the Ball people would get up and talk about HRS acquisitions, they were all successful. When Sally Heap would get up and talk about HRS acquisitions, they were all failures. They were both right. The Ball guy would say, "We got some data because the point spread function was so fuzzy that some of the light fell into slits," even though they'd be off. Sally was right because the most of the star wasn't falling into the aperture. Also, it depends upon what you mean by success. So there's lot of games being played, and it was relatively recently that they started working on this seriously to fix things. So that's okay.

The second thing was --

SMITH: What would happen to your program and how --

BLESS: Yes, we're still not sure about that. The reason I said AGN's [Active Galactic Nuclei] was that the whole point was to be able to use the four-tenths arc second aperture to discriminate against the surrounding galaxy, which presumably is just a galaxy, to be able to look at the engine at the center, which is where the interesting thing was going on. We can't do that anymore. With this fuzz ball, we're getting light contributed from all sorts of things. So our power, our ability to discriminate the interesting, from the uninteresting is way down. We may try one, we probably will, but our chances of seeing something interesting have gone way down because of that. And roughly, I'm not sure yet what the fraction of the program we will be able to do, but one-half to two-thirds may still be a reasonable guess.

SMITH: But it has a much longer --

BLESS: We don't suffer so much from through-put good because we decided not to use a four tenths arc second aperture. At least for now we're not using a four tenths aperture. One-half second aperture you're getting most of the light through, so through-put problem isn't as bad. But we are much more susceptible to any kind of jitter because the image spills over the edge or up to the edge. It spills over the edge of a one arc second aperture, and those images aren't symmetric, so any motion of the star in the aperture and you're get a different signal. And we're still dealing with that problem. So the jitter comes along and really adds to the problems. For a long time during this OV/SV thing, -- as these problems came to light, and the seriousness became more apparent, and the implications became more apparent -- I was getting pretty depressed about the whole thing. I think I reached my low point in the early part of this summer.

SMITH: That's the summer of '91.

BLESS: Summer '91. I was thinking that we may not get a damn thing out of this. But things started to look up. The acquisitions worked better, and Goddard was successful in damping out some of the jitter, not all of it, but it's not nearly as bad as it used to be, but it certainly affects us. So it began to look as if maybe we can see, be able to do something. Today for the first time, I was at a meeting like this, and I was actually able to say, "We got some interesting results."

There are still certain things we don't understand.

SMITH: Things going on in the spacecraft?

BLESS: That's probably where it is, yes. There may be things going on inside HSP too. We don't understand. Nobody understands it. The data show a periodic one percent level intensity with a variation with a period of the spacecraft. Everything varies with the period of the spacecraft. So that's hard to pin down. There's a secular drift, one-half percent over an hour, and there are some drop outs. Those are interesting, but they don't cause us too much problem. But the other two are difficult to understand, and people at the Institute are looking at this.

Apart from that, you can, to a certain degree, take out the wiggle from the data. The instrument is behaving beautifully, and the source of noise is simply photon statistics, so we're doing about as well as you can do. Again, you weep at the thought of what could have been had we had a good image. -- With good images, the jitter wouldn't have mattered because everything would have been concentrated within a about a two tenths of a second arc diameter inside a one arc second aperture. Even if it wandered around in there, there would be no over spilling. It would still stay there.

SMITH: It's really a combination of jitter and aperture.

BLESS: Right. I'm a bit more hopeful, now, that we will get something. But it's going to be a long, long trail. And, of course, we have our probable demise coming up in a few years.

SMITH: For the sake of the tape, replacing the HSP with CO-STAR.

BLESS: Right, and an official decision has not been made, but one is going to be made. It's supposed to be made probably in October. They have to make a decision soon because they're spending money on everything, and they can't afford it. So they have to decide what they're going to do. If they go with the way people think they'll go, they'll still be on November '93 repair schedule, which nobody believes.

It's going to be '94, during which they will do a lot of EVA stuff. Replace the solar arrays, replace the gyros, two of which have failed. Replace Wide-Field I with Wide Field II and replace HSP with CO-STAR. And that's a big EVA to do. I'm sort of rambling, but one thing that came up today is the whole question of what to do, how to handle all of this. As you know, there's been a study by the Institute about the best way to fix things. Ideas range from the hilarious to the maybe possible fixes, and they settled on this CO-STAR fix as being the only practicable one. At the same time they have to worry about Wide-Field II because that's what makes ST. With the pretty pictures, Headquarters will declare a victory. Without those pictures they're still in trouble with the press. So we've got to get back to the II, and then they have the follow on instruments to worry about. As usual, money has been taken from the follow on instruments to take care of problems now; both instruments have been pretty badly stolen from. So I don't know how that's going on, what's going on there.

Just this last week at this meeting at JPL, it turns out that JPL has gone way over budget on Wide-field II, and NASA has been told they're going to need twice as much money for next year as they had planned. Apparently, despite all the evidence to the contrary and despite criticisms, suggestions from the science team of Wide-field II, JPL got the idea in their heads that they could align on the ground all the corrective optics for Wide-field II with sufficient precision. You would not need to have to adjust them in orbit with so-called actuators, except for the pick-off mirror, it had just the one. This is hard to believe, but this is what Westphal was saying. Apparently, they spent many millions of dollars on this --making a model and showing how they could achieve this precision despite the fact that -- come on, folks, let's get realistic, you don't want to put all those eggs in that basket and so on. They didn't like this business of using actuators. Jim says that maybe it was the "not invented here" idea or something.

SMITH: I think there was some pressure about, even at the Science Working Group I attended where people were saying you --

BLESS: Yes, yes, yes, but JPL did not want to do it. As I say, maybe that was not invented here. I mean, JPL is not used to working with advice from other people. JPL knows best, apparently. That's essentially what he was saying today. So they're in a pickle, and what apparently came out last week was that the pick-off mirror would be actuated. There would be three wide field chips, two of which would have actuators. There would be one PC chip, I think, with an actuator. The idea is that the pick-off mirror -- there's probably a break point on four chips as opposed to five chips in terms of cost and schedule. That he didn't go into.

SMITH: So you're looking at something with a much reduced field.

BLESS: Indeed, indeed. So here's a major descoping of the primary instrument on Space Telescope that is being done with Level I specs being shredded, with no science input at all. Even [Riccardo] Giacconi implied that this wouldn't have happened if the Science Working Group still existed. Which was interesting.

SMITH: He said that this morning?

BLESS: Yes, I mean not in those words, but that was the implication of what he was saying. So he's unhappy about this and justifiably so; everybody's unhappy about this. How it's going to be

handled and how much passing by various committees will be done, I don't know. But they have to decide soon because they're apparently in real budget problems.

SMITH: When you say "they," you mean Rothenberg has to make the decision on this issue.

BLESS: Right. They have spent millions of dollars on this exercise.

SMITH: Exercise of the JPL --

BLESS: Fixing it. Screwing things down on the ground.

SMITH: So they don't have to do it again.

BLESS: Once and for all. So it's again the science capabilities are being sacrificed, etc. But also worse than that, the whole process is just --

SMITH: That's one of the things I meant to ask and that is, what is the project structure? I mean, who is running things? Is there anybody running things, even a formal sense? Also, in sort of actual reality of day-to-day decisions that get made, how do those get made?

BLESS: I guess it depends what particular aspect you're talking about. The Institute is supposed to handle the day-to-day operations, but Goddard is also involved. It's a fuzzy --

SMITH: Would you have a sense of it?

BLESS: No. In fact, Riccardo [Giacconi] has been mentioned several times today in that he's been criticized or the Institute's been criticized because it's become too bureaucratic, too high-balanced and so on. He hasn't argued with that. He also wants to debureaucratize the system in a way that helps the Institute, which is perfectly understandable. So the Institute is a very large organization. It's very hard to find out who is responsible for what, or who knows what.

All of a sudden we found out about this guy, Roberto Gilmozzi, who turns out to be a very useful guy who knows about how the pointing control system works. He's written some reports on this. We didn't know those reports existed. I didn't even know that this guy existed until two weeks ago at the RT meeting here when somebody pulled out one of these reports and said, "Ah, that's very interesting." He was looking at the effects of the breathing, the wide-field camera image doing this with the orbital period. We were wondering if maybe that's causing our little wiggle in the signal and so on. He's looked at things like that.

SMITH: This guy, what's his name?

BLESS: Roberto Gilmozzi. He's involved with the pointing control system. So the Institute's very large, and it's very hard to know who to talk to. Particularly since Science Working Group dissolved, we don't have any pipeline to information. In response to this problem, Giacconi asked if we need -- more meeting. This was our first one today, and indeed it was useful. Everybody thought it was very useful. Very informal.

SMITH: This was just the scientists basically?

BLESS: GTOs. It was GTOs and Science Institute people, including both Giacconi and [Peter] Stockman. So they had their big guns there, and they were listening. It was a nice rational discussion. Very useful. So I think we'll do it again.

But no, the project is big, diffuse. You know, Doug Broome's illness is complicating things. [Ed] Weiler is acting program manager, and all these things that had to be done. Nobody apparently controls JPL. It's really incredible.

So JPL, was off on this tangent refusing to look into the actuator business. Lots of money has been spent. The second generation instruments have been robbed blind. I don't know what they're doing. And Rothenberg has got himself a real mess. CO-STAR is charging a million a month or something like that, so they've got to do something quick about that if they were going to stop that. But I think they're not going to stop that. Then there's the problem of HRS, which -- depending upon who you talk to and what spin is put on it -- is serious or is not serious. So things are not very happy at the moment, I think.

SMITH: There's no sense of formal decision making process of which, say, the Science Team are a part at this stage.

BLESS: No, I mean, at the review of JPL last week, Jim took pains to point out that as far as he knew, they were privy to all the meetings that took place. The Wide-Field II Science Team, of which Jim is now a member. I'm not sure if he's an official member, but anyway he's a member.

SMITH: I was out there two months ago, and I think that John Trauger said that Jim was now a part of that team.

BLESS: You see even, Jim had trouble making JPL do what he wanted to do. But he had the clout of being a Caltech professor and not a JPL employee. Caltech, in some sense or other, runs JPL, in a very loose contractual sense, so he had some clout. Also, he screams. Whereas Trauger is a very quiet sort, a JPL employee, and he's had very little clout with them. So they went off and did this, and Trauger was unable to stop it. Nor did anybody in NASA. So from what Jim says -- and I've heard this from Jay Gallagher, who was also at the meeting and who's also a Wide-Field II member -- the amount of money varies, but they blew a lot of money, an inordinate amount of money on this study with nothing to show for it, apparently. To implement it apparently would cost -- that's where the factor of two comes from according to Joe's schedule, and Giacconi says, "Why not cancel Wide-Field II?" At this point, such a mess. That's really not the real world because NASA needs a camera desperately to take pretty pictures. So that was the highlight, I this morning or lowlight.

SMITH: Did you schedule another meeting?

BLESS: No, we didn't schedule one in detail, but the plan is to have them just ahead of the Users Committee and STIG meetings. The Users Committee is tomorrow. So I guess that's probably what they'll do. Giacconi asked how'd this go, and everybody said they were quite pleased. It was one of the better meetings.

SMITH: Are you still a member of the Users Committee?

BLESS: No. I fortunately drew a one year straw. So I'm not.

SMITH: Art Davidson is still the chair?

BLESS: Apparently, yes. Whether they're going to elect a new one or not, I don't know. A guy from the high speed photometer, Robinson, is there, and I was a representative of HSP, but as a GTO representative. That's the way they divide them: GO/GTO. He'll be there, so I'll have that pipeline. The Users Committee was a waste of time last year partly, I think, because a lot of Users Committee effectiveness depends on the chairman, and Davidson was up to his eyeballs with ASTRO, and he just couldn't pay attention to this. So it was an unfortunate combination of things. I hope in the future that they're more effective.

SMITH: I was wondering about Rothenberg to be defined in a positive light.

BLESS: Yes, and he is. He's good. He understands things at a technical level which is refreshing among the project managers. He also has a good feeling for the big picture. Giacconi, until recently anyhow, until these recent developments, said very nice things about him. I knew him slightly back in the old OAO days. He goes way back to that. He's also very straight forward and honest. But he had inherited one hell of a mess. As apparently every project manager does. [Laughs] So I feel sorry for him, but I'm sure he's doing what he thinks is the best job he can do.

There are people at Goddard who want to build up this big test facility, and they want to spend millions of dollars on this and their people to use with second generation instruments. Millions of dollars. Shoot, I mean, a hundred million dollar instruments they're talking about. Incredible things. And some of these they had to descope. They've got the refurbishment problem and this EVA. Talk about being in the public eye! This is going to be on television all the time, and the thing has to work. I mean, Rothenberg's neck is really on the block.

SMITH: Do you feel there's much of a change in mentalities and attitudes from the Marshall people? Do you feel that there's been a significant shift or not in these two areas? Now that we've got a management program in the air on the project, does it look the same or not to you?

BLESS: It was, again with the possible exception of this latest thing, and I'm not sure how much to blame -- I mean, the project certainly deserves some blame on this for not paying more attention to JPL, and they didn't handle this very well and this reaction -- I don't know what Joe's plans are to pass this by the Science community. Maybe he will, maybe he won't. I don't know. I assume he will.

But that aside, Joe is much more approachable. He's willing to talk to you. He's much more open. It was a rather different flavor than the Marshall people, with the exception of [Jim] Odom, used to give at Science Working Group. Sometimes he talks about budget, he talks about the problems he has, and as I say, he is unusual in that he has a pretty good knowledge of many of the details, as well as a reasonable picture of the overall, the idea of the overall picture. I think that everybody was quite favorably impressed.

But this JPL mess, it's really bad. Jim Westphal says that's the way they've always been. He says, "On Voyager they didn't have a science team, they had an imaging team." Once they had a set of requirements on paper, they'd tell the astronomers to go away. And they did. And they did it all by themselves, and they apparently [have] an institutional confidence or arrogance in that, "We don't need anybody to tell us how to do these things." So that has led to a real mess.

SMITH: Maybe the last thing particularly appropriate would be this business we were talking about earlier. Were we making the change? You said that no way that's going on right now again.

BLESS: Yes, that's what I mean. Part of the thing is this business. Wide-Field II is being redesigned, apparently for technical and budgetary reasons. Right now. You see, Weiler wants to have all of the decisions made. ESA is going to do the arrays, and they're going to pay for it because there's not enough money to go to Lockheed to get the fixed arrays. Twenty million bucks, something like that. ESA's going to do that. And ESA's going to let NASA look over their shoulders for the test program because NASA feels burned on that. He wants to have yea or nay on CO-STAR, Wide-Field II, exactly what that's going to be. He wants all that ready to present to Fiske by the 1st of October, and he hopes to get it through [Richard] Truly by the 15th. And they've got to work with ESA, they've got to work with the project, with JPL, everybody. So they're running like hell trying to get this thing done. Then these bombs explode like the JPL overrun. It's been made clear that ST is on a zero sum gain. They see the money they got, or they have the money they see, and that's it. And Bower has to make these decisions, get these decisions made because pretty soon the money will be gone. Their flexibility will be gone.

So there's a lot happening right now, and by another month we ought to know or have some idea of what the shape of the next couple of years is going to be. But [Chuckles] it's just one crisis, another crisis.

SMITH: Did you look back on the history?

BLESS: Oh, of course you tend to focus on all the bad, all the crises, but yes, you'd hope after launch that things would settle down. People are getting data, things are working. There are problems, but you work around them and so on. You get out of this. But we're right back. We've had a year and a half of difficulties and problems with the operations, and some achievements. And now for the first refurbishment mission, we're back in a crisis mode.

SMITH: Sorry we're running out of tape. Must turn off the machine.

I received this 3 years
ago, promptly misplaced
it, and only recently found
and corrected it (as best I
can after 14 years!)

Sorry,

Bob Olsen