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EDITED ORAL HISTORY TRANSCRIPT

NOEL W. HINNERS
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is August 18th, 2010. This interview is being conducted with Dr. Noel Hinners in Littleton, Colorado for the NASA Headquarters History Office Oral History Project. Interviewer is Rebecca Wright. Thank you again for letting me come to your house this morning and visit with you for this project. I'd like to start today's conversation by talking about when you were nearing the end of your studies in 1962. You interviewed with a new company named Bellcomm [Inc.]. This company had been formed to provide technical support to NASA Headquarters [Washington, DC]. Tell us why you decided to join this company and about your first tasks there, and what your role was.

HINNERS: We need to go back probably to earlier college years when I was figuring out what I wanted to do in life. I still haven't totally figured it out, but we'll get there. I started out at Rutgers [University, New Brunswick, New Jersey] in 1954 with the intent of being a chicken farmer. I went into the ag [agricultural] research program at Rutgers, which was full of science courses—biology, chemistry, physics, soil microbiology, poultry problems 202—neat things like that, and got interested in geology. So I took all the geology courses as electives. I ended up with as many as the majors did. Really, really liked geology. I said, "This looks fun," so when I graduated, I decided to go to grad [graduate] school in geology, geochemistry. I liked chemistry also.

So I went out to Caltech [California Institute of Technology] in Pasadena, got my master's out there. I intended to get a doctorate, but Caltech was such a hotbed of faculty turmoil at the time I said, "No way am I staying here." Got into geochemistry there, and really liked it with the faculty there, many of whom had come from University of Chicago [Illinois]. Harrison Brown, Jerry [G. J.] Wasserburg were into meteorite studies. That of course says origin of the solar system, the universe, etc.—the small problems in life. Intriguing and interesting stuff. That got me pegged on early solar system history, history of the universe.

I went from there to Princeton [University, New Jersey], majoring in geochemistry. Got into mineralogy, ore deposit formations—but kept an interest in the space things, the meteorites, etc. All this was during the time when the space program was starting to pick up. Of course while I was at Princeton, '60 to '63, was the period in which the commitment was made for the Apollo program by [President John F.] Kennedy. But that did not *a priori* tell me, "You should go into the space program." I was still doing a lot and had interest in the geology end of life, and had worked during the summer for one of the oil companies. Mobil [then Socony Mobil Oil Company] had sent me down to South America the summer of '58. Did everything from going out on the well sites in Venezuela to the laboratories, really got a good feel for what the oil companies do. I said, "I think this could also be an interesting career."

When it came time to think about graduating, my first thought was I'll get a postdoc [postdoctoral fellowship]; the perennial student. This is after nine years in college coming up. I had gotten married the year before in '62 and had a baby on the way. So Diana says, "Enough of this school stuff. Why don't you get a real job?" That got me out of the postdoc arena, which was a good thing. I did look at two career paths at that point. The oil companies; I went out for an interview in Bartlesville, Oklahoma [headquarters of Phillips Petroleum Company]. If you've

ever been to Bartlesville, it does not look like the green east coast. And then I interviewed with Bellcomm in Washington [DC]. This was early ’63, or it may have been late ’62.

I got linked to them through my thesis adviser, whose brother-in-law was working for Bellcomm. Bellcomm was an offshoot of AT&T, set up at NASA’s request in ’62 to provide systems engineering support to NASA Headquarters. Went out, had an interview down there at Bellcomm, and it all sounded like exciting stuff, and I liked the people I had interviewed there. The contrast was Bartlesville, Oklahoma or Washington, DC. Plugging around the oil rigs and looking at little microfossils or thinking about helping going to the Moon. Not only that, but Bellcomm was going to pay me $600 a year more than the oil company. That obviously was not a swinger.

WRIGHT: What were your thoughts of what systems engineering support would be?

HINNERS: I had no foggy notion of what systems engineering was. When I interviewed at Bellcomm I interviewed with a group that was focusing on working with NASA to identify and certify the Apollo landing sites using the data and the missions of Ranger and Surveyor [spacecraft]. They had figured we need at least a couple geologists who understand maybe what the Moon is all about to work on these programs. So I joined that group there, which had another geologist in it—this was in summer of ’63—and immediately got involved with the NASA Headquarters folks in the Apollo Lunar Exploration Office and the Surveyor program out at JPL [Jet Propulsion Laboratory, Pasadena, California].

You’ve got to realize Bellcomm was not an experimental laboratory organization. It was a people organization supplying talent to Headquarters to conduct studies, analyze issues and

problems, look at what the NASA programs were doing, critique those. We were in a strange role in a sense. I didn't realize at the time that it was so unusual, but in retrospect it was very unusual. That in itself created some early problems. You're well familiar with the Johnson Space Center [JSC]. The last thing Johnson ever wanted or needed was more insight and advice from NASA Headquarters and their cronies. Well, it took a couple of years before Bellcomm people were accepted by the JSC mafia. I can understand that. "What are these outsiders doing? What do they know? We're the experts." So you had to prove yourself and become an accepted part of the crew there, the management crew. Of course Headquarters has always been looked somewhat suspiciously at by the Centers. We all know the usual tete-a-tete that goes on between the field and NASA Headquarters.

That was all pretty well overcome, largely I think because we had people who, number one, weren't trying to pretend that they were running the show, but were working with them to help define and ensure that we were getting, in our case, the data that would support the Apollo landings. Working with Headquarters, Ben [Benjamin] Milwitzky was one of the prime ones. I don't know what his actual role was, but he was deeply involved in the Surveyor program at Jet Propulsion Lab. So, I had initially a number of trips out to JPL, started to understand the Surveyor system, what it was going to do, and I got involved in the lunar soil mechanics experiment, which was being run by a PI [principal investigator], Ron [Ronald F.] Scott from Caltech. The Surveyors had designed into them a scoop to go down, touch the lunar surface, muck around with it, try to understand its soil mechanics properties as they might relate to the lunar module landing pads and disturbance of the lunar surface by the engines on the touchdown. I got to know and worked with Ron very closely during that period.

At the same time that was going on, NASA and JPL were conducting lunar roving vehicle studies, robotic roving vehicles. The thought was to send one on a Surveyor platform—taking off some of the experiments—put a rover on top, and deploy that rover. So there were two studies, one with Bendix [Corporation] and one with General Motors [Company (GM)], who each had competing little roving vehicle designs. One of the reasons I remember this is we had a review of the General Motors design out at their Santa Barbara research center out in California, and there on a Friday afternoon a secretary burst into our conference room and said, "Just heard news, Kennedy has been shot."

Everybody remembers where they were when they first heard that. That's where I was, out at the GM research labs there, thinking about lunar roving vehicles. That was an incredible time. Nobody knew what was going on—whether it was a big international plot, whether it was a coup—just didn't know. Airports shut down for a while. You didn't know how you were going to get home, what was going on. That has stayed with me, just a marker in the progress. I realize that Kennedy started all this—in retrospect, [President Lyndon B.] Johnson had been the prime architect of an awful lot of the NASA organization program. Continuity obviously kept on going there. It was one of those early markers, things you just don't forget.

The roving vehicles on Surveyor did not last much longer. In fact the early Ranger and Surveyor series were much more extensive than that which was flown. They were much shortened after those programs, which had started off as science programs, and became Apollo support programs. Then it was obvious you don't need an infinite supply of Surveyors and Rangers. Some small number should do it.

In that period NASA initiated the Lunar Orbiter program to do the orbital site surveys, which was run out of [NASA] Langley [Research Center, Hampton, Virginia]. So we at

Bellcomm got involved in the Orbiter program. A number of the people in our group there worked on developing the specs [specifications] for the camera systems. Although some of that was preordained—a lot of that is public now, but I want to be careful because I don't know if any of it is still classified. It was a film system, very very ingenious. Obviously had its derivations in the black world [classified]. Photography coming out of the Orbiter program was phenomenal.

At Bellcomm we got involved in helping design the targeting for the Lunar Orbiter where it would take potential candidate site photography for the Apollo landing missions. The Orbiter program turned out to be incredibly successful, as was Surveyor. Ranger had its ups and downs, the program coming close to cancellation after the first six [failed]. [Rangers] 7, 8 and 9 succeeded, which provided some very interesting data. There had been early on a few people, namely Tommy [Thomas] Gold of Cornell [University, Ithaca, New York], who talked about this very deep layer of low bearing-strength dust on the lunar surface. He was convinced that there was the potential for a lunar module to "sink out of sight."

As geologists, we couldn't understand how you could build up what was then being called the "fairy castle structure" on the lunar surface. Just didn't compute. Although yes, who knows what happens in the vacuum there on the Moon. Then when the Ranger photos came in, let's see, that is a big rock sitting calmly on the surface and not sinking out of sight. So thus anybody in his right mind would conclude that the bearing strength of the lunar surface was not an issue. It could hold on to hundreds or thousands of pounds rocks. What's the problem? Most of us dismissed that concern although Tommy Gold kept at it for a number of years. We did have to deal with him.

George [E.] Mueller, the head of what was then called manned spaceflight, had an advisory group of scientists to help him understand some of the lunar science and the factors involved in landing. Tommy Gold was on that crew so one could never totally get rid of Tommy and his shenanigans. Interestingly, when post-Apollo people tried to pin him down he said, "Oh, I never said that." We could not document it. He never wrote it down. It was all conversation at meetings.

WRIGHT: How different that is from now with 24-hour broadcasts.

HINNERS: Absolutely. That gives you some feel for the things that people were dreaming up. It was right to do that. "What are all the hazards you can think of that could impact the Apollo program?" I got involved in one. There was a fellow who came to NASA Headquarters for a year or two, Leonard [Len] Reiffel. He was initially I believe from Illinois. May have been IITRI, Illinois Institute of Technology Research Institute. Very smart fellow. I think he had a physics background.

One of the potential problems that came up was the possibility of lunar pyrophorics. "Hm, pyrophorics, what the hell is that?" Pyro you'll recognize as like pyromaniac, set fire. The concept or the thought was you've got solar wind impinging directly on the lunar surface. Solar wind is mainly hydrogen. Hydrogen is a known reducing agent. So could that hydrogen become embedded in the lunar surface and chemically reduce the material, that when brought into the oxygen-rich environment of the lunar module could ignite? "Whoa." Is it physically possible? Yes. In theory land, that could happen. You could not prove by any experiment *a priori* that it was impossible.

So Len convened a group to spend a couple days working through thinking about it trying to understand the probability that such could occur. We had a meeting out there in Illinois at IITRI and concluded that no, it's highly unlikely—you couldn't say for sure zero—highly unlikely that this is going to be an issue. I wish I could remember the details of the thinking that went into that, because today I'd say, "Let's see, how did we really assure ourselves that it was such a low probability that we shouldn't sit around worrying about it?"

The whole effort of many of us was to think of things that could possibly go wrong. This was an attribute of the whole Apollo program no matter where you were, to think about things that could go wrong and to do your utmost to document that and work it through and say yes or no, it is or isn't a problem. If it is, here's what we do to try to resolve it or get more data. It was a very systematic and thorough environment in which we were all working. Didn't matter where you were. JPL, JSC, Marshall [Space Flight Center, Huntsville, Alabama], the Cape [Canaveral, Florida], Headquarters—the whole focus was on think of things that might happen and work them off. I think that still is the going way of operating in many of the NASA programs. It pays off.

The Surveyor program had some interesting aspects. As you're probably well aware, the first Surveyor landed. It somewhat caught people by surprise. I think they had been used to the Ranger program where there were so many failures that when Surveyor 1 landed successfully it was, "Oh my goodness." Not quite believing the first time it worked. There was a mindset to be prepared to fail. So when it succeeded on the first try—a delightful surprise. Demonstrated immediately that the Tommy Gold fairy castle structure, if you needed any more evidence, was not a concern.

WRIGHT: Were you with your colleagues when you learned that Surveyor had succeeded?

HINNERS: I was not out at JPL at the time. I don't remember where I was, probably back at Bellcomm. Our offices were there in Washington. That and of course prior to that watching those Ranger pictures come in. You today say, "How could you get so excited watching a bunch of nested pictures come in as you were impacting the Moon?" But at the time that was an amazing feat, which gives you an interesting perspective. We take so many things for granted now that are easy to do. At the time they were small miracles, fantastic. Everything evolves and you become used to doing things in today's environment and forget what a challenge it was 50 years ago.

WRIGHT: Lots of work, lots of thought.

HINNERS: Yes. The Surveyor had some other interesting downstream impacts in the sense that Surveyor 7 landed near the crater Tycho, down in the southern part of the Moon, successfully. The mission worked totally as designed, and had some interesting findings. It indicated from its chemistry experiment that there was a lot of fluorine in the analysis. To this day, we don't understand that, because all the Apollo samples show essentially there's no fluorine in any lunar rock we've looked at that amounts to anything, other than trace amounts.

That's still an enigma today as to why the Surveyor alpha backscatter indicated a high fluorine content by high, I think it was several percent. Volatile. Still not impossible that there's something odd down there. We've seen everything from Apollo and the other missions that said there's no water anywhere on the Moon, and today say, "So sure of that now?" Is it possible

there's something down there? Yes. You just can't totally dismiss it. The Moon may still have some surprises for us.

Back to the Orbiter program. The Lunar Orbiter with its five missions was designed totally for support of the Apollo site certification process. The first three missions were so successful in getting the photography that was needed for the Apollo site surveys that Orbiters 4 and 5 were essentially given over to the science community to use.

So NASA greatly expanded the scope of the photography, now to focus on getting good photography around the Moon for science purposes. Not just for site selection, but for really trying to develop an understanding of lunar geology through the photogeologic studies, a large number of which were done by the [United States] Geological Survey out in Flagstaff [Arizona] and Menlo Park [California]. I should mention that the Geological Survey, through its Astrogeology Branch in Flagstaff, provided a lot of site selection and mission planning support to JSC back in the '60s, led by Gene [Eugene Merle] Shoemaker.

A fantastic bunch of folks there in Flagstaff and Menlo Park. They were really running the geologic mapping of the Moon, a large effort in doing the mapping for the Apollo sites and for science purposes. Gene Shoemaker was a real stimulant. Flagstaff is near Meteor Crater, which is why Gene finagled the headquarters of the Astrogeology Branch there. That turned out to be a prime training site for the Apollo crews, along with places like Hawaii—anyplace that had good volcanics, things that appeared to be analogues for the lunar landing sites. We had a fair number of sessions, field trips up there in the Flagstaff area, Sunset Crater, out on the lava flows to run mission simulations, plan out the tasks for the astronauts.

And train astronauts—most of whom were pilots, a lot of them out of the Air Force—to be geologists. The science job on the lunar surface was to do field geology, as well as to deploy

the experiment packages, the ALSEPs [Apollo Lunar Surface Experiments Packages]. These field trips were right from the start designed to get the crews both interested and good at doing field geology.

During all this time, of course, some fair number of those people in the Astrogeology Branch and others aspired to be astronauts. "Why should we be training all these astronauts to be geologists? Why don't we just send a geologist?" At that time you had to have your fighter pilot and test pilot credentials to even be considered a part of the astronaut corps. Scientists in the human spaceflight program was always at that time a bone of contention among the professional pilots and the others.

Of course the culmination of trying to get a real geologist flying—turned out to be Apollo 17 when Jack [Harrsion H.] Schmitt was selected. Jack had not initially been part of the Apollo 17 crew when the 18, 19, 20, 21 missions got canceled. I think that was a two-step process. Jack, who was, I think, going to be on Apollo 18 at one point, got moved up to 17, replacing one of the crew who had been selected for the Apollo 17 mission. So the last Apollo mission was when they finally got a real live card-carrying geologist to the Moon. Better late than never.

WRIGHT: As planners during those first days, you mentioned that everyone was thinking about what could possibly go wrong so that you could dismiss that or deal with that. How did you start incorporating the objectives of science on the Moon? I know that you said that site selection was of course the first preference. Talk about how science became a part of that as well.

HINNERS: This goes back to some of the early both NASA-sponsored and [the National Academies] Space Studies Board workshops. One of the first major science studies of what you

could do on the Moon was ’62. It was called the Iowa Summer Study. It was named that because it was held by the University of Iowa [Iowa City]. “Hm, strange place,” isn’t it? Any idea why it was there? Ever hear of a guy named Jim [James Alfred] Van Allen?

WRIGHT: Yes.

HINNERS: One of the early stars of Explorer 1 [1958 satellite missions], the Van Allen [radiation] belts. University of Iowa, “Let’s have it at my school.” That’s how it got to be the Iowa Summer Study. There were a number of teams put together to start to consider what could be done scientifically on the Moon, and that laid the foundation for a lot of the work. NASA was deeply involved and sponsored that.

That was followed by a large number of science meetings and workshops, getting even eventually into detailed traverse planning, a lot of which was done internally at NASA. JSC started to build up a group there to work on the science end of it and they formed a Science Directorate at JSC. I think Bill [Wilmot N.] Hess was the first director of the Science Directorate there at JSC. It started to build up an in-house science capability, which was very good. In fact it was close to, in my view, necessary. You might say, “Why is that?” The greatest plus out of having that Science Directorate at JSC was that now science was a piece of the Center. Previously one always felt that scientists were the real outsiders, but having them in house was just fantastic because it just eased the communication. It became an accepted part of the Center, so a great move to do that.

Tony [Anthony J.] Calio took over after Bill Hess. Tony kept up that tradition and built the Science Directorate up some more. They got heavily involved in life sciences as well as the

physical sciences and were instrumental in getting the Lunar Receiving Lab in place and under way. Sometime in that period, the Lunar Science Institute [LSI] was formed [1968]. It went into that old mansion [Jim West Mansion] there just adjacent to the [Johnson Space] Center. Bill [William W.] Rubey was the first director of that. That too was a great adjunct for bringing science into the Center and its environs there. It was independent but worked closely with JSC. The LSI became the general meeting place for all the science meetings in conjunction with the Apollo planning. It's still going strong today—it's the Lunar and Planetary Science Institute now.

WRIGHT: I remember when I was doing some research there seemed to be a tension, or a constant check or double-check of the players involved. They had Headquarters, and they had this new Science Directorate. At first it was underneath the engineering, so there was this growing, evolving department at JSC, you had Bellcomm and you had USGS [US Geological Survey]. At some point you began chairing the meetings and hosted a number of these individuals. How were you able to get the bigger picture looked at and not what everyone wanted?

HINNERS: Well, we'd fight those out at the lowest levels first and then work our way up. There was a very methodical process in place to work on such things as the site selection. At JSC there was the Group for Lunar Exploration Planning, the GLEP. I think Bill Hess and then Tony Calio chaired that. They put together a subgroup to work the specifics of the site selection. For a large part of that I chaired that group to bring the scientists together.

Generally we'd meet down at JSC to work through the site selection criteria, understand the engineering constraints and try to find the science sites that were going to be the most useful. That relied very heavily on the lunar geologists, a good number of them at the Geological Survey, to provide the basic interpretations of the mostly Lunar Orbiter photography. Apollo 11, it didn't matter where that was going to land. It was all new no matter what. We had no science input to speak of for the Apollo 11, because pretty much the goal was to land safely, get back, deploy a package. Turned out to be a preliminary experiment package, not the full-up ALSEP, and collect the rocks nearby the lunar module.

After that, the science criteria became the dominant factor in selecting the sites. As you might guess, the geologists, our group kept pushing the limits on what the engineers were comfortable doing. It was a very dynamic environment, the engineers being super cautious—properly so, I'm not denigrating that. That's their job, to have successful landings first of all. The scientists, we would push to go to the most exciting interesting geologic sites we could think of and find, the peaks of Copernicus, the depths of Hadley Rille, Tycho Crater. Good geology correlates with bad terrain. Good geology, that's where you get the exposures. There was a constant back-and-forth with us backing off. "Okay, we won't pick that site." Then the engineers said, "Yeah, we think we can see how to do it." It was a very dynamic ongoing activity there to bring the two sides to, "Okay, there's a site." Final selections were made at Headquarters. We'd present the results of the site selection deliberations to—first it was Sam [Samuel C.] Phillips who was the Apollo program director at Headquarters. Then Rocco [A.] Petrone took over when Phillips left, soon after Apollo 11. Several good things happened in that period.

The Apollo missions were initially scheduled every two to three months. They were on a rapid fire, because nobody knew which one would be successful. When Apollo 11 was successful the first try, like the Surveyor, "Okay, what do we do now?" So rather soon after, the effort was made and followed through to stretch out the Apollo missions. There was a realization that what you learn from one mission on two-month centers you couldn't possibly feed back into the next mission. Just wasn't enough time to digest and redesign the mission.

So we soon got them off into six-month centers—a little longer I think for some of them—which gave enough time to digest from one mission, replan some aspects of the traverses say on the next mission, but then really impact a couple missions down. It got to be a fairly well oiled process of site selection, mission planning—both through orbital science and the surface science, getting the surface packages together, the ALSEPs. Jim [James W.] Head [III] and Farouk [El-Baz] of Bellcomm played major roles in that science part. Farouk focused largely on the orbital science, working with the crews and the photography from lunar orbit. Jim much more on the surface missions.

During that period, the mid '60s, there were all sorts of studies and plans for the initial post-Apollo lunar missions. In retrospect, these were absolutely wild concepts and thoughts. But at the time, "Hey, we're not going to stop, we're just going to keep going and do more and more and more." One concept was called the MOLAB, the Mobile [Lunar] Laboratory, which would be landed on a lunar module deck. [It was a] roving vehicle, pressurized, that could take astronauts off for hundreds of kilometers to go exploring doing field geology on the lunar surface. I think George Mueller was even looking at—it was called 6-6-8 as I recall. Six lunar modules, six command service modules, and eight Saturn Vs [rockets] a year. It was wild, it was

wild. But all on paper. Then it became evident that the lunar program was not going to be followed by a follow-on major program.

Of course all this in part was dictated by the whole situation in the country. It was incredible turmoil. It's hard to put things in juxtaposition there and make sense out of it. Here we were working on going to the Moon, an unbelievable feat just to even aspire to do it. Riots in the streets, Washington burning. Riots throughout the country. Kent State [University May 4, 1970 shootings]. This doesn't compute, this dichotomy: fantastic accomplishments and chaos. Clearly the whole budget situation and Vietnam [War] had a major impact on the NASA follow-on programs and it became pretty evident there was not going to be a follow-on lunar program.

We looked at using the spare Lunar Orbiter spacecraft—there were six that were built, five flown—we said, "Well, gee, let's use the spare, outfit it with geochemical instruments to do a geochemical survey of the Moon." [We] did some early planning on that, and that fell by the wayside also. Lunar science just went into a data analysis mode. Continued the operation of the ALSEPs for a number of years. The lunar program petered out. Oodles of data, so the data analysis program started up in vigor '72, '73.

That has kept going ever since, and really demonstrated—we're using that today as one of the arguments for Mars sample return, showing how the lunar sample that was brought back is still today, 40 years later, being used to do analyses and experiments that were inconceivable when the samples were brought back. The samples live forever. As you get new instrumentation and techniques, take out some more sample, and have at it, and find out amazing new things on the samples you thought had been sapped of their content. We're using that as part of the rationale for, "Let's get that Mars sample back."

As you're well aware, NASA's near term/far term goal is Mars for human exploration. I've been advocating Mars sample return, as I've been doing for 30 years. You find some interesting reaction in the human spaceflight community. Some are active supporters, and you detect that there's also some reluctance on part of some of the folks who seem to think that one of the reasons to send astronauts to Mars is to collect and bring back Mars material. If you guys have already done it robotically, you've taken away one of our key reasons for going. I counter that with, "I don't think you'd dare go to Mars without having had sample back here," from a safety viewpoint.

There are things in the chemistry of the Mars surface material that appear to give it a very high oxidizing potential. Dangerously high if you were to breathe that stuff. Would you really spend hundreds of billions of dollars to go without understanding the potential risk of the Mars material to a crew? To me it is such a no-brainer, I just shake my head in disbelief that anybody would think of doing it. That's one of the battles I continue to fight. We're actually starting to make some progress on getting a Mars sample return mission under way. Another story though.

WRIGHT: But one in good relation to what we're speaking of, because you had information that you were using to help plan the lunar surface expeditions, which is what you sound like you're trying to do now. You need more information before you can send humans there. And that was the route that was chosen. You had Surveyor, you had Ranger, you had information that was coming back so that you could—

HINNERS: Optimize that human mission, yes. Same holds true. Another aspect of that does in one sense go back to Apollo—Apollo was not done for science. In hindsight, obviously it

wasn't; it was a political decision. The same will be true of Mars. There's a lot of science to be done, but it's not for science. You can do most of the science robotically for a fraction of the price. So you don't send humans because you want them to do science. You send them for all these other reasons, some of which are hard to define in nice concrete terms. But it's part of that exploration ethos. "Let's do it. It's neat. Let's go." It's a peaceful activity of humans. That's an ongoing discussion that pops up every once in a while still.

WRIGHT: Since you're talking about astronauts, how much were they involved in the decisions for those lunar missions?

HINNERS: Quite a bit. The crew office was involved in a lot of site selection. For example, Jack Schmitt was in on a lot of our site selection activity later on that impacted his mission. Jim [James A.] Lovell [Jr.] was chairing GLEP for a while. Jim, after Apollo 13, got very active in working with us on the science world. Was an absolute gem, he is just one super fantastic guy. Along with another fellow who was great, Jack [John R.] Sevier. You've probably heard that name along the way. Jack could also bridge the science and engineering communities.

Having those folks on the inside at JSC was an absolute blessing. It did evolve, with their help, from much less of an adversarial relationship to one of cooperation, with both sides pushing where they had to. It turned out to be in retrospect—and even in real time—just a good working relationship, once everybody got to know one another, what was driving them. Dogmatism would stop against the hard facts of reality. The engineers gave in, scientists gave in. So it was a real good cooperative venture there as the process got tuned on Apollo.

It did not always keep on in subsequent programs. One that became contentious was the Apollo-Soyuz [Test Project (ASTP)]. It was an opportunity to fly some experiments on that flight, and JSC unilaterally decided what experiments would be flown. That caused a bit of a stink, to put it mildly, to go up in the science community. Some of those experiments were frankly not of very good quality. JSC did not understand the whole science mindset process of competition.

The way the best science emerges is when you're competitive. You let people compete on the basis of proposals and what makes most sense in terms of the instrumentation and meeting science goals and objectives. So a hold was put on that and a rapid meeting of the Space Studies Board subgroup was put together to revisit the science payload selection for the ASTP mission. Some of the JSC proposed experiments, as I recall, survived that. There were some additional ones put on. But it was a lesson to us that given their druthers, JSC will revert to trying to do everything in house.

WRIGHT: I'm sure it was easier that way.

HINNERS: It is. It's a human tendency, we all do it.

WRIGHT: There are so many science groups available for making input. They have all these different boards and the National Academies.

HINNERS: Yes. There's no one mind of the infamous science community. It's only a community when an enemy shows up.

WRIGHT: Tell me about some of the missions. You mentioned Apollo 11, that site selection was for them to get there. Then when you thought you had more missions you were hoping to plan out greater and long-lasting experiments, but then of course the program was cut back.

HINNERS: Yes. But there was a tremendous advance made beyond Apollo 11. First there was all sorts of mass margin. [Apollo] 11 was very conservative, for good reason. Then they found, "Hey, we can carry a lot more payload." So the whole concept of putting the roving vehicle on, carrying more science experiments, having a bay of the service module dedicated to remote sensing experiments was implemented. The ideas had been around for quite a while, but they actually then got approved for implementation. And were very well supported by Marshall, JSC and Headquarters.

One thing sticks in my mind. The budget crunches just kept coming after '67. George Mueller chaired something called the Manned Space Flight Experiments Board at Headquarters. That board was responsible for making decisions on what payloads would fly on the Apollo missions—engineering, medical, and the science experiments. Budgets were becoming serious enough that George contemplated canceling the orbital experiments on Apollo 15 through 17. I was still at Bellcomm at that time and got a call from Bill [William T.] O'Bryant, who was heading up the lunar office, just before lunch. He said, "Hey, Noel, we got to go make a presentation to George Mueller as to why we should fly the orbital experiments." So we march over to George and his Manned Space Flight Experiments Board members.

I went on about an hour talking about the great science that we were going to get from the lunar orbital experiments, and at the end of that session George says, "We're going to do it." It

struck me at the time. "Hey, good, okay, super, onwards." It wasn't until maybe decades later as I thought back on it, I said, "Holy smokes." There was no screwing around with committee, subcommittees. George made the decision in real time. Today you'd spend a half year mucking around with groups studying this, groups studying that, analyzing the hell out of it, and maybe eventually making a decision. The decision process was so much cleaner then.

Part of it, the circumstance that you didn't have time to fritter away working something to absolute death. And two, I think the nature of the people. George Mueller was fantastic. He had the capability, the ability, to make decisions. Sum up in his mind everything he had heard and make a decision and here's what we're going to do.

It had interesting implications. You're probably familiar with the all-up testing program for the Saturn V, which was resisted by JSC and Marshall, by [Robert R.] Gilruth and [Wernher] von Braun, and probably [Kurt H.] Debus at the Cape. I don't remember for sure; I read about it, I wasn't there. George made absolutely clear to them, "This isn't for debate, we're going to do it this way." Giving orders to Gilruth and von Braun, you better be damn sure of yourself. You're fighting two big personalities there who are used to getting their own ways. That was remarkable. And later on they both admitted that was the right decision.

WRIGHT: Because you learned so much from that.

HINNERS: [We] never would have made it; never would have made the '69 date without that all-up testing. You'd like to see that today. It's a different environment—I don't want to just say it's because people won't or can't make decisions. The decision process now is so convoluted. The interference from external—from OMB [Office of Management and Budget**]**, from GAO

[Government Accountability Office], congressional committees micromanaging NASA—it would drive me berserk. I don't think I could survive in the environment today. Just too much of a bureaucracy. This is not what NASA needs.

WRIGHT: We talked about Apollo 11, and you mentioned before about you knew exactly where you were when Kennedy was assassinated. Where were you when Apollo 11 landed?

HINNERS: At JSC. I was down there with George Mueller and some of his colleagues. We were just outside the [Mission] Control Center, we weren't inside. We were down there waiting and watching the TV, being somewhat astounded, as I recall, that the crew decided that there was no way they were going to take a snooze on the lunar surface at that point, so let's get out and do something.

WRIGHT: There's a decision.

HINNERS: Yes. Well, they made it. That was another one of those, remember where you were. And all the news coverage. I had been on for an interview with Walter Cronkite [broadcast journalist] explaining what some of the crew were doing when they got out there on the lunar surface.

WRIGHT: I know that each one of the missions had its own goals and objectives. Do some stand out more than others for you?

HINNERS: Oh, they were all superb missions. Look at what we learned. We learned so much from each of them, including getting surprises. Apollo 16, that was a Fra Mauro [formation] mission. What it turned out to be was not what all the photogeologic interpretations said it would be. So it really did bring home the limits of your ability to interpret orbital photography in terms of what's actually on the ground. Even today as you look at both lunar and planetary photography, Mars photography, and you listen to interpretations, you somewhat shake your head and say, "Yeah, you may be right," but you may not be if you really go down there and look on the surface. Orbital photography has come a long way but it still has its limits in your ability to interpret in very concrete terms what's actually there on the ground.

WRIGHT: How did you see that part of that package? You had been working with engineers. You worked with scientists. Then of course orbital photography grew into its own being. There was an evolution there, what originally started and then how it evolved through the lunar missions. How much were you involved with that? Were you helping with that or was it just something that you were overseeing and watching yourself?

HINNERS: At Bellcomm initially it was a mixed bag. We got involved with the actual details of planning. When I say we, people who were working in my department like Farouk and Jim Head. They were the real workers. And others in the group. It was a highly cooperative venture where the roles just somewhat melded. You hear about this infamous badgeless kind of environment, and at most times it was. People didn't care where you were from. If you had good ideas, good thoughts, throw them in the hopper, we'll talk about them.

That whole relationship that developed during Apollo did not carry over in the same mode to the follow-on programs. Never figured out totally why. I mentioned the ASTP. Then when the [Space] Shuttle was approved by [President Richard M.] Nixon it became evident that there was going to be an awful lot of tete-a-tete between the manned spaceflight world as it was called at the time and the science world.

Basic fact that you can't avoid is that doing science on human missions is expensive. You can do a lot of the science a hell of a lot cheaper robotically. Some of this comes down to who's going to pay the bill. On Apollo, the Apollo program funded the bulk of the science. So in a sense to space science it was a freebie. Most of it did not come directly out of the science budget.

Subsequent to that, it became evident that the science program itself would have to fund a lot of the what was called utilization of the Shuttle. We were pressured off and on. Say almost, "Wish this would all go away, not bug us, just let us do our science." But obviously recognizing human spaceflight is part of NASA, so we've got an obligation to say how do we best use some of this capability. John [E.] Naugle was very instrumental in trying to foster that. He was the AA [Associate Administrator] for Space Science prior to me.

At that time also the program was becoming international, when it became evident that NASA could not afford to outfit the Shuttle with all the capability they wanted to have. So that's when they started this major relationship with ESA [European Space Agency], some other countries, on providing a lot of the equipment that would go into the payload bay.

In the science office we had started a program to conduct science on the Shuttle missions. Initially putting our budget together, that was the first year I had the responsibility for it. So that must have been '74. I put in the five-year budget, a budget for science payloads for the Shuttle.

As I recall it was around $30 million, which today would be what, $150 million. Went up, gave the budget presentation, which was run by George [M.] Low. He had come up to Headquarters after Apollo as the Deputy Administrator. Real top-notch super guy. All business, and demanding, but a super super guy, much as he could be a thorn in your side if you didn't specifically know what you were doing. [He] put the fear of God into you, as did Rocco Petrone. And there's some merit to that.

George took a look at our budget proposal, and in his usual way told us that we were not being responsive and needed to significantly increase the budget for the Shuttle science. So that was imposed on us. We were trying to minimize it so we could do stuff we really wanted to do and George says, "Some of this other too." So our budget was upped. It had the great benefit in those days that it was not an *a priori* constraint, what you proposed in the budget. Today you're always working on a very firm budget limit. In those days there was more flexibility, even though you had guidelines from the equivalent of OMB. So actually putting more money in it just increased the space science budget. We did set up a new office to deal with science and started to work the relationships with, in large part, Marshall. Marshall had a lot of the responsibility for the science that was going to be done on the Shuttle.

WRIGHT: Did you have concern that the Shuttle was expected to be the only launch vehicle at some point in time?

HINNERS: Yes, darn right. At that time the expendables were part of space science: the Titan, the Atlas and the Delta [rockets]. So I had that responsibility, had a group that worked launch vehicles under Joe [Joseph B.] Mahon, because we were of course the only ones in NASA using

them. As the Shuttle program evolved—it started off with this unbelievable proposed flight rate. Twenty-five missions a year, it was going to be every two weeks there was going to be a Shuttle launch. I just could not conceive of how anybody was going to develop payloads for that kind of launch rate.

I was at a meeting with George Low one day—we wanted to fly something called the Shuttle Infrared Telescope Facility, SIRTF. George in one of his less lucid moments, my interpretation, said, "We'll just fly it multiple times." Like tens of flights. Quick calculation. I said, "George, it's very evident that doing this as a free flier is a hell of a lot more economical than putting it on a Shuttle 10 or 20 times. We want a year of observations, that's 25 Shuttle flights. Can't afford that." George didn't like that answer. But it did bring home the reality of the fact that there was no way you could conceive of funding payloads at that kind of rate. Then other things of course transpired to say there are other issues that they can't fly anywhere near that rate. Just pure budget issues on outfitting the Shuttle, the solids [solid rocket boosters], the external tanks. Let's get back to the expendables.

One way that NASA conspired to get the Shuttle used was to propose—I think Hans Mark was a big factor in this decision, he was still Secretary of the Air Force—that the Shuttle would become the one and only US launch system. An absolute cockamamie scheme. They wanted the Shuttle to be used and had to justify it with all sorts of users—DoD [Department of Defense], NASA, commercial. The only way you could make Shuttle economical was to capture all the users. That was what was behind that. Of course at that time the Air Force was busy outfitting a launch pad for the Shuttle on the west coast at Vandenberg [Air Force Base, California]. Put what, $3 billion I guess, into getting that launch facility set out there, before it all fell apart.

George put out a dictum, must have been around the ’76 timeframe, that we would phase out the expendable launch vehicles and become totally dependent on the Shuttle. He was looking for a pretty fast phaseout. I argued that was not a reasonable path to follow, that at least we should keep the expendables going until the Shuttle had been demonstrated, was flying. Because ’76, it was all still on paper. It was supposed to fly in ’79, but it was ’81 before it actually had its first flight. So we would have been totally without launch capability.

George did not like that input. Must have been around a month after I resisted his proposal that he decided to transfer the expendable launch vehicles out of space science into manned spaceflight, where they could control the phaseout of the launch vehicles. I guess he must have thought, “Okay, if I can’t convince him, I’ll just take it away.” That’s what happens in Washington. But I got a gift in return. He transferred life sciences from the Manned Space Flight Office to Space Science. There was actually some merit to doing that, because the chief of the Manned Space Flight Office just did not understand how the hell you do science.

That exists to this day. It’s just this, as we’ve talked, the dichotomy between science and engineering. Two different ways of working in the world. That made some sense, so we started the Life Sciences Directorate there in Space Science. Dave [David L.] Winterhalter. That became an embedded part of the Space Science. Later on somewhere a lot of the human-related medical stuff went back into Manned Spaceflight. I don’t remember when that actually happened, it was after my time there.

Meanwhile, in the Air Force, the Secretary of the Air Force, Pete [Edward C.] Aldridge, [Jr.] also thought this is not good for the Air Force to become dependent on a single launch vehicle. That’s when he initiated the Titan IV program with a buy of ten. The Air Force contracted with Martin Marietta [Corporation] to evolve the Titan III into the Titan IV system to

deal with the large payloads that were coming along. This was all before [Space Shuttle] Challenger [STS 51-L accident].

Then of course when Challenger happened there were a lot of people around who were very glad that he had made that decision, or the Air Force would have been grounded with all the NRO [National Reconnaissance Office] payloads. That whole concept of flying everything on Shuttle was flawed from day one. The foresight of, "We've got to use the Shuttle to the max, got to capture all the users," that goal just overcame, in my view, common sense. We just cannot afford to become dependent on any one launch system.

Of course that's now one of the big problems at NASA: where are our launch vehicles for the future? Shuttle is going away. Ares I [Constellation Program project] seems to have died or is in the process thereof. The EELVs [Evolved Expendable Launch Vehicles]—the Atlas V and Delta IV—have limits on their payload capability. Ares V is quite a ways off, so the country is in another bind on launch vehicles. Been there before, but there's not that backup. The Titan IV is not there ready to take over. So we're in another launch vehicle bind, more serious than the last one in my view.

WRIGHT: You started [as AA] in June in '74, and kept that job five years. At the time you went to work as the Associate Administrator for Space Science, you really had minimal experience working directly with Congress and OMB, yet you hit the ground running. A number of achievements and accomplishments, programs were done during that time. Tell us about how you were able to adapt professionally to this new role so well.

HINNERS: Oh, no sweat. I say that not as, "I can do anything," but it was easy in this sense: it's always been part of my management style, and it goes way back. I am not anywhere near as good either a scientist or technical expert as so many people are. What I do like to do is I love working with people and getting the best out of them, helping them get the best out of themselves. There's so much talent around that all you have to do is surround yourself with that talent and off you go. Let those people do what they're good at doing, they'll do it. And they thrive on it.

My belief is that the great experts who become managers frequently—I shouldn't say frequently—at least often, quash the talent beneath them because they view themselves as the experts. One recent glaring example of that is Mike [Michael] Griffin. Mike is very very talented technically, but tends to cow the people reporting to him. I think in some ways he quashed good debate that I think is so essential to a program. Unilaterally make decisions. So did George Mueller, but it was a totally different personality at work there, and capability and background.

My whole career has been built on just surrounding myself with the best people I could find, letting them do their job. If they don't, change them out. And [I] had to do that. It's always painful. None of us like to do that, but if somebody's not doing their job, you owe it to them to get them out. I had one experience with a fellow heading up one of our divisions. He just wasn't doing a good job, and I brought him in. I said, "It's not working. I'm going to have you go out to Goddard [Space Flight Center, Greenbelt, Maryland]. You're a scientist. Do some science out there. Let us get on with changing the management here."

"Oh, give me some time to fix it."

Six months later brought him back and said, "It's not working. You're going out to Goddard."

He said, "Thank God. I've never liked this job."

WRIGHT: Sometimes just not a good fit.

HINNERS: Yes. And he couldn't bring himself to tell me that the first time. It was like too much admitting that it wasn't working and he couldn't do it. Frequently you'll find changing somebody is something they actually like when you do it. I had that with the Hubble [Space Telescope] out at Goddard. Fellow heading it, not getting along with the [Space Telescope Science] Institute, with Headquarters and with Marshall. I concluded, "It's not their fault, the problem is yours." That's when I brought Frank [A.] Carr in.

When I told him, "I'm taking you off this," he said he had major problems with one of his children and had major health problems, and now he could spend time. So when you find somebody's not working in a job, there's frequently a reason that's not obvious on the surface, and if you can figure out what that is and get them in a position where they can deal with the personal problems and issues they have, everybody's better off. But you have to be sensitive to those factors, because people are in those jobs and if they have real strong personal problems, it creates havoc in the organization.

WRIGHT: I have to think when you were working on those lunar planning mission teams with all these different people of different backgrounds that you certainly learned a lot of different characteristics that can help. It was then too you started working internationally.

HINNERS: Yes. I never realized what "it" was. After my freshman year at Rutgers I went out west for the summer with a bunch of my colleagues, a couple of guys from Cornell and his brother, went out to thin apples in Wenatchee, Washington. Thinning apples is one of the most boring, tedious jobs you'll ever do. You climb up in an apple tree, a ladder, see a clump of three little green apples, you snap two off so that the remaining apple grows nice and big and gets rosy eventually and you eat it. After a week of that I said, "I'll go berserk doing this all summer." The state cops had come in looking for one of the immigrant workers who was wanted for murder. I said, "I've got to get out of here." So my buddies took me to Seattle [Washington] and dumped me off there. I looked for a job. Boeing [Airplane Company] was all full for the summer so I couldn't get a job with them.

I answered an ad [advertisement] in the paper for a traveling salesman, sell ironing board covers door to door, traveling crew. They were looking for people, men and women. So I got an interview and they signed me up, they bought me some new clothes. And after failing at being able to sell ironing board covers very effectively, Tom Fitzjohn said, "I'm going to make you a crew manager." Somehow even though I can't sell ironing board covers, I could manage one of the crews. That was probably the first time—looking back on it, I had no idea at the time—that someone saw that I had some people skills and likes that worked. Even though I couldn't do the job I could get others to do it. So I didn't have to do all that much, just get others to do it. It's worked ever since.

WRIGHT: It's good to learn a lesson and apply it, as you were talking about.

HINNERS: Yes. But I had no idea I was doing that. When I was still at Bellcomm I had a session with Rocco Petrone. He asked me to come over, spend some time talking about some astronomy. We were going to go for half an hour, start at 1:00. We started to talk, went on and on—interrupted by phone calls from Jim [James A.] McDivitt about lunar module issues down at JSC. Rocco would hang up, we'd just go back and start talking about something. We went on for three hours. Rocco had this incredible thirst to understand what was going on, from the universe on down to its small parts like Earth. He said, "Someday you could be the AA for Space Science." I didn't think anything of it. He was right, it happened.

George Low was a big piece of that. They had tried to get, I think it was Riccardo Giacconi, to take the job when John Naugle moved up. Riccardo decided—smart move on his part—he didn't want any part of being part of NASA bureaucracy. So it was largely George and John Naugle that said, "Well, let's try this young guy Hinners and see what he can do in the job." George with John said, "Yeah, we have to bring some of our younger people along, put them in positions. Most of them will probably perform." So that's how that all happened. As much as I had problems with George Low, I owe a good piece of my career to him. I did like him, but he put the fear of God into you.

I don't work that way. It's just not in me. I don't put the fear of God into anybody, just the opposite. But I would make up for that. I'd hire deputies who could put the fear of God into people. It was just a natural—because I didn't go out with that in mind. I had Tony Calio as my second deputy. My first one, that was George Low's suggestion, a fellow from Goddard, who did not work out well. I finally got him to leave, and then brought Tony in as my deputy. Tony was a very strong, good engineer, no-nonsense, so he complemented me. I was Mr. Nice Guy,

my deputy was always the bad guy—beautiful. Then Andy [Andrew J.] Stofan was my second deputy when Tony went over to head up the Earth Science part.

WRIGHT: When you went over to work for Headquarters did you still have involvement in the lunar conferences that were going on?

HINNERS: Yes. I would usually go down to the conferences to give a talk, listen to some of the science results. But it was not in-depth involvement. That was handled by the lunar office, which was by then integrated with the planetary. The lunar office started separate post-Apollo, and then it was put back in with the lunar and planetary. By that time the lunar conference was becoming an annual event, a real focal point for presenting the lunar science results.

Talk there, I think it was at one of those conferences, that got me in a bit of trouble back [at] Headquarters with Al [Alan M.] Lovelace. In the talk I was giving I talked about the Shuttle. Somehow what I said became a headline in the Houston Chronicle, made its way to Headquarters even before I got home. "The Shuttle is overweight, over cost, behind schedule." All true. But NASA did not like to see that in the headlines. Got my butt chewed out when I got back to Headquarters. Was not a politic thing to say.

WRIGHT: One of the other items that I thought was of interest while you were in that position is that you took a group to Russia [then the USSR/Soviet Union] to talk about a possible Shuttle-Salyut joint mission. Do you remember that trip?

HINNERS: Yes, vaguely.

WRIGHT: Charlie [Charles F.] Kennel mentioned it.

HINNERS: Yes, Charlie was on that trip. I do not remember who the whole delegation was. It was post-ASTP and there was a desire at NASA at least to look at doing a follow-on mission to keep the momentum going with the Soviets. There wasn't a lot of energy behind it. George Low was advocating it. My recollection, it was more of a, "Let's see if there's something there that makes sense." We had our meeting over there. It pretty much fizzled. There was not a lot of enthusiasm on either side. Of course we were going into it from the science side, and the Soviets didn't seem to have a lot of energy in it either, and I never really understood the total dynamics of that situation.

WRIGHT: Was that one of the first times you had worked with the Russians?

HINNERS: No. [I had] been over a number of times. First time we had a meeting over there was a COSPAR [Committee on Space Research] meeting, 1970 it must have been. It was post-Apollo 11. We had a conference there in Leningrad [now Saint Petersburg, Russia], had a lot of presentations on the Apollo science, and met with some of the Soviet delegations. We had a number of follow-on sessions on robotic cooperation both looking at lunar missions and Venus and Mars. This went on through the '76, '77 time period. The Soviet delegation was still heading that up on their side. They did not really understand why we wanted to cooperate with them. I guess they figured after Viking [Mars mission] that, "You people can do all these things on your own." Then it also turned out that Viking put some—limit is the wrong word, but

decreased their desire to do Mars missions, because they said essentially, "After all, you've shown there's no life on Mars."

That comes back to the fact that the whole life issue, the astrobiology piece of Mars exploration, still to this day drives a lot of the impetus to explore Mars. If it were only the interest in planetology and geology, geophysics, you wouldn't have seen this tremendous emphasis on Mars exploration. Geology per se just doesn't get you that far in terms of that kind of investment. First time you see a new body, like the outer planets and the satellites, and the amazing things you'll see—Io [moon of Jupiter], Enceladus, Titan [moons of Saturn], real eye-openers. But doing detailed geologic studies beyond a certain point—for the dollars you spend on it, you ask yourself, "Is the return on investment there?"

Somewhat like the lunar missions. Great geology, but geology and the interest in geology alone would never garner the budgetary and political support necessary to do that kind of extended Apollo exploration that went on. Science is in an interesting position that you've got to keep doing forefront science to get the resources.

Jumping forward to today, that leads to ever increasing cost and complexity of missions. They just grow and grow and grow as scientists want to do more but also the instrumentation is getting so much more complex, therefore expensive, that mission costs zoom. That's a major issue right now, one I'm working with Headquarters on, which is looking at the total cost of missions, and why is it that every NASA mission has a cost overrun. There are reasons for it. Whether they're good reasons or not is a debatable topic.

WRIGHT: Were these some of the same issues you were facing when you first moved into the administrative job in the '70s, because the Shuttle was first coming up with their budget issues, and then now you were trying to keep science going?

HINNERS: Yes. You always do come down to what does something cost and how do you allocate the budget that you do have and distribute it among the things that you have as a priority. You try to prioritize and eventually some of the things at the bottom do drop off, but you just cannot afford to do everything. Managing the cost is a pain in the butt, but you have to do it. Because there's not infinite budget, you've got to make priorities and decisions. At the same time there's an inexorable march towards complexity and large size. There are two NASA missions right now under way which have not been subject yet to detailed cost reviews even though both have overrun tremendously, the Mars Science Lab at JPL and the Jim [James] Webb Space Telescope, which Congress just a couple months ago asked for special cost review because there was a prospect of having major overruns. Cost continues to be a major driver. Trying to understand and better control those costs is a key challenge faced by NASA.

Nothing new in that sense. As I pointed out at our meeting a couple weeks ago with [Christopher J.] Scolese [NASA Associate Administrator] and [Michael G.] Ryschkewitsch [NASA Chief Engineer] at Headquarters, in 1981 what's called the Hearth Study [Project Management Study] looked at science mission costs and their overruns and made a number of observations and recommendations. This problem was seen and known and to some degree diagnosed 30 years ago. Yet today we have the same problem. So you ask yourself, "Did we do anything about it?" The answer was in large no. We didn't learn the lesson learned. Just like *Challenger* and [Space Shuttle] *Columbia* [STS-107 accident].

I don't know why it is we seem incapable of applying these lessons. We can find the excuses, but are they good reasons? No. Just sent a note off to Ed [Edward J.] Weiler [Associate Administrator of the Science Mission Directorate] on that topic. Our [observations] coming out of that last meeting. We've got to make some tough decisions and relook at how they're doing the management, because if we don't, clearly on [Capitol] Hill—the environment in these budget times is getting tougher and tougher, and if we can't demonstrate to OMB and Congress and GAO that we're responding in their view responsibly to the criticism of cost overruns, we're going to pay a price for it. You lose credibility. Go up with a proposal for a new mission, and it's no different than 30 years ago, "Why should we believe your cost estimates?"

WRIGHT: Part of your opportunity while you were in that leadership position was that the [James E.] Carter [Jr.] administration came on with their—

HINNERS: Zero-based budgeting. Oh yes. Yes, that was an interesting several months that first budget go-around. One of the first things that happened on feedback we got from OMB and the President's science adviser, Frank Press, was cancel Galileo [Jupiter mission] as unaffordable. We resisted that. We went over and talked to them and somehow we talked our way into keeping it in the budget.

In those days it was a different environment in the Office of Management and Budget, the budget folks. It was a lot friendlier. I think today it is significantly more adversarial. We had great support from the examiners for the science missions. A fellow named Hugh Loweth headed up the section that dealt with NASA. The year we were trying to get both Hubble and Galileo through as new starts, we got very good support from OMB, and got it through the White

House in the NASA budget submission. When that went through Congress we had the fight with Congress on the Galileo mission and Hubble.

WRIGHT: How were you involved with that? Did you have to do the testimony?

HINNERS: Yes, the usual cycle of the testimony with the authorizing committees, appropriations committees. The NASA Appropriations Subcommittee in the House [of Representatives] was not inclined to keep both Galileo and Hubble going in the budget. The Hubble external advocacy did a pretty good job of lobbying on the Hubble side, so the committee decided we'll take the Galileo out. The authorizing committee, Don Fuqua had approved the NASA budget, which included both missions. So they raised an issue of who's in charge of what. They noted that it's up to authorizers to say what's in the budget, it's up to appropriators to appropriate funds to support what has been authorized. So it was drawing a very sharp distinction of responsibility between authorizing and appropriations. It came down to a vote on the floor of the House.

It's in one of the other stories about a conversation with Jim [James C.] Fletcher, who was the [NASA] Administrator. Before all this came to a head, Jim had said—I went up to his office—"You're going to have to give up one or the other, Hubble or Galileo."

"No, I can get them both. I think I can do it, Jim, let me give it a try."

Bless his soul, he said okay. A lot of Administrators would say, "No, I made the decision, which one do you want to give up?" He said, "Okay, give it a try."

So we got to work, and came down eventually to a vote on the floor of the House as an issue between authorizing and appropriations. Tom [A. Thomas] Young and Jesse [W.] Moore and I went up to the gallery to watch the vote. We won, by close to two to one. The head of the

appropriations subcommittee and his chief staffer, were they pissed. Unbelievable, because it was a major defeat for them. But as you know, on the Hill, there are winners and there are losers.

Then the next year at the appropriations subcommittee hearing with Eddie [Edward P.] Boland and Dick [Richard N.] Malow [staff], "Okay, Dr. Hinners, let's start to go through the space science section of the budget." Got to the Hubble, and I started to talk, went into a great description of the great things Hubble was going to do. All of a sudden Boland got red in the face, pounded the table. "I've heard enough. Turn to section blah blah blah." So I was being given the shaft. He had not forgotten the year before. I had been fiercely defending all these great things they had approved. We went out and Jim Fletcher said, "Noel, I'm glad you said what you did, but I'm glad you stopped when you did."

WRIGHT: That's a good lesson learned too.

HINNERS: Interesting times with Congress and OMB. But that's somewhat the norm. I agree it's a lot tougher today. Those days, you could go up and just meet with them one on one without having a whole bevy of staffers from NASA along with you. Go up one on one with Dick Malow, who was the chief staff guy on appropriations subcommittee, and spend a Saturday morning talking about the budget and issues and what was bothering them, what we were trying to do. It seemed like a good normal way to do business, which it was, but you can't do that today. The bureaucracy is running the town, bureaucracy in its worst form, where the less you know seems to get you more power.

WRIGHT: Before our time is up today, just a couple more things on the Apollo era. One that came out of it was Skylab. Were you very much involved with the science and what was being done with Skylab?

HINNERS: No, I had very little direct involvement in Skylab. Almost none to speak of. So I just can't speak to it. [I] look back with interest on what was done so rapidly when that solar array came off during launch and how in 30 days or so NASA developed a fix, engineered it, got it ready to fly and flew it. The response time was incredible. That still remains today in NASA. If something happens the resources available to rapidly understand, analyze, come up with a fix is phenomenal. You can see that today with [the International] Space Station, some of the things that go on. A big problem will come up, and boy, the folks on the ground and in space come up with these fixes in close to real time. Some of the Hubble repair stuff when things weren't working, somebody will come up with that bright idea, try this, try that. It's a phenomenal and rare capability I think in both government and industry to have teams that well trained, honed, integrated, and knowledgeable that can come up with those rapid solutions.

WRIGHT: We were talking briefly about Apollo 11. You mentioned about remote sensing, the element that had been part of that package. Then Landsat [Earth observing satellite program] was introduced in the '70s. Was that something that you were involved with as well or administrated?

HINNERS: No, Landsat was in the Earth Science or applications area. That was separate from Space Science. You'll see through history those things merge, unmerge, merge, separate. Today they're together, and we rapidly expect that someday soon they'll be separated again.

WRIGHT: The other thing I had listed was the Viking and the Pioneer programs that were ongoing. Did you move into those programs at that time?

HINNERS: Viking was under way. The Viking program as it finally flew started in '68, having evolved from an unsustainable monster to something doable. Again somewhat a lesson learned here. NASA will often start with these grandiose projects that rapidly outgrow your ability to implement them. They get restructured into something that's doable and still spectacular. The original Viking, which was called Voyager—had nothing to do with the Voyagers that eventually flew—was two monster spacecraft, orbiter and lander, two sets, on one Saturn V. That did fall under its own mass and evolved into what became the actual Viking program. I got involved in Viking in the site selection in '72 to bring in some of the Apollo site selection experience, and met with the Viking site selection teams, usually down at Langley.

Then of course in '74 I got responsibility for Viking, which was in deep trouble budgetarily and technically at the time. So I got rapidly immersed in dealing with budget issue, and again found how the power structure works. The experience on the Viking cost overrun—this was probably '75 or may have been '74. I got a call from Ed [Edward M.] Cortright, who was the director at Langley. Before I returned the call, Bob [Robert S.] Kraemer, who headed Planetary, came up to my office to tell me he just had a conversation with Cortright. Cortright

was asking for another $30 million, which meant today it'd be $150 million or $200 million. Bob said, "Tell him no." It was just here they are overrunning again.

So I said, "No, Ed. Not going to give you any more money." Hung up. It couldn't have been more than guess an hour, couple hours before I got a call from George Low, Deputy Administrator. Ed Cortright and George Low were buddies. George said, "We've got to give them the money." This is a national program with high visibility and we can't afford to do everything we can to make it work. So I went to the Comptroller and had the Comptroller give me $30 million. Didn't come out of my hide, and on we went. It was an interesting play of the politics, but also the view of where you do and don't want to add any risk. Lesson learned.

WRIGHT: One of many. I'm never quite sure where the Space Station began, but where's your first recollection of where you first became involved in or started making input into thoughts of long duration space?

HINNERS: I didn't pay an awful lot of attention to it early on. Partly it didn't pick up internally in NASA until after I had left the AA job in '79. I don't recall being involved in any discussions on the Space Station. Then I was over at the [Smithsonian National Air and Space] Museum until '82. Then, when I went out to Goddard in '82, the Space Station activity had started up and the question there at the time was what role, if any, does Goddard play in the Space Station. Jim [James M.] Beggs was Administrator at the time. We had some debates internally at Goddard whether we wanted to touch the Space Station or not. We did what I think is the first strategic planning activity in NASA. Did it at Goddard. I was trying to figure out how the hell do we figure out what to be involved in, what not to, make decisions. So we did start, and completed a

strategic planning activity, and looked at and analyzed why should we be involved in this or that. Space Station was a big piece of that, the whole involvement with human spaceflight.

I decided at that point we should be involved in the Space Station. It's part of the major piece of NASA's program, they're going to do science on it, and we can contribute to the implementation and the quality of the science that gets done on Space Station. Always in your mind is it's also work to bring to Goddard. The Center Director is always thinking, "Okay, what work can I bring into the Center?" Your perspective changes depending where you sit. So [we] decided to go into the Space Station and find a role involved in the implementation of the science. Have to think through again and look at some of the background of the pieces we were looking at. Outfitting a laboratory, attached payloads area. It was not a uniformly popular decision, but so be it. You're not looking for a vote.

We got support from Headquarters. Hans Mark, who was the Deputy with Jim Beggs at the time, was very supportive of us playing a major role. Also building up a capability at the Center to do large Shuttle size payloads in house at Goddard. We ended up sending some people out to support the activity in Reston [Virginia] where some of that early Space Station activity was headquartered.

Again, I don't remember all the shenanigans that went into establishing Reston as a Level II for Space Station. It was to provide the Headquarters equivalent of the integrating facility for all the Space Station work for Marshall, Johnson and the Cape. Then anything that Goddard or JPL would add to it. We did supply some people to go out there to work in Reston in the Space Station Program Office, which I think John [W.] Aaron may have been heading up at the time.

WRIGHT: Well, why don't we stop there for today, and tomorrow when we start out we can talk about other projects. More about the space telescope efforts that you did at Goddard.

[End of interview]