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Dr. Bahcall briefly reviews Phase B reduction of the projected scope of the Space Telescope. He reviews early concepts for an infra-red instrument for ST, as well as the final proposed IR instrument designs. Bahcall discusses European participation in ST science through the Faint Object Camera, as well as US scientists' reactions to that participation. He recreates discussion of emerging CCD technology as a potential detector for the Wide Field Camera, and recounts the decisions which went into final selection of the CALTECH CCD proposal. Bahcall concludes with a thumbnail sketch of the origins of other SIs and a discussion of difficulties associated with achieving the limiting magnitude required by the ST fine guidance guide star system.

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ORAL HISTORY INTERVIEW

With: Dr. John H. Bahcall
By: Dr. Paul Hanle
Date: March 28, 1984
Place: Institute for Advanced Study, Princeton University

Dr. Paul Hanle: This is Tape No. One, Side No. One of interview with John Bahcall on March 28, 1984 in the Institute for Advanced Study, E Building. It is the concluding interview in which we will discuss the development of instruments for the Space Telescope.

The original proposal for the Space Telescope, as it evolved in the LST Working Group that you were in, had fife instruments, plus the Fine Guidance System, on a 3-meter telescope until 1974. And then discussion began about reducing the size of the thing to 2.4 meters eventually, and four instruments. Do you remember in this discussion of reducing the parameters, did you ever consider going to fewer instruments even than four? Was there a sort of bottom-line telescope that you were afraid NASA would foist on you?

Bahcall: Yes. Certainly, we considered our minimum configuration that we thought that was viable, and in fact, I defined for myself a minimum configuration which was sufficiently interesting for me to continue in the program.

Hanle: What was it?

Bahcall: In terms of science, it was 2.4 meters. Now, we did discuss, and I am sure Bob O'Dell's files are completed on that, the core instruments.

Hanle: Yes.

Bahcall: And that was a concept which in Phase B we took very seriously, and debated a lot and considered carefully.

Hanle: Do you remember what the core instruments were? O'Dell's files aren't so clear on that.

Bahcall: Yes. Well, the core instruments as finally approved were the Wide Field Camera and the Faint Object Spectrograph. And I pushed for in our final meetings, and previous meetings, to add to that an infrared detector.

Hanle: What happened?

Bahcall: I lost on that one.

Hanle: Yes. What happened to the infrared instrument?

There was some discussion on it.

Bahcall: We had throughout Phase B a very good proposal for an infrared instrument. It was a state of the art instrument that was an instrument which could have been built. It was very ably defended by Gary Neugebauer.

Hanle: Who was on the committee then, was he not?

Bahcall: He was on this working Group in Phase B. Gary did not propose in Phase C-D. Part of that was his involvement as principal investigator on the IRAS satellite. But also, part came about because he did not get, he says, support from JPL administration to help him in the proposal the way the Wide Field Camera did. During the time in which proposals were being prepared for Phase C and D by instruments teams, I did a lot of lobbying to try to stir up an infrared proposal. I talked with Gary a lot. I talked with Caltech people a lot. I talked with Harvard people a lot. I talked with my friend, Mike Hauser. There eventually were infrared proposals, but not at the level which there would have been if Gary had made the proposal.

Hanle: Yes. Was there one from, you said, Harvard, the Center for Astrophysics?

Bahcall: Yes.

Hanle: Was there a Kleiman?

Bahcall: Doug certainly participated in that, I think that -- I don't know who the PI was. I know that Mike Hauser was a member of that.

Hanle: So that was proposed?

Bahcall: Yes. It was proposed, but none of the infrared proposals got a high ranking. And I felt that, from the start, you have four decades of wavelength that are accessible to you with the Space Telescope and three-quarters of those four decades are not covered with the present instrumentation.

I felt that it was essential to cover three of the four decades, and in fact, I thought it was easy to make an argument whereby you would be more likely to make discoveries in those three decades than in the decade that we are covering, simply because in the Rayleigh Jeans limit, where the intensity of light is roughly proportional to the square of the frequency, if something's temperature or its characteristic frequency is much higher than you can see in the visible region, there will nevertheless carry over to the lower frequency region in the visible being down in its intensity, just by power law, by the square of the frequency. Whereas if most of the characteristic energy is in the

infrared region, then it's exponentially cut off in the visible high frequency region, compared to the exponential. cool objects are cut off exponentially, whereas hot objects get fainter, only like the square of the frequency, so there is a real asymmetry. So you could argue that things that we could never have imagined might be there in the infrared, and not get here to the visible. Whereas, anything that is the brightest of the very hot objects will in fact appear in the visible. So that was sort of the qualitative argument behind what I felt was just the seat-of-the-pants argument to suggest that there was really a gold mine out there. And I thought it was tragic and extraordinary that we don't have an infrared instrument. I did everything I could in the interim period to get those guys to come up with a proposal. They did but some of the best guys didn't propose.

Hanle: How badly did NASA want an IR instrument?

Bahcall: I don't know. But I think the decisions were made based on the technical characteristics of the instruments, and the proposals weren't as good.

Hanle: Was it an embarrassment either to you or to NASA that ultimately no IR instrument appeared on the telescope? I say embarrassment, because IR was used in selling Space Telescope quite explicitly.

Bahcall: I guess a minor embarrassment. It was nothing that I felt I had to hide my head about.

Hanle: No, I don't mean you personally. I mean you in the Working Group.

Bahcall: I don't think anybody else felt embarrassed about it. I don't remember NASA saying -- I think they just chose the best instruments. And I felt that the community let itself down. The infrared community let itself down.

Hanle: One of our interviewees pointed out that without the IR you were losing a great deal of the reason for the existence of Space Telescope in the first place.

Bahcall: I believe that.

Hanle: And he personally felt some loss in the quality.

Bahcall: There certainly is a loss in the quality of the science.

Hanle: So, all of the instruments were judged on the basis of the quality of the science, and they were in competition with one another. Is that right?

Bahcall: Yes, to the best of my knowledge.

Hanle: Could you tell me a little bit about the process by which they were selected. I don't know that.

Bahcall: I'm not sure that I know the details. I was in Israel at the time, and there were, I think, ad hoc groups which judged the instrument capabilities and judged the science and the proposals. Those were NASA-organized committees. I believe that played a significant role in that, among others. I just happen to remember that he was one of the people involved in that. It was hard to get people without a conflict of interest. So many people in so many institutions were involved in bidding.

Hanle: Yes, I'm sure.

Bahcall: I don't know the details. Bob would know it.

Hanle: Let me pass along to the European participation, which came out finally to be the Faint Object Camera. How early were the European photon-counting detectors considered for the Space Telescope?

Bahcall: Not very early, because I remember they made a serious -- Their proposal, even, at the end of Phase B (at which time the American instruments were all well defined and rethought, capable of being designed and then built -- they underestimated the difficulty) nevertheless had been studied carefully. At that point there really wasn't a European proposal. What there was, was an interest in an instrument.

And I think the decisive argument was made by Bob O'Dell, who said we can get an extra instrument aboard Space Telescope, an instrument that none of us regard as essential, not a core instrument, and which would probably not otherwise fly, by including the Europeans. Let's go ahead and get that for-free instrument. That was Bob's argument.

Hanle: Do you know if he was aware of the pressure from Headquarters especially to get the Europeans involved?

Bahcall: Oh, I'm sure he was.

Hanle: I guess what I mean is: Did you and the LST Working Group become more concerned with European participation after Congress directed it?

Bahcall: Oh yes.

Hanle: Were the detectors that Boxenburg developed considered to be one of the great advantages of having the instrument on board Space Telescope? Were you aware of that?

Bahcall: As I remember, the feeling was that the Boxenburg detectors were examples of what you could do, but were too

bulky, too requiring of personal service, also, of an expert, to be capable of being developed as flight instruments. They were big and they required an expert: namely, Boxenburg, to use them at the state that we were discussing. Boxenburg was regarded as a great Guru; his instruments were not regarded as flight instruments.

Hanle: If someone told you that the Europeans felt that the Americans had come to them for this instrument because they were worried about not being able to develop their own, the Americans' own camera, what would you say? Does that sound likely? First of all, is it true? And secondly, would the Europeans have felt that way?

Bahcall: Let me answer the questions in the reverse order. I think it's plausible that the Europeans might have said that. It is incorrect. I think it's not true that we couldn't have done our own, but it is not implausible for reasons of pride or whatever else that people have different viewpoints about that. And so I'm not surprised if they really thought that way. I don't remember whether they thought or said that at the time, but I would not be surprised if they did. But I don't think it was accurate, if that was the perception.

Hanle: At one point the Faint Object Spectrograph was considered for the Europeans to produce. How did that fit into the Faint Object Camera picture?

Bahcall: I dimly remember that some of us, myself included, felt that it was unconscionable to give to the Europeans either the Faint Object Spectrograph or the Wide Field Camera, because those were the two core instruments, and I felt that if we were going to spend such a large amount of money on a major new instrument, we ought to keep in this country the most important instruments, because they stimulated industry, stimulated the science, and produced the greatest science. I was opposed to us providing to the Europeans at great cost to ourselves the choice opportunities. I remember arguing that I had no problem with the Europeans winning any competition for any of the instruments. I had a problem with assigning to them any of the instruments. That was always my position, and so I wouldn't have been unhappy if they had made the best proposal for a wide field camera and won that, because they built the best one. But I was opposed to them having either the wide field camera or the faint object spectrograph as an administrative decision prior to the scientific decision of who had the best proposal. I was won over by Bob's argument. I think it was Bob's argument, that we couldn't possibly afford, and we wouldn't choose a faint object camera in addition to a wide field camera; therefore, we would lose nothing by granting it to the Europeans ahead of time, and taking our teams out of competition for that instrument. And

I was won over to that.

Hanle: What about the rest of the Working Group? Did they have similar feelings as yours or was yours a unique position?

Bahcall: I don't remember.

Hanle: Was there a lot of discussion about this among you?

Bahcall: I don't remember. I remember feeling very strongly about this inappropriateness of giving away, without a scientific competition, any of the core instruments. And I argued as effectively and as forcefully as I could on that point. And eventually that was the decision. I've no idea whether my argument was helpful or not helpful. I don't remember the details.

Hanle: There was also about that time some discussion about guaranteed time for the Europeans, based on a fraction of the technology that they would provide, the fraction being about 15 percent, and the technology being an instrument. O'Dell put himself on record against giving a dedicated fraction to the Europeans. Was there a discussion about that among the Working Group?

Bahcall: I'm sure there was. That discussion I don't remember as much.

Hanle: Did they have it dedicated?

Bahcall: Oh yes, and I think larger than the fraction of their contribution. They do get 15 percent, cutoff at the start, independent of the quality of their proposals.

Hanle: And that's in addition to any proposals that they might put in in the competitive guest observer program.

Bahcall: That's right, so in appearance, they could have 100%. The rest of the world could at most have 85% whereas the Europeans could have 100%.

Hanle: And you feel that that's serious, as you say?

Bahcall: I thought it wouldn't be serious at the 5% level, but the 15% level, I think, there is just a tremendous amount of science at the 15% level.

Now, it may well be that the Europeans will qualify on the basis of their scientific contributions for much more than 15%. That's not unreasonable on the basis of their contribution to world astronomy. But you know, there are a lot of groups which aren't included in prior arrangements: Japanese, the Chinese, the Russians, and good groups elsewhere. I don't know, but whatever it is, it is.

Hanle: How are they going to choose to distribute among their 15%?

Bahcall: Ultimately, I don't know. They've decided to really divide up into small pieces their guaranteed time; that is, the guaranteed time for specific proposals that occurs at the beginning of the observational period.

Hanle: This is off the instruments, and I want to get back to them, for one last question: Is there a danger that the best science will come in as guest observer proposals, and the 15%, because it's guaranteed, will come in as lower quality science?

Bahcall: I don't know. I would hope the bookkeeping was such that the first 15% was the best 15% was the guaranteed 15% at all times. And after that it had to compete. But as you took the top 15%, the European proposals which were of the highest quality, you accepted until they were equal to 15% of the total available time, after which the stuff that was at 16% and below competed against the best non-European proposals anywhere. I would hope that that would be the case.

Hanle; Has that rule been set? And who decides?

Bahcall: I suspect that that's the sort of thing which Neta's group has to ultimately establish policies for.

Hanle: Neta Bahcall.

Bahcall: Yes.

Hanle: Did the prospect of including the planetary astronomers with their detector technology get discussed before 1975 at the LST Working Group, that is, before doubts were expressed about the Princeton SEC vidicon?

Bahcall: I don't remember.

Hanle: Do you recall any discussions explicitly about bringing in the planetary people into the Wide Field Camera?

Bahcall: Do you mean specifically with respect to the Princeton proposal, or in general?

Hanle: In general.

Bahcall: I guess the answer is, I don't remember in either case.

Hanle: I wondered if this issue of getting planetary people on board the Space Telescope was ever discussed explicitly,

even later?

Bahcall: I'd be surprised if it wasn't, but I don't remember it.

Hanle: You wrote Bob O'Dell in May of '76 that you wouldn't attend the ST Working Group. That was the time when there were funding problems, and you said, we scientists have finished our work. That was also the meeting in '76 when the SEC vidcon of Princeton's role in it was discussed at some length. I wondered if you participated in the discussion either before or after?

Bahcall: I don't remember. First of all, to give you the background on it, I prefer science to meetings any time.

Hanle: Yes.

Bahcall: Strongly. And I might also, by that time, felt that the SEC vidicon was not a state of the art instrument. And I might very well have felt that it was just coincidentally useful to me not to be present in a discussion of it. But I can't remember. It would have been characteristic of me to be not unhappy at having an excuse not to be present, but I can't remember.

Hanle: Was film ever a serious contender to replace the SEC vidicon?

Bahcall: Serious, I don't know; but it was certainly discussed as a contender.

Hanle: I've seen some notes that pointed out problems with it, but also some advantages, potential advantages to it. Was a study done on it?

Bahcall: I don't remember a major study, but advantages and disadvantages were sufficiently availabe on the surface, I think, so that people could make a decision.

Hanle: And so the Working Group looked at it, and did the Working Group write it off as a possibility? Did they dismiss it?

Bahcall: Again, I don't remember an absolute specific decision of that nature, but it's my impression that we did. And the decisive reason was that you just couldn't revisit the facility often enough to make it a practical method for taking data.

Hanle: You could scan film up there and send it down as digital data.

Bahcall: That's right. And that was discussed, and it was

felt that it wasn't reliable or practical, or whatever. I don't remember the details.

Hanle: One thing I've noticed in the Working Group notes: they said that it would be possible; there is military technology that is done already; and this could be checked into. And I do know that Hinners initiated a study in late '75 to consider it, but I don't know whether that led to dismissal of it or not.

Bahcall: Again, in issues like that, it hinged upon military considerations. I suspect that Bob looked into them himself. He had all the proper clearances, and found out where the military was going. It may well be that the military was going to CCDs. I'm sure they were.

Hanle: Wehre were CCDs being developed, aside from the military, in it for science?

Bahcall: I don't believe they were at that time. And certainly, I didn't know about them, and I believe that astronomers came to them certainly before particle physicists.

Hanle: Yes. There was something called an ICCD being supported at Goddard, which I believe meant improved.

Bahcall: I don't know that. I remember that. I don't even remember what the initals stand for now. But they were in quite an experimental stage.

Hanle: Yes. Stanley Sobiesky was the instrumentation scientist.

Bahcall: Yes.

Hanle: And responsible for those and some other things, too. The question is, do you recall his role in developing the idea of the wide field camera detector, if any?

Bahcall: I don't know. I don't remember any particulars of Stan's role. I would not be surprised if he had some major role. What it is I am not able to say.

Hanle: You mentioned, but sort of indirectly. Did Princeton consider CCDs on their camera?

Bahcall: There was a meeting at Caltech in the new library, where we spent a good bit of time discussing CCDs and SEC vidicons and other detectors, after which I told Lyman that I could not longer support the Princeton proposal without a CCD.

Hanle: Without a CCD.

Bahcall: I told him that I felt it was decisive, advantages to the CCDs.

Hanle: Do you know about when this meeting happened?

Bahcall: We had a Working Group meeting at Caltech. He and I went for a walk in the Atheneum parking lot, and I told him then.

Hanle: So this was a meeting in which the CCDs were raised in the LST Working Group?

Bahcall: Yes, CCDs and the SEC vidicon performance was discussed in the Working Group.

Hanle: I see.

Bahcall: I think maybe it was maybe our only working group meeting at Caltech. It was in the new library there, and afterwards between the time everybody went out to dinner and the time one of the meetings was over, Lyman and I went for a walk in the parking lot, and I told him that was my position.

Hanle: I think Princeton may have considered some CCDs at one point.

Bahcall: I believe that the final Princeton proposal was a hybrid proposal which did have, in addition to the SEC vidicon, some CCD detectors.

Hanle: Yes. They would have been used for different purposes on the same camera.

Bahcall: That could be. I don't remember the details.

Hanle: Let's go back to a little bit earlier. Was the Princeton SEC vidicon considered a shoo-in in the early thinking, in about '74 and '75, when the camera was being defined?

Bahcall: By whom?

Hanle: NASA, which had been supporting the SEC vidicon for about 10 years.

Bahcall: I don't remember discussing it with any people at NASA. I think most people in the astronomical communtiy probably assumed that it would go to Princeton because of our long involvement, and because our great lead in that area. Because of the prominence of some of us, especially Lyman, in the Space Telescope program, I certainly know that it was considered a shoo-in by Princeton people.

Hanle: Who led the group at Princeton?

Bahcall: At that time, Bob Danielson.

Hanle: He did. And I believe he passed away in '75 or '76.

Bahcall: I'm terrible about dates. It was one of the worst days in my life. It was really a tragedy. I loved him. He was courageous, magnanimous, very intelligent, wonderful companion, a lot of fun.

Hanle: He led the development of the detector. Was he over all the leader for the Princeton proposal for a camera up to that point?

Bahcall: He died before the proposal was really submitted. I'm sure that he was the PI in principle for that although I'm not sure if he didn't die before it was necessary to define a PI from the point of view of making a proposal. I think he did. And I think Lyman debated -- that's right. Lyman debated whether Princeton would submit a spectroscopic proposal, an instrument for spectroscopy or an instrument for imagery, and decided, against my advice, to submit a panoramic instrument rather than a spectroscopic instrument. I felt and argued that they should submit a spectroscopic proposal because that was what their expertise lay.

Hanle: Who would have done the work under Spitzer?

Bahcall: Spitzer had a number of people who had been connected with him from the Copernicus Program, and they would have continued: Don Morton; Don York; Ed Jenkins were primary among them. Ted Williams was still here, and I suppose would have been involved also.

Hanle: But I should ask you about your own involvement. As you said, you raised the question of whether there should be an SEC vidicon or CCD on the Princeton camera with Lyman Spitzer. What was Spitzer's response? Then in a discussion.

Bahcall: I don't remember what he said to me. I remember it affected him strongly. I remember it was an emotional discussion. You know, I mean, emotional for the sort of discussion that you have with Lyman. He is not usually emotional. We have had very few discussions which were emotional. Lyman is usually all facts and clear, cool logic. Eventually, Lyman did include a CCD component to his proposal.

Hanle: I believe in the O'Dell files you have gone on record as not supporting the SEC vidicon.

Bahcall: I didn't know that they were in the files. And I'm not sure Lyman knows to what extent I felt that it was

inappropriate, or not the best instrument, but I certainly felt that it was not technologically the best instrument available.

Hanle: So you saw an alternative.

Bahcall: Yes.

Hanle: In the Caltech?

Bahcall: Yes, I was personally supporting the Caltech proposal, although when invited to join the team, I declined to do so.

Hanle: And when did Caltech actually enter as an alternative proposer for the camera?

Bahcall: I believe only after the Announcement of Opportunities.

Hanle: So it was fairly late.

Bahcall: Very late.

Hanle: Was Westphal basically an astronomer or a planetary physicist? Geophysicist?

Bahcall: Westphal is basically a very bright engineer, a brilliant engineer, who made very important contributions in geology and geophysics on the basis of his technical ingenuity; made important contributions in planetary astronomy on the basis of his instrumental abilities; made fundamental contributions in astronomy on the basis of his instrumental developments. He is one of the most original and creative, as well as non-conformist, people anywhere in science.

Hanle: In astronomy.

Bahcall: Yes, in all cases. He was a forerunner of introducing a lot of technology, and a very ingenious guy. Usually, I believe, the scientific ideas as to the applications came from other people, although he continued to have, even at the height of his pressure and involvement in building this wide field camera, to invent things for detecting precursors of lava flow, for example. And he invented a whole set of detectors which were placed around Mt. Vesuvius and so forth. He continued to do that sort of work, just because he was very good at it. This was after he did his 90 hours per week for Space Telescope, he would do that (laughs).

Hanle: So he, in fact, straddled the camps, by being an engineer. He worked on these detectors, he straddled

planetary astronomical and some other geophysical camps.

Bahcall: I believe that's right. I say that he's an engineer. I believe that he has not a Ph.D. in any scientific subject. I don't believe he has a Ph.D. in any subject, but you will never meet a smarter guy.

Hanle: Was he invited to propose, or did he more or less pick it up? How did that happen, do you know?

Bahcall: He claims that both Bob O'Dell and I are responsible in some deep sense for this Caltech proposal. I've heard him say that on occasions, public occasions. Now, in what way that's true, I don't know. I've known Jim since my earliest days of Caltech, and he introduced me to some technical innovations there which at the time he was just introducing them to astronomy. so I always had the greatest admiration for him as a scientist inventor. And it may be that some of us suggested to him, or proposed to him -- I just don't know, and I'm sure that maybe his memory is better, or maybe it isn't, but I don't know what was decisive with him. He said that recently to Riccardo, or to somebody when I was riding in a car, that Bahcall and O'Dell are responsible for the Caltech proposal. But exactly why he says that, I don't know.

I remember being at Caltech and discussing problems with the panoramic detectors with Jim Gunn and with Jim Westphal, and I remember again a conversation with Jim Gunn, which I'm not sure of the exact time frame, where we joked about what Jim could do with the CCD detector that he had, a ground-based CCD detector that he was building. And he told me that the limiting magnitude would be 24th or 25th magnitude, which has been achieved now. And I told him that's wonderful news, but please until the next two or three years say nothing about it until it's absolutely done and completed, until the latest possible time you could announce it, because it can only make difficulties for us in the space telescope, because we lose, instead of being a factor of 100 fainter, we're a factor of 10 fainter. So please don't make that point too clear. And we joked about it. I remember that because that was the major one of the crucial scientific turning points for me, understanding the much greater capability of the CCD detector. I may have gotten it from Westphal or I may have gotten it from Gunn, but the conversation I remember understanding it most clearly was from Gunn. And it may be that that was the turning point and I made a lot of noise about it. I don't know. It wouldn't be uncharacteristic of me, as you see me here, going around and telling people this, what the problems are, and getting people interested in things, but I don't remember.

Hanle: Yes. How did Gunn happen in '76?

Bahcall: How did Gunn have access to a CCD in '76?

Hanle: Yes, or Westphal.

Bahcall: I don't know.

Hanle: Where did they come from?

Bahcall: I don't know.

Hanle: How confident of CCDs were the astronomers in '77 when the instrument was chosen?

Bahcall: Very confident by then. It was demonstrated technology. There were things that were available prior to the choosing of the instrument, were good enough for Space Telescope. Or at least, you could see that they would be good enough for Space Telescope. Also, the military had a lot of experience.

Hanle: Were you aware of it? Or did you simply have suspicion that they had been tried and tested in the military world?

Bahcall: I don't remember the details, but i surely must have known it.

Hanle: And the rest of the Working Group? No, sorry. The Source Evaluation Board surely would have been the one to decide.

Bahcall: Yes, the Source Evaluation Board would certainly have been very knowledgeable about this. And I presume had access to whatever they needed.

Hanle: In general, the appearance of the thing is that the CCD detector and camera that was accepted is far more advanced technology, far closer to the frontier of technology than any of the other instruments, or in fact, any of the other parts of the Space Telescope. It's not conservative innovation, as NASA usually defines it. Do you agree with that?

Bahcall: With the possible exception of the fine guidance system. But on other aspects, I think that is probably true. For the other scientific instruments, that is true. The fine guidance system is frontier technology.

Hanle: Yes. The fine guidance system, however, had no alternative, whereas, there were other proposals for the camera.

Bahcall: Yes, that's right.

Hanle: And so, I'm wondering why in this instance NASA

departed from its usual doctrine?

Bahcall: I wasn't part of the Source Evaluation Board, but I understood that the proposal was so excellent in terms of its technology and science that it stood out well ahead of the other proposals. And the Princeton proposal was not competitive. It wasn't even in the second rank of best. It did not do well in the Source Evaluation Board.

Hanle: Do you know who else proposed?

Bahcall: I don't remember.

Hanle: Go to some of the other instruments. Is there anything else about either the Princeton proposal or the wide field camera that I should ask you?

The Faint Object Spectrograph: how did that evolve? And why was Harms selected as the PI? You mentioned it was one of the work horse instruments. I wonder if you could just give sort of a thumbnail sketch of where the FOS came from, where the FOS team came from, and what the thinking about the FOS was?

Bahcall: No, I don't know about that team. I know of nothing about how it evolved. I don't remember whether I had a role in that at all; if so, I have no recollection of it.

Hanle: How did it happen that -- forget the team for a moment -- back to the Working Group -- that the FOS was considered one of the work horse instruments?

Bahcall: I wuld say core instruments. Because it was felt that the two major functions of the telescope were panoramic detection and spectroscopy. And spectroscopy of faint objects, because that's what the telescope, about the most distant objects in the universe. You can argue whether or not that was the correct perception of the greatest scientific contribution of the telescope, but that was perception of those of us who participated, I believe, at the time. Our perception. And the Faint Object Spectrograph was needed for that purpose.

Hanle: Was there simultaneous discussion of a high resolution spectrograph as a complement to the FOS?

Bahcall: There was proposed a high resolution spectrograph.

Tape #1, Side #2

Bahcall: (Tape begins during a sentence) ...But as I remember, it was not a chosen instrument, one of those that were highly recommended. So I was a little surprised, in fact, when the high resolution spectrograph was chosen as one of the flight instruments for this Space Telescope.

Hanle: Surprised because you felt that the science or engineering of that instrument was not as good as some of the others?

Bahcall: I had no judgement on that. I had no doubt that the reviewers selected the best proposals, but coming from when we had studied the problem in terms of the science to be done, and the instruments that could be designed to do that science in Phase B, it was the consensus opinion of the Working Group that this was not one of the preferred instruments. It was a desirable instrument, but not one of the most desirable, as I remember it.

Hanle: But now the infrared instrument was considered to be one of the most desirable, and yet you weren't surprised when that wasn't chosen.

Bahcall: Yes, but that was because I knew of the difficulties of getting together a team of the best infrared astronomers to propose at the time.

Hanle: Was the high resolution spectrograph that much easier technology to develop than infrared?

Bahcall: Well, a priori you would say it is easier. But the infrared detector that was proposed by Neugebauer and his group in Phase B was a state of the art detector. It worked on the ground, so it could have been made to work in space.

Hanle: I'm curious, how Neugebauer's proposal, Phase B, might be compared to the IRAS satellite.

Bahcall: It was entirely different. It was designed for a pointed instrument, which is the Space Telescope, whereas the IRAS is a survey instrument. It doesn't go sit and point at anything for any long time.

Hanle: I see. Is that the only difference, big enough?

Bahcall: Well, that's a big enough.

Hanle: The high resolution spectrograph technology, was that the forte of Goddard? They may have developed such a thing before IUE, I believe.

Bahcall: I don't know.

Hanle: Do you know if Sobiesky had anything to do with the original Goddard proposal?

Bahcall: I don't know.

Hanle: You said that Boggess supported it. Was he reflecting Jack Brandt's interest?

Bahcall: No, Al Boggess was on the Phase B of Space Telescope, and was the proponent for high resolution spectroscopy in that group. I believe he did not propose for Space Telescope as a PI because of his IUE involvement. But I wouldn't be surprised if he wasn't a key person, or the key person in the development of the instrumental ideas. I don't know. He certainly knew the history more than anybody else.

Hanle: Let's move on to the high speed photometer. How did that come into being?

Bahcall: It got in the back door. There was no high speed photometer. There was no photometry represented on the Phase B Working Group. Nobody from photometry proposed.

Bob Bless was on the Working Group in charge of data operation, which was presumably his role in Copernicus, in the Wisconsin OAO proposal.

Bahcall: And Art Code, who was an old photometry buff, was also there. I felt very strongly that there should be a simple instrument, and that there was a lot of good science to do with photometry. Although I wasn't an instrumentalist -- in fact, I never used a photometer myself -- I formed an ad hoc team which I led until I went to Israel, and then Ed Nather.

Hanle: The high speed photometer story: that's really all I wanted to ask. Just a couple of more questions after this.

Bahcall: Yes, it came in the back door. There was no high speed photometer selected. At my initiation, Bob appointed me the head of the ad hoc photometry team. Ed Nather and some other people who knew much more photometry than I did served on that. They essentially wrote a version of the Phase B proposal for a photometer for which I played some role in the scientific ideas, but not in the machine ideas. And although I think, legally I had to sign off, my agreement with Nather was that he would take the full responsibility for the design of the instrument.

Hanle: Why was the idea of the high speed photometer introduced at all?

Bahcall: I felt that it was simple, and that was useful. It was simple and had good science. And it was important to have an instrument aboard which was simple, so it would surely work, and that the science that it could do was very important -- not necessarily the most glamorous, but very important science.

I was also involved at the time in x-ray binaries and other problems where a lot of the work was photometry, so it was natural for me to have thought photometry at that time.

Hanle: Yes. You said simple. Were you worried that perhaps all of the other instruments wouldn't work? Or would only work to a degree and you needed to have one that you could count on?

Bahcall: Well, without being negative, it would certainly seem to me to be an advantage that there was one instrument which seemed so simple that it couldn't fail, and whose principles even a theorist could understand, and whose components a theorist would be basically familiar with.

Hanle: How did Bless enter the picture?

Bahcall: Bless was the PI on the photometry proposal, but I don't remember if Bless played a prominent role in the photometry discussions in Phase B.

Hanle: Bless headed data and operations in Phase B?

Bahcall: Right.

Hanle: And then somehow he ended up PI of the high speed photometer.

Bahcall: He did because he was the PI on the Wisconsin proposal for a high speed photometer.

Hanle: And why did he propose for a high speed photometer?

Bahcall: I guess he had a lot of photometry experience, having played an important role in the previous OAO from Wisconsin.

Hanle: Were there other proposals for a high speed photometer? Did Nather propose?

Bahcall: I urged Nather to propose. I urged Nather also to propose for the data and operations team leader position. He, I believe, did not have an independent proposal on either. I mean, he certainly didn't in the data and operations team leader. I don't even know whether he was a member of any photometry team. I don't remember.

Hanle: One last thing: I don't know that we have on tape the story of how it was found necessary to go to 14-1/4 magnitude stars for the guide star selection system. Would you just quickly say how you --

Bahcall: You should have it. That is an interesting story.

Hanle: Yes.

Bahcall: And those calculations were calculations carried out by the project, and also by Ed Groth, based upon --

Hanle: Do you mean by the project, O'Dell?

Bahcall: Somebody at Marshall presumably under O'Dell's direction. And also by Ed Groth independently, all based on Allen's astrophysical quantities, numbers that appeared there. And Ed Groth described those at one working group meeting that we went to, and at the same time Ray Soneira and I had been working on galaxy models as an outgrowth of another project, models of our galaxy and the star distribution. And I made some calculations on the plane coming back which showed me that our estimates for 85% probably required a much fainter limiting magnitude than what was being discussed by the contractors and by NASA as what had been accepted.

Hanle: You could know this with the back of the envelope calculation?

Bahcall: Yes, it wasn't just back of the envelope, but it was something I could do on the plane, and I did it on the plane.

And then I came back here, and Ray and I discussed it, and Ray wrote a computer program which simulated the fine guidance system. We got new data from Donna Weistroff, and we reanalyzed the original Allen data. We went back to see what data that Allen had analyzed in order to get the numbers that appeared in his standard book of astrophysical quantities, and in fact, substantiated that very clearly that there was a big discrepancy of a magnitude and a half, a factor of four, a really fainter light power that the device had to have in order to do what we had promised to do, and what NASA had accepted as its responsibility to do. And I communicated that in writing and by telephone calls around as it was being developed. And I remember that Bob O'Dell was very skeptical at the beginning, the project had signed contracts on 13 magnitude. Also, there was the standard Allen work which everybody had accepted. And so that was one of the reasons why in one of our reports we had to circulate an analysis of how Allen had made several incorrect steps in the analysis of the old data, although the new data was much better, and I thought much more convincing anyway.

Hanle: Did you let Allen know this?

Bahcall: Yes, and I exchanged letters with Allen. They are in my file. Where Allen admitted his errors, before we had published anything, on the basis of an analysis of what he had done, and what the new data showed.

He thanked me warmly for it, and said our results would appear in his new astrophysical quantities. I haven't checked, but I'm sure it does. And all subsequent measurements, of course, support our results. We had very good arguments by the time we were finished. But I did feel

that the project was ambivalent about our work. For one thing, it seemed to require an enormous investment for the project in order to improve the fine guidance system as defined by the 13th magnitude.

There was embarrassment, because this was a technical error that had been made by the project. So in one sense, the project and Marshall were not happy to have this made public. On the other hand, it had a lot of implications for the telescope. You couldn't go up there and only reach the target that you wanted less than half the time. That would also be a terrible embarrassment. So I had to push to get our papers circulated and published. And in fact, the project would not publish the second one, which had to do with color errors. They are now used by the people who do the derivation of the guide star selection system, the definition of the guide star selection system. That's part of their task. But they tell me that every six months or so they have to xerox them because they were never published by NASA, and send them out to the contractors or to new people involved in the project, because they are just not known.

Hanle: What are color errors?

Bahcall: We investigated a number of effects in our second report, which had to do with smaller effects than the ones we originally investigated. One of them had to do with the color mismatch between the plates on which you took the survey of the sky and the response of the telescope fine guidance system. So a star which had a certain characteristic brightness as seen on the survey plates with one spectral response might be relatively brighter or fainter compared to its companions, as seen by this fine guidance system from the Space Telescope. So some stars which were close to the limit of what you might think was acceptable on the basis of the survey plates, might not be discoverable by the fine guidance system, and lead to problems. Whereas new objects might appear that you would not have programmed the telescope to look for, because of their difference in color match. And so those were small problems, small effects, but we investigated a number of effects like that.

And one which keep coming up which we never published was the effect of binaries on the confusion of the telescope, Ray and I did work out the effects in great detail. But the only evidence that is around is some viewgraphs that I showed at a Working Group meeting.

Hanle: Is there no outlet for that kind of publication other than within NASA?

Bahcall: Oh, I suppose if Ray and I had been energetic about doing it, we would have, or we could have published it some place if we had wanted to. It certainly would be possible to do so now as a technical publication of the Space Telescope Science Institute. But I don't see any need for it, because

first of all, we'll know what's right when the telescope goes up; and second, if anybody has thought about it, they can always call me and ask me. There aren't very many people that worry about it.

Hanle: Once you discovered this and, indeed, it was published or the first major results published, did NASA follow up by changing the specifications?

Bahcall: No and yes. It was very expensive. And that led to continuous interest on the part of myself and many people to the performance of the fine guidance system, in the fear that the fine guidance system would not do the trick, and that cost a lot of money and reprogramming. Eventually the system was improved, so far as we can tell now. That's what we're promised.

But the stakes were too high for NASA ever to -- I think you explained that to me -- have reneged on its formal 13th magnitude commitment. Although right now NASA remains curiously ambivalent on that. There is this highest level of requirement for the Space Telescope, which is a level of requirement that the project must meet, and if it doesn't meet, it has to go to Headquarters and to Congress to explain why it doesn't meet those specifications.

Hanle: They are called the Level One Requirement?

Bahcall: I think so. And I think the only place that a scientific document that is cited to meet those Level One Requirements is the first report that Ray and I made. And there the performance of the fine guidance system so far as its faintness level that it must reach, and confusion level, is defined in terms of that NASA document we wrote. And it says 85% as described in the document we wrote, and that is to distinguish it from 85% as might be calculated from the then-existing scientific literature, the pre-existing scientific literature.

It's one of the few issues on which the scientific community is unanimous, because there is no point in sending up a billion-dollar instrument if you can't look at half the targets that you want to.

Hanle: In this discussion of instruments, it's quite apparent that you had a major role in defining instruments, as one would expect from your position as interdisciplinary scientist.

Bahcall: I was one of many people who had a role.

Hanle: Yes. Do you feel that you made a substantial contribution to defining this, to defining the instruments?

Bahcall: No. Not the real instrumentalists. I think the one thing which I did which was probably unique was to

suggest and push for the mode in which you had several observations, one or more additional observations to the main observation, at a given time, the so-called Serendipity Mode.

Hanle: Yes.

Bahcall: And that was my idea, based on what people did for particle beams. And I didn't see why it wouldn't work just as well for photon beams. And that, I think, probably would not have happened in the environment in which the telescope was developed, with the people that were there, if I hadn't been there; because there was resistance that it wasn't being done that way at that time in optical telescopes.

And none of the people that were on the Working Group at that time were particularly enthusiastic or interested or even thought that was a good idea initially. But that one exception may be a minor exception, probably is a minor exception. I don't think I had a major role. I made some contribution to some of the other instruments.

I think it would be hard to argue the case whether or not there would have been a photometry instrument or not have had a photometry instrument if we hadn't discussed it in Phase B. Why, I wouldn't have been surprised if Bless or Wisconsin hadn't proposed one, even if we hadn't had one discussed in Phase B, but who knows.

It's not a case that you can argue about, so I would say, with the exception of the Serendipity Mode, I did not have a particularly important role.

END OF INTERVIEW of March 28, 1984