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Stockman describes his work as head of the Research Support Branch of the Space Telescope Science Institute (1983-). He begins with a brief review of his academic background, and work at Columbia, with the Air Force, and with Arizona's optical astronomy group. Space Telescope topics discussed include the selection of CCDs as ST's prime camera detector, ground-based use of CCDs, and tube-based detector technology of the 1970s. Topics relating to Stockman's career at STScI RSB include ST mirror decontamination, CCD Quantum Efficiency Hysteresis, STScI relations with ST contractors, Fine Guidance Sensor issues, 'microripple' of the ST primary mirror, second-generation ST scientific instruments, and NASA managerial practices.

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SPACE TELESCOPE ORAL HISTORY

With: Dr. Peter Stockman

By: Dr. Robert Smith

Place: Space Telescope Science Institute

Date: February 15, 1985

Smith: I wonder if I could start by talking a little bit about your earlier career? First of all, when did you decide you'd like to make astronomy your career? I noticed you did physics at Princeton.

Stockman: Yes. I graduated in physics from Princeton and I had not done spectacularly well. I graduated in the middle of my class both in the physics department and Princeton, but I went on to graduate school, as did three-quarters of the class at that period, to some extent driven by the (Vietnam) war which was on at that time, and to some extent, to see in fact if the physical sciences were really where I wanted to be. So I let the decision-making be the acceptance board at a variety of colleges.

I was accepted by Columbia, and found that I very much enjoyed graduate school. It was very different from Princeton. It was much more of a work situation, 8 to 5 work, and I felt more responsible and did much better there. After two years and passing the qualifying examinations, I started working for Bob Novick in the Columbia Astrophysics Lab. He was flying x-ray astronomy sounding rockets at that time, and there were several interesting people working for him then. One was a young professor, Paul Vanden Bout, who is now the director of NRAO, Charlottesville. Another one was Marty Weiskopf, who is now the AXAF Project Scientist at Marshall, and one was Roger Angel, who is now at Steward Observatory, and we'l get back to Roger later, who's involved in very large mirrors for future telescopes both on the ground and in space.

I worked for almost all of those people at one time or another, through my graduate career, and ended up working with Roger for the last two years. I designed and had built a sounding rocket to look at x-rays from Sco X-1, in particular looking for He-like iron line emission. At that time in x-ray astronomy the whole subject was a very premature and very qualitatively understood subject. All the x-ray sources were an enigma. Sco X-1 was even more enigmatic and still remains one of the least well understood, even though it's the brightest source in the sky.

Roger Angel at that time was also becoming involved in optical astronomy with John Landstreet doing polarization observations of white dwarfs, looking for magnetic fields.

He, John Landstreet, and Jack Kemp discovered the first magnetic white dwarfs, which had been predicted because of highly magnetic neutron stars (their existence had been verified by pulsars). It was predicted that white dwarfs should have large fields too, scaled appropriate to their diameter. But, in fact, there was no evidence for fields in white dwarfs, and so this discovery of a small fraction of white dwarfs with magnetic fields was very interesting and caught the attention of astronomy at that time.

I had finished my sounding rocket experience and was writing up my dissertation, and Roger diverted me, as it were, to help him do circular polarimetry of white dwarfs. I worked a bit with him in that area, and also with Angel, Bruce Woodgate, and Bob Kirschner, looking for forbidden iron lines from the Cygnus loop. Later, I graduated and went on to the Air Force Weapons Laboratory, because I had spent four years in ROTC in college and had an obligation to the Air Force.

At the Air Force Weapons Laboratory I worked as a numerical analyst, physicist, as it were. We worked with one-dimensional and two-dimensional magnetohydrodynamics codes with simple x-ray production calculations in them. I wrote some very detailed radiation transfer codes, to better simulate the implosion of magnetic Z pinches, which the Weapons Lab was hoping to use as a bomb simulator instead of conducting underground tests.

I learned a lot about physics, learned how to run large computers and became very interested in them. But after two years, when I was allowed to leave because of the riffing of military personnel two years after the end of the Vietnam War, I talked to Roger Angel. He was then at the University of Arizona. He said, "Sure, come on out here, there's no problem with jobs in astronomy, you'll do very well."

Well, happily I didn't know the true facts at that time, because it was a desperate period indeed for people who were trying to continue on in astronomy. So I went out to University of Arizona, and took about two years to learn about optical astronomy. Up to that point I'd really been an x-ray astronomer. I started by building instruments for Roger and myself, and cleaned up his instrument, which was in sad shape. After four years, I had a number of publications. I would say the two areas where I'd worked the hardest and done the best were in the polarimetric areas. One interesting class of sources combined all my experiences to that date: The AM Herculis objects. These are magnetic white dwarfs in x-ray accretion systems, in which the accreted material falls from a cool red star onto a magnetic white dwarf, shocks up, and it emits x-rays.

These turned out to be astronomically a perfect fit to my experience. I understood the x-rays, understood the magnetohydrodynamics of the accretion flow because of my work in the Air Force. I was also in the right place to measure the primary characteristics of the stars. This degree of polarization was unique in astronomical sources.

So I and other people at Steward did very well in that field as a result. The other astronomical area, was continuing Roger's and Peter Martin's work looking at the polarization in active galactic nuclei, particularly quasars. I did a very large survey of quasars to understand the nature and the existence of their linear polarization, which was usually taken as a sign that they were synchrotron sources. In fact, current models are that they're disks, just like cataclysmic variables, and -- but the survey cinched it. Up to that point, there'd been very rudimentary measurements with poor instruments, and this was a massive survey with an instrument that was ten times beter than anything that had been used before, and it opened and closed the field, as it were, in one shot. Though there were some interesting sidelights that the survey brought out, it closed off the possibility for many types of follow-up. In fact it verified that polarization wasn't a particularly good diagnostic to use.

There are some interesting things one can do with it, and we did to some extent. But in all, that's not the best way to attack those sources.

At that time I finished being a post-doc for Roger, and then wanted to stay at Arizona. I started looking around and was offered a position at Arizona where I would be in charge of instrument development for the telescopes. I took that job, which was essentially a staff job, it wasn't a professorship, and helped them develop a CCD system and also helped them develop a pulse-counting Reticon spectrograph system, and finished the development of a number of television low light TV guiders, which are used in all the University of Arizona telescopes.

That went along well, and it was at the end of that period -- and I continued my astronomical studies during that period of time -- that I began looking for other positions. This was after eight years. And to be honest, the primary motivation was our family's desire to come back to the East. It was generally a very bleak prospect for an optical astronomer on the East Coast, and at that time the Science Institute advertisements caught my eye, and I went to talk to Don Hall, and he was semi-encouragiing.

I asked him, "What kinds of jobs do you have?"

And he described a few jobs, and I sent in a letter asking to become chief of the research support branch or an instrument scientist, presumably with the Wide Field/ Planetary Camera since I had a lot of experience with CCDs (Charge Coupled Devices).

So I was asked in for an interview. This was the first that Riccardo (Giacconi) had seen or heard of me. Don was not in town at the time, and Riccardo called Bob Novick because Bob Novick was an old x-ray astronomy collaborator, to see what I was about. I don't think he trusted the opinion of any optical astronomers. And fortunately, Bob Novick gave me a rave review. I was offered the job about a month and a half or two months later, and accepted it immediately.

At that time this position had been offered to a number of other people, but that didn't bother me. I came early in April and my wife followed about three months later. We've enjoyed it ever since.

Smith: What time in '83 did you join?

Stockman: I came at the end of March in 1983. It was still while the Institute was in Latrobe (Hall) and Rowland (Hall). I was the first member of the research support branch, and it was a very enjoyable time for the Institute because it was such a small group.

It was very tight. People worked late at night. There were many bachelors that hung out together. The whole staff, including secretaries, were working very long hours, in order to catch up to the rest of the project and continue building the Institute. In the Spring of '83, however, the work load began to taper off somewhat. Up to that point my impression is, though one can never tell, that the people were working 60-70 hour weeks. Since then it's been fairly rational as far as the work hours go.

Smith: I wonder about the interest in instrumentation. Were you building your LiF spectrographs for the sounding rockets?

Stockman: Right. Yes. It was a very simple design. Essentially it was ten sheets of crystal panels, each followed by a large proportional counter, and since Sco X-1 is the brightest source in the sky, it forms its own perfect collimator with no required baffling, and its light comes off the crystal panels and into the proportional counters. By rocking the spacecraft back and forth you rock the crystal and get a very high resolution spectrogram of just the very narrow part of the spectrum around the iron lines.

We were hoping to see the helium-like iron line complex. The temperature was about right. It also was a very interesting part of the spectrum in the sense that it has resonance, forbidden, and inter-combinational lines all within a very narrow area, and within the range of the spectrum we could take. However, we got null result and were able to put a stringent limit on the line strength for narrow lines.

Now, it turns out that Sco X-1 has very weak, very broad lines, and that's why we didn't see them. They're extremely broad and extrememly weak.

Smith: So this position here is not anything very unusual for you except coming back to space astronomy after being away from it a while?

Stockman: No, that's right. In fact, I've had to learn a lot about it. There's a very large difference between a sounding rocket and a satellite, just in terms of all the things that need to be in a satellite to insure that it lives for any length of time, whereas a sounding rocket doesn't have problems with thermal stability or power per se, or radiation damage.

But I've always enjoyed the technical side of things. It's been very enjoyable.

Smith: To come back to the CCDs, I wonder when you first became aware of CCDs? You say you were working with them in the seventies at Steward.

Stockman: Well, yes. I'm trying to remember. I believe it was in approximately 1978 when several things happened. At that time the TI CCDs, the prototypes, had just been delivered, and they were touted to give very good performance. Up to that point, very little good performance had been obtained with CCDs. RCA at that time was coming out with a number of prototype devices which looked very encouraging in terms of cost and so forth, and they were the first devices that had the correct format and quantum efficiency for astronomy. We had hoped that early tales of their (RCA) poor performance, in terms of their noise characteristics and the way that they could shift out a low level image were exaggerated. We were hoping that people just didn't know how to build their electronics. As it turns out, that wasn't the case. In fact those particular imagers were indeed poor. But that was what we had started with, and we continued a process of building a camera to use those imagers. By the end of the time I left, the camera systems that we had developed were more or less copies of the ones that were developed at Kitt Peak to run with the Texas Instruments 800 X 800 and Fairchild chips very successfully, and I had great confidence that, when good imagers became available, that we would do well. And we did well. But they're just now taking advantage of those cameras at Steward Observatory.

So basically I left just after handing off the detectors and the instruments; and I didn't really get to reap a lot of benefits from that effort.

Smith: I'm interested in how CCDs got onto Space Telescope in '77. One of the things I'm trying to understand is the general reputation they had in '76, '77, '78?

Stockman: My impression was that they didn't have a very...I mean, I think it was total optimism that got them onto the spacecraft, that in fact the ones that had been built to that date were not satisfactory, basically in terms of noise, their read-out noise, and their charge transfer characteristics. Although that latter area was not particularly well understood, because you only really understand the problems when you can go down to the lowest signal levels, which involves low noise.

And so it was a great leap of faith, although I wasn't involved with them, to hope that the Space Telescope CCDs would achieve such a low noise characteristic.

In fact, the thing that was so encouraging was that Roger Lynds and Steven Marcus had achieved a very low readout noise with the Fairchild devices in 1978-79. That was really the thing that triggered us, and made us realize that CCD images were going to happen. Up to that point, there was a natural reluctance to believe that one could make a device that could read out at the 10, 15 electron level. The Fairchild CCDs showed that in fact such a device was very practical.

The Fairchild images had other problems. Their electrode structure and geometry were not really appropriate. Half of them were covered with aluminum, and they read out in two fields: they were nasty devices. They had very thick polysilicon electrodes. So in fact their quantum efficiency was not superior to image tubes at that point, and we didn't go completely hogwild because of that. But it was understood that if the RCA chips performed or if the TIs ever became available, that we would really be in great shape. And they're just now getting to that point.

Smith: I guess you knew what was going on at Kitt Peak?

Stockman: Yes. Our proximity to Kitt Peak really let us know their successes very early. So we always lagged two or three years behind Kitt Peak in that area. I didn't consider that very bad, actually. And it turned out to be very profitable, because what we ended up doing was copying their system totally.

I made a very pragmatic decision, in the end. We had a certain amount of money and we had to deliver a certain amount of products. There was no way we could continue our development schedule, we had to go into production, so we just copied their system lock, stock, and barrel.

Smith: I wonder about the directions detectors were taking in the seventies? Was there perhaps much less interest in, say, integrating TV detectors than there had been?

Stockman: Yes. Well, there really was a more sophisticated understanding on the side of the astronomers, realizing that certain types of astronomy are benefited by certain types of detectors, and that one should not try to make a universal detector. In fact, it may turn out that there will be such a thing. At that time, it was realized that the real gains were being made by detectors that were tailored for certain operations, and CCDs were perfect for broad band imaging, just looking at the sky like a photographic plate.

So they gained a lot of favor that way, and they clearly had higher quantum efficiency, stability, and linearity than anything around, so they would have succeeded on their own simply for that reason. In fact, high noise RCA chips are still being used at Kitt Peak for that purpose, because they have excellent quantum efficiency, stability, and behavior, perfect for broad band imaging.

Peter Strittmatter, my director at that time, was gung ho to use CCDs for reading out, like an integrating television, the phosphor outputs of image tubes. He wanted to have an image tube setup where you read the phosphor output with a staring CCD. He was a contrarian in this. No one else wanted to do that. It was too simple. And it does lack a sort of elegance.

However, it is a feasible way to use CCDs, and with the proper readout CCD, it's a very good way to go if you want to have an image tube for your front end.

Smith: But they were still reading --

Stockman: --The phosphor. He wasn't going to photon count. He was just going to allow the glow of the phosphor to accumulate a signal in the CCD between readouts. And he would just get a time-integrated picture of what the back end of the image tube had looked like for the last 30 minutes or so. So it was an electronic replacement for putting a photographic plate at the back end of an image tube.

And it is a useful way to go. At the time, the CCD formats were not particularly well matched. They weren't big enough, didn't have enough pixels. And also at the time, well, and still today, image tubes are not total panaceas. They have their own problems. They have funny image profiles. They have dark counts, cosmic ray and ion events.

Strittmatter was in a contratian school, certainly was. I always wanted to go with direct imaging on CCDs but I was willing to do what he wanted to do because he was my boss, and using CCDs as readout devices was a good intermediate goal. There were the Boksenberg people who invented the IPCS, the Image Photon Counting System, which is precursor to the Faint Object Camera. The IPCS is very successful, particularly at the AAT. Then a cheaper linear version of the IPCS, the Schectman Photon Counting System, was in favor at a number of places in the U.S. because it was a very cheap, easy to build, version for linear spectroscopy. And Steward ended up building one, over Peter Strittmatter's disapproval. Even though he hated the concept, we ended up building one that was a democratically designed instrument, performed well but cost a fortune in the end. So much for cheap.

Smith: I wonder about the SEC Vidicon? NASA had put money into it for the Large Space Telescope program, and then Space Telescope -- whatever happened to that?

Stockman: I don't know. The SEC Vidicon, to my knowledge, was never successfully used for astronomy on the ground. Certainly it hadn't caught on, on the ground. There were SIT Vidicons which I think are similar. To be honest I'm not exactly familiar with the nature of the SEC Vidicon. But there were SIT Vidicons that were used at various places. Their problems were that they had a very limited dynamic range, and they had a real problem with electrical stability, thermal stability, and something that wasn't appreciated early on in the game, the influence of their nonlinear geometric stability, which was dependent on the image that one obtained.

One thing that was discovered with the SIT Vidicons, and I suspect would have been discovered with the SEC Vidicons, was that when one built up an image with peaks in the stored charge signal, the electron beam was slightly attracted to those peaks. This created, in essence, a distortion of the beam as one read out the device, which was very difficult to restore. It was a very nonlinear process, and it worked fine for spectroscopy, but for imaging it was a serious problem. And it might have been a far more catastrophic problem for Space Telescope had that developed.

But I don't believe that was the reason the SEC Vidicon wasn't chosen. I wasn't involved in that selection, but it may not have been chosen because of its lower quantum efficiency, and because it didn't have as high a dynamic range.

For a given exposure, you could only see things within the central range of brightness. The faintest things would be lost, the brightest things would be overexposed, whereas the Wide Field Planetary Camera has a fairly high dynamic range.

Smith: The Apollo 12 astronauts turned their SEC camera to the sun.

Stockman: To the sun, right, that's a perfect example. But there are other examples of that just on the ground, with the SIT Vidicons, but you can only look at something 100 times brighter than the faintest thing. If you get over that, they totally saturate, and the signal is essentially lost.

Smith: I wonder if you were following developments on Space Telescope, for example, if you were aware of things like the Hornig Committee?

Stockman: No. I was not at all paying attention to politics. In fact, interestingly enough, the only relevant conversation I can remember from those days was when Angel came back from the East and said that they were going to set up an Institute and how crazy it was that they were setting up one with a shop in it, because he didn't think that it was necessary. And of course that portion of the institute is exactly one of the things I'm responsible for!

But at the time I wasn't paying any attention to the politics of the ST. All I knew was that Angel had gotten on the Faint Object Spectrograph team and that Ray Weymann had become involved with the High Resolution Spectrograph.

Smith: Was Space Telescope something that might be talked about at lunch time, things like that? A topic of conversation?

Stockman: No. It was something that would come up very intermittently. People didn't understand how the telescope system itself worked basically, so they didn't talk about that. They might talk about the detectors. Peter Strittmatter would complain about the cost. That was the level of discussion.

Smith: OK. Moving forward to the time you came to the Institute, you said that you were the first person in the research support branch.

Stockman: Yes.

Smith: I wonder how many people you have in the branch now?

Stockman: It's hard for me to answer. It's something like 20 people. Predominantly, those are research assistants who live across the hall, and I don't have much managerial responsibility for them other than hiring them and keeping the paychecks flowing and assigning them to people.

I have a secretary, another astronomer, David Soderblom, who's interested in the selection of the second generation of instruments. I have Pierre Bely, who's ESA engineer, who's concerned about the telescope system as a whole. I have Chris Burrows, who's an ESA scientist, whom we've got working in optics design, and analysis. We need to understand what kinds of images will be produced by Space Telescope and how to diagnose them. And Jim Kinsey is working with me as a physicist who is also in charge of the shops downstairs, where we have Bob Denman, an electrical technician, and Anatole Evzerov, who is an opto-mechanical engineer machinist. Anatole is a very interesting person, since he used to work on the six meter telescope in Russia and was the designer of one of its spectrographs.

Smith: I wonder about the kinds of tasks that you have to do in research support?

Stockman: I'm not sure what the origins of the branch were. I believe, back in the old days, before I came, Riccardo wanted to shelter money and people in it to support the research within the Institute. That's why the resarch assistants ended up in it, and we initially had a research publications group which has now been transferred to normal publications. We still have a science photographer which we haven't hired but we're responsible for. And we have a certain amount of budget to cover things like page charges and so forth.

On the other hand, he wanted to have a group to do technologically advanced things, and to advise him technologically. I think he was hoping to get the best technologist he could into the branch to go on to new satellites or new instruments. So it became the responsibility of the branch to advise him technologically as to new projects, also the performance of the Space Telescope as it was.

Since I came, the job evolved a bit. It was probably not anticipated by Riccardo or Don (Hall), that the Institute got more and more embroiled in the systems engineering aspect of Space Telescope, and was, in a sense, a representative of the scientific community in concerns over systems engineering flaws. And so it turned out that many of the upper management levels in the Institute ended up going to many meetings, review panels, engineering review panels, Science Working Group meetings, Orbital Verification Planning, Science Verification Planning, etc. Don delegated a number of his responsibilities in these areas to me: in particular, the Orbital Verification Planning period.

Since then, I've taken on, and my branch has taken on, a much more programmatic responsibility. Instead of being just advisors to the Director, we now actually are getting to the point where we're beginning to have deliverables, just like every other branch in the place. We have action items that come out of these committee meetings and we respond to them. We've taken on the shelter of what are euphemistically called "observatory level tests," which are a series of tests to be performed on orbit during the first six months or so to ascertain the performance of the spacecraft, and not just the instruments.

Smith: I wonder how you got involved in, say, cleaning the mirror...what role you played?

Stockman: Well, it's my impression that when there was a big flap over Space Telescope and the first big schedule slip, they created the the system engineering and end-to-end engineering effort, both at Lockheed and through Headquarters. There was also created at Huntsville a number of review panels which were to review the outputs of the system engineering analysis, and say "yes indeed, we believe these analyses and we believe that the spacecraft is headed towards Level 1 performance. It will meet all its objectives."

The Institute attempted to get people on every one of those panels, at least those for which it had any relevance, and I ended up on a number of them. Since then much of that work is being done by other people in the branch, Pierre Bely and Chris Burrows. I ended up on the FGS panel, on the Optics panel, on the Orbital Verification Planning Team, and I've sat in on the Contamination panel at meetings when [Francesco] Paresce couldn't, though he is our prime representative.

And so something like the mirror cleaning would surface to the top, or was an obvious concern. People had seen the mirror. They had seen that it was dirty. There was a concern apparently that was raised at the Science Working Group, the Project Scientist's Group, then probably -- this is a guess -- got the Project Manager to ask P-E (Perkin-Elmer) to measure how bad it was. P-E came back with a measurement which was worse than they had thought, by a lot, and it was ascertained that dust was a real problem. How were we going to control the dust contamination?

Then there was another review or creation of an implementation plan to contain the whole contamination, part of which was a cleaning of the mirrors. So in these panels, we would be presented with a plan, which involved mirror cleaning, and we would question the technique to make sure it sounded rational, and make sure that P-E had done all its homework. They would generally satisfy us, or come back at the next meeting and satisfy remaining questions; and then they would go do it.

That was the level of involvement. We would not design the method. We were more of a review panel; a last ditch review panel before a decision was made to go ahead.

Smith: So your involvement would be largely review?

Stockman: Yes, that's right. Very little real engineering has gone on at the Institute in that kind of area. Ours is more a storehouse of concerns, as it were, both scientific and technological.

Often times problems will emerge that have scientific ramifications that are not totally understood by the people that find the problem and think in terms of one area. We've gotten involved in a number of these areas. When they had to enlarge the FGS apertures from 3 arc seconds to 5 arc seconds because of the effects of G loading and alignment of the keel, that had a number of implications that we asked about and that finally were resolved by Perkin-Elmer. But initially, there were many questions asked that Perkin-Elmer was not able to answer about that change, which seemed like a simple change.

Similarly, Perkin-Elmer discovered nine months ago that their aspheric collimators were more distorted than they thought they were going to be for the Fine Guidance Sensors. That problem is still being worked out. But that had a number of wide-ranging implications about the operations and science output of the spacecraft, and we would get involved there.

Slowly the entire project is becoming more operationally minded. It appears that the ST project as a whole is becoming more attuned with the scientific goals of the satellite. All of the project, at the same time as the astronomers at the Institute, are becoming more attuned to the operational and engineering consequences of the satellite.

So I do feel that the project's growing together, in terms of communications.

Smith: Do you ever get any requests for assistance from Bob Brown? He's got himself and a very small staff.

Stockman: Yes. Bob will create a list of action items, as it were, for areas where he needs help, and he will just deal those out to whomever is the most relevant scientist. So I tend to get some; that's how I became involved with the Wide Field Camera quantum hysteresis problem. He knew that the Institute would be best served through me, since he was concerned that (John) Hoessel and (Richard) Griffiths would not be able really to carry the ball quite as strongly as he wanted it carried. Since the original QEH meeting, however, Richard Griffiths has taken over the responsibility of tracking that problem. He is also the scientific investigator on the JPL WF/PC clone project (notes added in 1985).

Smith: I wonder too about Bill Fastie's role, I know he's a consultant to the Institute.

Stockman: Yes. I can only speak about what I know about Bill from 1983 on. I know he's been involved with the Space Telescope before that, but you would know better than I what the history of his previous invovlement was. Bill also came up with the initial budgets for my branch, so I know him that way.

Smith: He was acting chief?

Stockman: In a sense he was acting branch chief, but he primarily defined some budgets and so forth, which I changed. He also did one thing which was very useful. He started a program to analyze alternate FGS designs through APL, and that program was a very useful program. I first really spoke with him about that subject.

Since then, I've known him in discussions on the Optics Panel, where he's a member, and we still get involved in Fine Guidance Sensor questions. He's a paid consultant to the Institute, to consult on a variety of areas. One of the areas he's still working on is the Optical Control System, OCS, which is the way that they intend to align the secondary mirror to achieve the optical quality of Space Telescope. He is still trying to study that and come up with a briefing, a description, and areas of concern. It's part of the Space Telescope Observatory that has almost no visibility to the Institute, and is a very important area.

Smith: When you want information from P-E or Lockheed, is it easy to get it?

Stockman: Sometimes it is and sometimes it isn't. In terms of the OCS, it has not been easy. Recently Bill has obtained information on that system.

In terms of the Fine Guidance System calibration software which is one of the areas we've studied, we've been able to develop a relationshjip with Linda Reed at P-E which is a very successful one. But basically it's not easy from P-E; less easy from P-E than it is from Lockheed. I think Lockheed's more free with their documentation that Perkin-Elmer.

However, I must add that I haven't made a large concerted effort to get a body of documents and been frustrated. It's just been in a few instances when I've asked, it's either been a long time or never forthcoming. But it's never been a show stopper.

Tape #1, side 2

Smith: Do you have to work through Marshall if you want information from P-E? Or can you just pick up the telephone and call P-E?

Stockman: If it's someone I know well there, then I'll just call them. Otherwise I'll call Charlie Jones who is the Marshall representative at Perkin-Elmer, and he will try to do something, if it's not a problem.

Smith: I wonder about the Fine Guidance Sensor, because that was something that was very much on the table in early '83. How much of a concern was that and is it still very much of a concern?

Stockman: Well, yes, it is. It's changed its whole complexion. In 1983 the worry was that the interferometric properties of the system would fail, that P-E simply wouldn't be able to make it to specifications. That would have been a disaster. If, for some reason, the inherent design of the system or its construction kept it from being a proper interferometer, it would result in a catastrophic failure of the system. It's not a robust design, from that point of view. That is, it just doesn't work well at all, if it's not interferometrically almost perfect.

In fact, when P-E was initially making the Koester's prisms, the prisms weren't very good interferometers. It was also shown analytically, first by Jim Westphal, and later by a consultant working for Bill Fastie, that the refractive element group in that design had to be designed to incredible tolerances, about five, ten times better than Perkin-Elmer allowed.

P-E finally acknowledged that and had to go into a wholly different method for constructing those groups, in order to obtain the tolerances that were necessary. As a result, for six months to a year the Project feared that they simply wouldn't be able to do it. Finally, P-E was able to get good S curves, good interferometric fringes, with the whole optical train, the refractive element group and the Koester's prisms. So we all heaved a sigh of relief, thinking this is it, this thing will work.

Well, since then, they've not only had manufacturing problems, but they've had really serious problems in the area of alignment and desorption. One of the problems that was not apparent to us at that time was that the FGS finally creates an image of the star very near the photomultiplier. The image is very tiny, and passes through a small masking aperture on the photomultiplier. That image is the result of the beam passing through the Koester's prism and bouncing off a number of fold mirrors and so forth. And if any of these elements warp or move, the image of the star will move off of the aperture. If there were only one photomultiplier, it would be all right, but since there are four, they all must have the image centered in their little apertures, or it's a catastrophe. If one of them loses that image because of the fold mirror bending or something of that sort, then the system simply does not work at all. Thus the structure must be very stable, and it must be capable of being aligned prior to launch and maintain that alignment through lauch, and subsequent desorption of the FGS structure.

Well, when P-E tried a G loading test -- after aligning it in one orientation, they turned it 180 degrees -- they discovered that the images all fell out of the apertures. Or one of them did anyway. Subsequent analysis showed that one of their structural supports for one of the bend mirrors was flimsy, and they fixed that.

They also found that their keel was not as stiff as it was supposed to be, and I don't think they still understand quite why that is.

But most recently, in the first vacuum tests, the images again fell outside the apertures, because of the warpage due to desorption of water from the graphite-epoxy.

After a certain amount of time, something like 24 hours, the images drifted back into the apertures. But this was not very comforting. Our confidence that the system would work was severely shaken, since for the first day or two after launch it simply wouldn't work. However, if everything is right and no bolts slip or anything of that sort, everything will finally come back into alignment. It seems almost fortuitous, because with water desorption out of the structure, the geometry changes, not in a perfectly geometric sense. Everything doesn't just scale smaller, because there's a sense of orientation to each piece of graphite-epoxy given by the layers of graphite mesh. So each piece will distort and relax in a different way I don't know if that problem's been resolved.

But what's interesting is that that problem came up and submerged again within Marshall, with relatively little communication to the scientists, the Institute, the Science Working Group, anyone else.

Smith: For a while it was visible, but then it got lost?

Stockman: It poked its head up just long enough for them to say, "Well, we had this experience, that the images went out and then came back in again a couple of days later," and then it fell down, at least from my perspective, and didn't gain any more attention. And I fear that, in fact, they're having problems with it.

So I would not say we're out of the woods.

They haven't really demonstrated a robust, completely flight-qualified, vacuum-tested model yet, as far as I know.

Smith: They have the engineering model?

Stockman: That's got through, that's right. But I think they're having problems at every stage now. And as they perform more delicate tests, they find more problems.

They also found recently a color term problem, which they do not understand, apparently. If they change the color of their source, the images shift in the FGS, which would be terrible for the astrometers, just terrible.

Smith: I remember you made a remark to me once, when we were coming back from Marshall, about the FGS being a kind of Cadillac of the system.

Stockman: Yes.

Smith: I wonder if you could expand on that a little bit? It seems to have been very complicated. Perhaps a simpler one could well have done the job?

Stockman: Well, looking back on it now, after the full engineering that's gone into that system, it would appear to me as if a simple quadrant detector system, similar to the one proposed by Fastie, would have been a more robust, simpler system. However, the way they've done everything up to the Koester's prism is great. I mean, that was nice design work, and I don't know that one could do much better than that. But the promised added performance of the interferometer is not realized in practise. That is, a quadrant detector would have just as good a noise performance. At the time when the interferometer was proposed, they hadn't done a full engineering study. Now, perhaps when one did a full engineering study of the quadrant detector, similar flaws could be found. It doesn't appear so. They hadn't folded through, to my knowledge, the fact that the light was polychromatic. They hadn't folded through, to my knowledge, the detailed performance obtained with the overlapping primary pupil. And they hadn't folded in the manufacturing tolerances either, at that time.

So it seems to me that a quadrant detector would have been much preferable, much sturdier. But I know more now than I did at the time I talked to you. The Cadillac part -- I don't feel quite that it's a Cadillac. Only part of it is a Cadillac. They were hoping to get a factor of two better performance, noise equivalent angle, but other than for astrometry, the realized performance imporvement is not significant for operations. The FGS portion of the error budget is a minor part of the budget for pointing.

Smith: I remember the first time I think the Science Working Group saw a picture of the Fine Guidance System, at a Science Working Group meeting at Marshall, there were gasps from the working group for a short time. They hadn't realized the level of complexity. That's what took them aback.

Stockman: Well, as I said, I can remember conversations of people trying to describe what the Fine Guidance System was, back at University of Arizona. And they all got it wrong. They were incapable of actually describing how the Fine Guidance Sensor worked. I can remember as recently as 1983, Jim Westphal coming out here and trying to explain how it worked, and he had it totally wrong. And he's a fairly clever man. So it never was realy well understood.

Now, it is actually fairly well understood by three or four bodies of people. The PASS software people have some understanding. I think the Institute has a very good understanding of how it works. APL, Applied Physics Laboratory, has a good understanding of how it works. And of course, there is P-E. That's a certain understanding that I think won't be lost. We'll be able to hold onto that.

But I can't say the same thing about the OCS.

Smith: Right. Just going back a little bit, the mirror and the dust problem -- I've heard about what's been described as a "microripple."

Stockman: Yes.

Smith: I see the claim by P-E that it's the best mirror ever, and then I hear about the microripple.

Stockman: Can I rationalize those two questions? Well, it's something that an optical astronomer wouldn't even think about. At the microscopic level -- well, it's not totally microscopic, the millimeter level -- you can have a ripple in the mirror which is caused by the polishing method, of the order of only tens of angstroms of hills and valleys of order millimeters to centimeters in length. The effect of the ripple is related to its size compared to the wavelength of the light that you're using. At optical wavelengths, 5000 angstroms, the amount of light lost goes roughly as the square of 15 to 20 angstroms ripple over 5000, which is almost nothing. So for optical astronomers, it is not a serious problem. They don't even worry about microripple.

It's effectively like many out of phase diffraction gratings. When wavelengths get down to the size of the ripple, the ripple begins diffracting beams out into various orders of the grating, and once it begins diffracting into the first order, it turns out that the mirror is diffracting seriously in the second, third, fourth order.

So, at 1000 angstroms, in the ultraviolet, it's now a serious problem, because the effect goes as something like 4 pi times the ripple size divided by the wavelength squared. So, in fact, a large fraction of the light is diffracted out by microripple into its side lobes, into the first order, second order, and so forth. The fundamental effect is, that the light is removed from the main central image into a variety of orders, and the orders also have different scale lengths, depending on whether the ripple is on a centimeter scale or tens of centimeter scales or hundreds of centimeter scales. For the HST mirror, 30 percent of the central light at Lyman alpha will be lost from the central beam due to microripple effects. And there's not much one can do about it without improving the polishing. And it's normally not something people think about for optical astronomy because wavelengths are long. It's something that people worry about for ultraviolet astronomy.

Smith: I can see if you're looking for companions to stars it's a problem.

Stockman: Yes. Yes. What you end up with is a diffraction image which still has a very sharp core, and then has very broad wings to it. Blurred out, to some extent. The ripple does that as well as the pointing jitter.

I believe the problem is real. When I talked to Fastie about it, he claimed that the ripple estimate is probably overly pessimistic. But he did also mention at that time, so this is third hand, that at the time that the mirror reached its certain level of perfection, whether it would be 50th wave or 60th wave, there was a decision not to go further with trying to polish out the errors, because at some point polishing generates more ripple than it does smooth out the overall figure of the mirror. So at some point it's not worth continuing the polishing. You'll make the figure better but you'll generate more ripple than you want to have. You're constantly lapping the same area and generating more little pits and valleys and so forth, at the microscopic field.

Smith: I imagine that P-E would not have to worry about ultraviolet performance.

Stockman: Right. To the extent that the ripples on many centimeter wavelengths, between ripples, telescopes such as IUE, which don't have particularly good imaging properties because thy're small mirrors and fairly large apertures, wouldn't notice that kind of ripple. They would have noticed ripple at the millimeter wavelength because that would throw out light from their beam and they would have noticed the lack of efficiency. But ripple at centimeter wavelengths or longer, of amplitude tens of angstroms, they would not see, because their image quality is never that good, whereas in

Space Telescope you will see that.

Smith: OK, on the primary, I've noticed there are three fixtures.

Stockman: Right.

Smith: Are those related to the OCS?

Stockman: No, they're related to the mounting. Those are the hard points for the telescope. Those are the basic mounts for the telescope. I asked somebody why they went all the way through the mirror, and that was apparently done just out of very conservative design, that they wanted to sandwich the mirror between two metal pads with maybe some stickinesss or epoxy or something, so that they wouldn't worry about lauch stresses. They wanted to distribute lauch stresses as evenly as they could. And if you make a pad too big on the back, then you might create deformations in the mirror by the adhesion of the pad to the mirror. On the other hand, you may get a local stress at launch in the mirror which might crack it. So they wanted to make the pads big enough, and then they decided to distribute the stress top and bottom, so they minimize the stress with also a minimum of global deformations in the mirror.

Smith: I wonder about the status of the CCDs, with the hysteresis problem. Is the physics of that any closer to being understood?

Stockman: Well, I don't know. Closer than what? The last I heard, was that the physics were understood. Well, it was still not clear whether the solution to the problem was generated by illumination of light which creates electrons within the CCD, which pop out the back and somehow stick to the backside, or whether the solution to the problem is generated by flooding with UV light which disassociates atoms or makes, say, negative molecules in the environs of the CCD, which then stick to the back.

So that physics is not understood at all well, although the latest theory is that the UV flood dissociates molecules and creates negative molecules sticking to the back of the CCD.

There's also been recently an interesting finding that those molecules in fact migrate. That's another supportive evidence for this concept of molecules sticking to the back, that over time the charge bleeds off more or less uniformly off the CCD and goes off to the edge or somewhere. It just leaves the back side like a current.

The actual problem within the bulk of the CCD that causes the problem is understood in a very qualitative way. That is, it's clear that there's a well generated near the back that traps potential signal electrons. But where exactly that well is, and how does the size of that well and its filling depend on incoming light, I don't believe that's well understood by anybody.

But all they're trying to do is get rid of it by sticking on a bunch of charge on the back.

Smith: I wonder if there's developed, for instance, an Institute position on issues like this. Say Mr. Welch decided, well, we won't do anything this time, but we'll fix a clone WF/PC.

Stockman: Yes --

Smith: Now, that clearly would mean that there would be a science impact.

Stockman: Yes.

Smith: I wonder if the Institute has a position on that?

Stockman: We do. Riccardo (Giaconni) makes the recommendation in these cases, and has done, and he's recommended that in fact, barring danger to the spacecraft, that the problem should be fixed. He feels it's that serious. And that position was arrived at through a lot of scientific impact studies, descriptions to Riccardo of what the problem was, presentations to Bob Brown about what the problem was.

It was a massive educational experience for Bob Brown, finding out why it was a problem. He learned a lot about astronomy at that point, about stellar astronomy.

Smith: And this would be a process in which you'd be actively involved, doing these studies?

Stockman: Yes, that's right. We would do it, as well as outside scientists associated with the project. All the scientists in the Institute are potential advisors on it.

We discussed the problem with those who had been involved with the STAC (Space Telescope Advisory Committee) proceedings, workshop proceedings, and asked each one of those what they thought the impact would be on their area. They came back with impact statements; why their area was important; and why it would make a difference; and what performance was required for their area. First we briefed Bob Brown on this, and then he chose those that were the most serious impacts and the best science and used those to brief (Jim) Odom and (Jim) Welch.

I think it's going to be approved. I mean, at the moment the current fix is through the WF/PC radiator. Therefore, it will be an internal Wide Field/Planetary Camera fix and they won't have to look through the aperture door. That's almost sure. And the cost will be somewhere between three and four million dollars.

Smith: It saves the red --

Stockman: It saves the blue, not ultraviolet, but blue, 4000 angstroms to 5000 angstroms was the major region affected. Also, I think it will save the entire spectral region because I don't think that they ever did really estimate what the real effect was. They never did a proper test to show that it was limited purely to the blue. I don't think they ever really explored how bad it could get in the red. But that's my personal opinion. That was one of my recommendations.

Smith: I wonder, to finish up, how different you found working with NASA than working at Steward?

Stockman: Well, it was very different for me, and it wasn't just NASA. At Steward where I had been, I had the mentality of a post-doctoral student and I had let my own influence and interests be confined. And when I came here, because the Institute was so open and there were so many interesting things to do, Riccardo encouraged people to do what they wanted. Particularly, he encouraged them to achieve any level they want. If they want to pretend that they're on the level of the director, in a sense, so much the better, if they're able to pull it off. He's willing that people take a very active lead in anything.

So that really helped open my horizons, and I just totally adapted to this way of life, and in that sense, NASA's not a shock.

The major area that I had not been at all aware of in my vision of NASA was that I had not understood how businesslike and contractual things are with NASA. The fact that NASA at Goddard itself does very little other than manage contracts. That was a revelation. I had no idea. I thought that all of these vehicles and satellites were put together by civil servants, more or less, when the truth is quite the opposite. The thing that's put together by civil servants is a set of contracts and reviews of management plans. Many of those management plans and goals are not even necessarily set by NASA themselves but by "Beltway Bandits," as consultants to NASA.

I had no idea that was the case. And I can understand it from a business point of view, a governmental point of view, but it makes understanding how NASA arrives at its scientific goals, those it wishes to support, less understandable to me. I don't understand how that happens, truly. I better understand it at NASA Headquarters than I do at Goddard.

Smith: One final question. I wonder about the ability of the Science Institute staff to bring forward good second generation instruments, things you want to propose. Could you actually do that?

Stockman: Yes. That's happeneing. I mean, I'm not doing it, Richard Griffiths wishes to build a second generation CCD camera to take the place of the Wide Field Planetary Camera, and he wants to use a Tectronics large format CCD, which has not been built but which is in the developmental stage, as the prime detector. He has put in a letter of intent and will submit a proposal to do that.

He will choose an outside contractor, and any major effort will be done in collaboration with Johns Hopkins.

My understanding is that NASA Headquarters and the AURA board will permit a maximum of approximately five people on that project to work within the Institute, as AURA employees. Beyond that, they have to be Johns Hopkins employees.

Smith: NASA doesn't mind the fact that something's being --

Stockman: No. No. Well, there were two things that NASA didn't want to have happen. It didn't want an element of the Space Telescope to be responsible, and have built into their statement of work that they would provide instruments. I agree with that and so does Riccardo, because that is not always the best way to achieve the best instrument. It tends to get a very conservative, low risk instrument, or a very wild difficult instrument out, depending on who happens to be running the shop at that time.

And it doesn't take full advantage of the peer review system.

The other thing they didn't want was to build a big building and hire a lot of people to support that effort. So they didn't want to formalize a commitment by the Institution to build an instrument and therefore have NASA obliged to pay those personnel to do that.

As long as we want to do it through an external contractor and win the proposal in the way that a university would win it -- fine, no problem.

Smith: There always seemed to be this tension between the astronomical community and NASA, on this point of what facilities to put at the Institute.

Stockman: Yes. Yes. Well, that's because the astronomical community is more attuned to places like NRAO and Kitt Peak, where there are in-house instrument development groups that are part of the structure of those areas. And in some cases, they're very successful. But they are also exceedingly expensive, and don't always come up with the state of the art. They generally lag the state of the art.

Smith: Perhaps better to have competition --

Stockman: Better to have a competition, flush out the best ones, and then fund those, than it is to institutionalize the development.

Smith: Fine.

Stockman: Thank you very much.