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ORAL HISTORY TRANSCRIPT

HAROLD D. BECK
INTERVIEWED BY REBECCA WRIGHT
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The questions in this transcript were asked during an oral history session with Harold D. Beck. Beck has amended the answers for clarification purposes. As a result, this transcript does not exactly match the audio recording.

WRIGHT: Today is December 9th, 2004. This oral history is being conducted with [Harold D.] Hal Beck in Houston, Texas, for the NASA for the Johnson Space Center Oral History Project. The interviewer is Rebecca Wright, assisted by Sandra Johnson.

We thank you for coming by this afternoon to visit with us for this project.

BECK: You're welcome. It will be fun.

WRIGHT: It will be fun. We'd like for you to begin by telling us how you became employed with [NASA] Langley Research Center [Hampton, Virginia] in 1959.

BECK: My interest in aeronautics started years earlier. As a kid I was very interested in aeronautics and did a lot of model airplane building and flying. Then when I got out of high school, I went into the Air Force and was in the Air Force for four years, out of high school. While in the Air Force I decided that I would take some correspondence courses in aeronautical engineering; so that's when I got my appetite for aerospace.

I got out of the Air Force and went to North Carolina State [University, Raleigh, North Carolina] for four years. That was about 1955. They were one of the first universities at that time that had an aerospace-type option along with aeronautical engineering. They had a few aero-type courses, and I was very enthused about college at that time, because I had spent so much time in the Air Force and realized that I really needed a good education.

Then Sputnik [satellite] came along. That was an incentive or a validation that aerospace was something of the future. That's the first hint that I got, real public recognition, that there was something in the space business, and so I got very interested and enthused about the aerospace-type courses with NC State. So we had quite a few good aerodynamic courses, but the aerospace courses were fairly limited.

That was my first introduction to computers. That was probably in 1958. We had a short course in computer technology, and so we had access to one of the earlier computers. In this course we did our programming in machine language, and so we had several projects to program in machine language and put on the computer and execute. So that was most interesting.

Then in the last semester at NC State in the aerospace world, we had a team project, and we designed a rocket system and a small payload and for the whole semester we did the research and put together a top level system design. That was my first exposure to orbital mechanics and rocket science. So that was challenging.

When I graduated we had several people in our class that eventually went up to Langley. I'm sure you've interviewed quite a few of them. I had a cousin that worked there. He was in the research side. He invited me to come up to visit. [Edgar C.] Ed Lineberry [Jr.] and I were classmates in college; in fact, we were from the same hometown. So Ed and I went up, and we interviewed and got a job over on the research side of Langley Research Center. That was in probably June of [19]'59.

I started out working in aerodynamics with wind tunnel data. I worked for a guy named Bill Aiken. He was in Henry Pearson's department. I did a lot of aerodynamic analysis using the Friden calculator, which was our means of calculation in those days. I looked back in some of your interviews, and you had interviewed [James] Kirby Hinson, and he mentioned the Friden calculator when he was at Langley. The Friden was a large mechanical machine, which did only addition, subtraction, multiplication, and division, and you had to use log tables and square root tables for your calculations. So to do matrix operations on a Friden calculator would take weeks to do.

It was very interesting. You had a large group of computation aides. It was like a typing pool, and you had thirty or forty of those people in a room operating Friden calculators, and all day long, all they would do is punch in numbers and do mathematical calculations. That's the way we did all our computations.

I soon started working with [John M.] Jack Eggleston who was later here at JSC [Johnson Space Center]. Jack was working on a rendezvous analysis project, and he had a real clever analytic technique. He was analyzing the approach trajectories of what we called ferry vehicles to a Space Station. I was his aide at that time, and so we did pretty extensive analysis on approach trajectories to a Space Station by a ferry vehicle, and we published that in a TN [technical note] format.

It was called a Langley Research Center Technical Note. To get one of those published in those days you went through very laborious weeks of editorial committees and review cycles. The Langley way was very methodical and precise.

So we finally got the document published, and I don't know for a fact, but I would imagine that was one of the first rendezvous applications that was published in this country. I don't know about Russian rendezvous analysis and documentation. That was my last assignment while I was at Langley Research Center, and then I transferred over to the Space Task Group in 1960, I believe.

I worked for John [P.] Mayer in the Mission Analysis Branch. That branch was already established and had been for some time. [Catherine T.] Cathy Osgood was one of the people there and Ted [H.] Skopinski and Carl [R.] Huss, and a number of people who eventually came to Houston. That group was doing extensive analysis on low Earth orbit flight mechanics. We did a lot of ground track type analysis. There we started developing programs to compute trajectories and ground tracks for the orbital flights and were supporting the early Mercury planning and also the unmanned, like the Enos [chimpanzee, Mercury-Atlas 5] mission and Sam [Rhesus monkey, Little Joe 2].

So we did our computations then on one of the—oh, to go back, when I was at Langley Research Center—I remember that was probably in late [19]'59—they announced that they had gotten a computer, an actual electronic computer. So everybody was invited to go to the building and look through the glass windows and see this big box sitting in the middle of the floor with the blinking lights. So all the Friden people pooh-poohed that like, "Oh, that is too complicated. It will never replace the Friden calculators." So we went by there and oohed and aahed about the wonders of flashing lights and how fast it could add two numbers and subtract two numbers, etc. That was our first introduction to a big computer.

Then when we got over to the Space Task Group, in that branch we had an IBM [International Business Machines] 1620, and that was a small—well, it was a large, physically large, computer, but it was low capacity, a fairly slow type machine, and it operated on paper tapes. You would type your program in a programming language. I imagine it was a form of Fortran, but I remember how enthused I was with the programming capability.

You would punch the program on a paper tape that was about a half inch wide (this was before the card readers). Then you would feed that program into the computer and then execute the program, and it would go through computations. In those days, you didn't have data displays. You went through a series of computations, and at the bottom, you stored your parameters, and then printed them out. Then you'd go through the computation cycle repeatedly.

Then you would have a printout of the results, and you went to a group that were called—we called them math aides. It was a group of people that went through the documentation and picked off the parameters that you were interested in and wrote them down, and then they plotted the parameters in a graph format. They plotted the data in a presentation-type format for publication. So that's the way we did the displays of all the parameters. Because the memory in the machine was very, very limited, we couldn't devote computer space to developing displays.

The days at Langley went by pretty rapidly. We were in an old building over on the Air Force side and had a pretty good office space. We had this one little computer room with an IBM 1620 in it, and Shirley Hunt Hinson, was the mother and owner of the 1620, and she ruled the 1620 like a kingdom. She was very good at that.

So, let's see; anything more at Langley. One of the functions of that group was to analyze the trajectories and do the planning for the short orbital flights. They also did a great deal of postflight analysis. That organization was responsible for the early development of the basic computer programs that we would use for all of our planning and analysis type work. We were taking the orbital mechanics functions and programming those into computers and checking results against actual flight data to verify the computations, like the Earth models, vehicle maneuvers, etc. So really the fundamental work that was done there contributed or laid the base work for our lunar planning and analysis capability for Apollo.

WRIGHT: Were your calculations close the first time, or how much trial and error did you go through?

BECK: Oh, it was a lot of trial and error. One of the first things that we did, we started out programming the basic orbital elements, and we didn't take into account the atmospheric drag properly; we took it into account, but it wasn't really simulated accurately. There was a lot of trial and error. In the early, early days, we started with using a spherical Earth, just a plain Earth point mass computation, and we could use that fairly accurately for things like parametric analysis, but it wasn't good in terms of a simulation. So we actually went through the process of refining all of that and developing higher fidelity simulations.

Back in the Langley Research days, they also had another computer facility that was extremely useful, especially for the analysis of spacecraft control systems, and that was an analog computer system. That was managed by a very well known Langley engineer. I think his name was Sam Phillips.

Anyway, Ed Lineberry worked with him quite a bit and analyzed control system characteristics. That was something that could be done with an analog computer that couldn't be done easily with a digital. Analog computers in those days were really the old, old analog computers with the vacuum tubes. It was really an old system but very accurate and very easy to simulate control systems. That's how Ed Lineberry got started with the control system analysis. He also transferred over to Space Task Group.

WRIGHT: How did you learn that you could join the Space Task Group? Did they approach you, or did you notice that you would like to join them?

BECK: Yes, they actually approached us, and I don't know in what form, whether that was informal or not. I'm not really sure whether I initiated that move. I know that it was known that they were trying to build up capability. But anyway, we got the offer through the proper channels eventually, but I'm not sure exactly how that was initiated. I really don't remember. But quite a few people at that time were moving over.

I was fairly late going into the Space Task Group. I think my badge number was 202, if I remember right. So they had already had a pretty good complement over there. They were really in two buildings. [Eugene F.] Kranz's group was over in one building, and our group was in another. [Christopher C.] Kraft, [Jr.], of course, was already over there, and John Mayer and Carl Huss and [Howard W.] Bill Tindall [Jr.]. Then we had a contingent come down from Canada. That was [Rodney G.] Rod Rose and Morris [V.] Jenkins and a fairly large group of very sharp engineers.

Then in early 1962, we moved down to the Houston area. We came down in February just to see the place, and it was not too much of a shock, mainly because it was at that time of year, and Langley is not the prettiest place in the world in February, either. It's drizzly and gray. But we came here and landed at [William P.] Hobby [Airport] and drove down to—and I'm sure you've heard this story from everybody you've interviewed—drove down I-45 and took this dirt road over to the site. It was a gloomy, dreary, ugly day, and all you could see around you were—nothing; shrubs, and no pretty trees, no anything. And, of course, Virginia is just gorgeous. So I thought, "Well, this is going to be something else."

While I was visiting here in Houston, we had a terrible storm in Virginia, and it was really an off-season storm, almost like a hurricane. I had a beach house right out on Windmill Point [Virginia], which looked over Chesapeake Bay.

When I got back—well, I had an Austin-Healey [automobile], and it was completely covered with sand. The only thing that was sticking out of the sand was the radio antenna, and the whole area was completely flooded, and there was like three feet of sand in my driveway. But the people that were watching out for the house, they did a good job, because they tied a rope around the bumper of the Austin-Healey, ran it through the window, and tied it around the washing machine so it wouldn't float away. So anyway, that worked, evidently, because it didn't go anywhere.

The car was a disaster, and I tried to get the insurance company to total it, and they wouldn't. So they took it to the Austin-Healey place, and they replaced just about everything on it. It must have cost them a fortune, but anyway, that was in February, and I was transferred down here in April, so the Healey wasn't finished.

That's when Cathy Osgood volunteered to drive it down, and so she probably regretted it, because by the time she came down, it was summer and it was really hot. The Austin-Healey, of course, didn't have any air-conditioning, and I remember the floorboard was so hot that she had to wear a thick, heavy sock on the accelerator foot to keep from burning her foot. She said it was really, really hot. Anyway, that was the story of the Austin-Healey that Cathy mentioned to you.

When we got to Houston we were assigned to the HPC [Houston Petroleum Center] Building on the Gulf Freeway, and that was a nice place to work. Good office space, and it was a nice, new building at the time, and it was in a nice area, too.

We were there, and the IBM 1620 came with us, but in addition, we had contracted with the [Houston] Medical Center to use their computers. I would imagine they were IBM 704s. Later, we contracted with the University of Houston and used their computers. When I came here I worked for Morris Jenkins in the Mission Analysis Branch. That was under John Mayer. Morris had formed a Lunar Trajectory Section, and so I was assigned to that section. In fact, I was head of the Lunar Trajectory Section under Morris. At that point, I started working on the development of capability for the Apollo Program. Even though Mercury and Gemini were going on, I had gotten into a look-ahead-type program, and we started developing the capability to go to the Moon. That was in the summer of [19]'62.

I think I have a picture of the HPC group. I've got a good photograph of that group that you could get copied, if you'd like.

WRIGHT: Okay. I'd like to do that.

BECK: It's taken there at the HPC Building. There was a little pond out in the middle, and it's taken over a bridge of the pond, and most of the members that were there in the Mission Analysis Branch were in that picture. I think Glynn [S.] Lunney is in there and John Mayer and Carl Huss and Bill Tindall and Cathy and Mary Shep Burton. I guess you've had people mention Mary Shep Burton.

WRIGHT: I think so, yes.

BECK: Remind me, I'll come back to her, because she really had a very, very significant role in the whole program. At that time, we had started developing the capability to go to the Moon, and of course, that was a brand-new activity. How do you simulate the Earth-Moon system with a computer? Especially when you look at the computers in those days. It's hard to imagine, but we did most of our work—a lot of the work was done with a 32K machine memory, and a lot of it was done with 64K, and that's almost no memory compared with even a small hand calculator.

But again, the only way that was accomplished was that when you would do long computations, you would take a time step and go through the computation and compute all the parameters and you would enter those parameters in a print block and just print them. You didn't display them or you didn't plot them or anything on the computer. Then you would repeat the computation for the next time step.

Well, as a result, when you computed a lunar trajectory, you'd have a stack of paper like a foot thick. You'd take this stack of printout in to the math aide group, and they would go through page by page, and each parameter that you were interested in, they would write it down. After tabulating all of the required parameters they would plot it and then type up headings, scales, etc. That's the way we did our formal documentation. All of the trajectory work was done that way.

So Mary Shep Burton headed up the math aide group—I imagine at one time there were probably twenty-five or thirty people in that group, and extremely dedicated, very, very hardworking people. They were a real stickler for accuracy, and they sort of watched over the engineers, so if the engineers brought them anything that they considered out-of-bounds, well, they would send it back and say, "Hey, take it back. You'd better do this over." Mary Shep still lives here in Houston, and she comes to our MPAD [Mission Planning and Analysis Division] luncheons occasionally. We have a luncheon about every three months.

She played a very significant role in the early history. She and her group were greatly appreciated. All the information that was sent to the upper management for review and for decision-making processes had to be prepared from very fundamental raw data. Today all you'd do on a computer is run the program and take the parameter of interest and put it through some software and get a nice plot out in color and do it in thirty seconds.

Mary Shep's group would often work all night to meet publication deadlines for imported internal notes (INs). In the early days, when you made fifty copies of a document you made fifty copies of each sheet, and you had to hand collate them and staple them. I remember when we got our first collating machine; it was like getting the first computer. It improved efficiency tremendously.

While we were busy in the lunar trajectory section, other elements of the organization were doing their thing with Mercury and Gemini. Mission Planning and Analysis Division soon developed into a very specialized analysis group, and they had all the fundamentals of flight design. They had navigation; they had robotics; they had trajectory; they had control systems; propulsion. They had the launch/ascent analysis responsibility, so they simulated all of the ascent vehicles, and, of course, the entry.

Within MPAD we had specialty groups (called discipline specialists). They developed specific software applications for their discipline such as launch or entry, etc. In the mission planning process the Launch Group would simulate the ascent trajectory and then the end conditions or the primary parameters for orbit insertion, and they would pass those conditions to someone in another group, like the on-orbit trajectory section, and they would generate an Earth parking orbit, okay?

Then after we would determine the position for translunar injection, we would take the conditions out of that Earth parking orbit, and give that to the translunar injection people, and they would simulate the burn for translunar injection, and they would take those cutoff conditions and pass them to the next group, like translunar midcourse correction type people, and they would—and so what I'm getting at is that at that point, we didn't have an integrated program that worked your mission from end to end.

We began to develop what we called the Apollo Reference Mission Program (ARMP). It was a simulation of all phases of the lunar landing mission from lift-off through reentry. It was used for mission planning and for the generation of the operational reference missions for all of the lunar missions.

We developed what we called reference missions, which were published in the form of an internal note. For Apollo 8, for instance, we generated an end-to-end reference mission for the nominal launch conditions and nominal lunar orbit, translunar injection point, etc.

From that mission, we spawned all kinds of alternate mission and contingency profiles, including abort trajectories. Probably the vast majority of the work that was done in MPAD was with contingency planning, as opposed to the nominal.

Also, in addition to the nominal profile, we had a launch window capability. If you had a hold on the pad at some point, and you picked up the count again, well, you would have not the original launch azimuth to go on, but you would have another launch azimuth. So your launch conditions changed as you slipped into the window. As the launch slipped, then everything downstream changes; there were some of the basic functions of the organization.

That organization, for years, was headed up very ably by John Mayer, Carl Huss, and Bill Tindall. They were absolutely incredibly devoted to the space program. They were highly respected by the people who worked for them and by co-workers around the Center and throughout the Agency.

Bill Tindall was one of the most remarkable people in the space program, to me. He was very, very enthusiastic and an absolute genius. He had a remarkable capacity for bringing diverse groups together. As a team lead, he would bring representatives from a number of groups together and address a common problem, and he could sort through the rubble and really come out with the important points of a discussion or an argument; mostly arguments.

He ran a group called the Flight Techniques. That was across the center. It usually had representatives from the flight crew, the Engineering Directorate, the Mission Operations Directorate, Mission Planning, and Flight Software. The interface between Mission Planning and Flight Software was a very complex type interface, and he was very instrumental in keeping that interface working efficiently.

Within MPAD we had a Mathematics and Physics Branch that was eventually headed by Emil [R.] Schiesser. He was a space navigation guru and still is, as a matter of fact. His group was responsible fro the navigation computations and for the perfection of the math models used in simulating the Earth-Moon system. They were especially key in the definition of the lunar potential models.

Apollo 8 was of course the prime test of our Earth-Moon system simulations. To me, one of the most interesting experiences during the Apollo [Program] was Apollo 8, when the spacecraft went behind the Moon just exactly when the model said that it would, then after an endless wait it came out from behind the Moon after lunar orbit insertion just as predicted.

WRIGHT: That must have been rewarding to know that your calculations were right when they needed to be.

BECK: Yes, and especially, I think, for the navigation people. The lunar orbit for Apollo 8, I guess, will always be remembered by many.

WRIGHT: Since you're talking about it, I was going to ask you a question. Apollo 8 actually got moved up in its rotation of where it was supposed to be in the schedule. How did that affect your job to get the calculations right? Did you know it was going to be moved up, or was that announcement to you as well?

BECK: We didn't know very much earlier that it was going to be moved, but of course, scheduled products were always a challenge. Organizations around the Agency depended upon the timely release of MPAD planning products. Everybody just worked around the clock. It was incredible the work that people were doing.

I told Kranz this when he was writing his book, that we used to have a real challenge running lunar trajectories because machine time was so precious and lunar trajectory iterations were so consuming. We'd go over to the University of Houston [Houston, Texas] on the midnight shift to get our computer runs done.

I was working with [James R.] Jim Elk using the lunar iterator program to calculate the target parameters for translunar injection. These calculations required long computer runs. We decided on a strategy to get the computer runs done. The computer operators at the University of Houston really loved Budweiser [beer], and so I always loaded up the trunk of my car with a cooler with Budweiser and gave all the computer operators a key to my trunk. So in the mornings I'd have to go to work real, real early, because I'd have to sneak my printout into the building, because I didn't want people to see how much printout I had managed to get. We got good turnaround time.

The development of translunar injection targeting capability was a key capability in lunar mission planning. It was necessary to iterate on translunar injection parameters in Earth parking orbit to obtain injection burn cutoff conditions which would give you the correct arrival conditions at the Moon.

In the Apollo Reference Mission Program, we had what we called a translunar iterator, called the forward iterator. The iteration technique was formulated by two mathematicians, [Samuel] Sam Pines and Henry Wolfe. They had a company called Analytic Associates, Incorporated. They were a contractor to NASA, and they developed the iteration capability that could iterate on the proper parameters at SIV-B cutoff to achieve the proper translunar trajectory.

That was a bear of a program, because you had to iterate, and then you had to integrate the trajectory all the way to the Moon to fine-tune the injection parameters. In the early days, we had to target a free-return circumlunar trajectory, i.e., the spacecraft would go around the Moon and back to Earth without any major maneuvers.

So this iterator did that kind of work. In part of that iteration, we developed what was called a patch conic technique, such that you would use conic trajectories and not have to integrate the whole trajectory for your special targeting. Then you would switch from the patch conic trajectory, to the integrated for precision. But all that patch conic meant was you would use a conic trajectory outgoing from the Earth, and then you would have another conic trajectory, patch conic, that was in the vicinity of the Moon. So you basically used two two-body systems and didn't integrate; you just did conic-type analysis. It was a sort of a shortcut for computation capability.

A big part of the lunar trajectory and mission planning capability had to do with the interface between JSC and Marshall Space Center, because Marshall had the responsibility for the launch vehicle and also for the performance of the SIV-B for translunar injection. MSC assumed the responsibility for overall mission design. We went through extensive negotiations. Some of us spent more time on the airplane to Marshall than we did at the office. The mission planning interface was worked out through the jointly chaired Flight Mechanics Panel.

You want to take a little break?

WRIGHT: Sure, be glad to.

[pause]

WRIGHT: So many times today people send e-mails or faxes, and of course, you didn't have that capability back then. How were you able to maintain a good system of communication, even within in the center of your own group, with so much coordination of passing on information?

BECK: Our old method wouldn't work today, because it was one of the most rudimentary type things. We were on such a schedule crunch; we didn't always have the freedom to write formal memos. Between organizations within MPAD important interface parameters were transferred via informal handwritten "memos." This was well before PCs [personal computers]. Today those parameters would be approved by management and transmitted electronically.

I think it's remarkable that the system worked as well as it did, but like you say, without e-mails or even electronic (correcting) typewriters. In those days, you typed on a plain old typewriter, and what you put in was what you got, and the only way you could correct it was take Snopaque and Snopaque it out and retype it. Important formal memos that went out of the Division had to be letter-perfect, and the excellent secretaries were responsible for that. When electronic typewriters came out they greatly improved efficiency.

Even toward the latter years when the computers came out, John Mayer was sort of old-fashioned about it, and he early-on resented engineers sitting down at a computer and doing their own charts. He said, "You engineers ought to be doing engineering work, and you let the secretaries and the math aides do that work." But things really changed with the advent of the computer.

WRIGHT: What changed after Apollo 8? So many years you had been working on the calculations moving toward the Moon, and now that you actually took a mission that got that far, were there changes that you made in how you did work to prepare for the lunar landing missions?

BECK: No, things were so fast, and while Apollo 8 was going on, of course, we were turning out the planning products for other Apollo missions, the follow-on missions. After Apollo 8, we were concentrating on additional things like the lunar rendezvous capability.

WRIGHT: Were you able to use some of the first work that you worked on?

BECK: No. The early rendezvous work was only of historical interest. That document was sort of recognized by a few of the rendezvous people, but that was like, "Oh, that was done way back when, you know, so that's trivia." It was of interest though in that all of the computations were done on the Friden—prior to computers.

WRIGHT: Just a few years, but yet. [Laughter]

BECK: Yes, right.

WRIGHT: A lot had happened in those years, hadn't it?

BECK: And, of course, the work done at Langley was Earth orbit trajectory analysis, and it was very simplistic, in terms of accuracy of computation, because there was no atmospheric model or anything like that. It was really conceptual and had to do with the later phase of rendezvous trajectories to a Space Station. But I looked at that TN not too long ago, and it was sort of interesting to read in the context of the 1960 publication date.

WRIGHT: How much information were you able to use from the Mercury and Gemini flights to apply to what you were going to do for Apollo?

BECK: A tremendous amount of technology was transferred from the early programs into the Apollo mission planning tools. Earth models, atmosphere models, rendezvous planning and analysis tools, etc., were fed directly into the Apollo planning tools. And of course there was a direct transfer of knowledge and experience into the lunar trajectory section.

Something that was very complex, even complex today is to represent the upper atmosphere density, and how that influences the orbit of a spacecraft in low Earth orbit. Ed Lineberry and Sam Wilson (and others) developed analytic models to represent the atmospheric density in a simplistic model to avoid extensive integration.

The Gemini rendezvous techniques were used and the orbital/flight data from the actual vehicles were used to test our modeling (e.g., the Earth's oblateness). We used things like radiation profiles to get the Van Allen [Radiation] Belt better represented, and of course, the atmospheric density. Then up around the Moon, we took the flight data and improved the math model of the Moon to get things like what they called MassCons, which were mass concentrations that perturbed the lunar orbit. But postflight analysis was always done to perfect the mathematical models.

WRIGHT: Well, tell us about those fast days, that were really years, between Apollo 8 and Apollo 11, and how you and your group took that data and applied it to prepare for Apollo 11.

BECK: Well, like I say, it was really applying flight data. Emil Schiesser and his group were taking navigation data and preparing it and beating that against the models to improve fidelity. But we basically had by that time, I guess, validated the Earth-Moon system in our models and the perturbations and so forth, so that we knew that we had good representation. And of course, all of these things overlapped because of the long lead-time required to plan each mission.

A big job for the lunar trajectory group was supporting the lunar landing site selection activity. You can imagine, in the selection of lunar landing sites you had a lot of special interest groups that were interested in pushing their approach.

Let me get back to one other thing for a second. One thing that we had in those days which was very valuable, and it's a luxury that—we probably hated it at the time, but it was a real luxury. We had an organization at [NASA] Headquarters [Washington, D.C.] called Bellcom [Inc.]. I don't know whether anybody else had mentioned that name or not, but anyway, it was an organization of very capable people, and they had analytic capability that replicated what we did in MPAD to some extent.

One of our important contacts with Bellcom was a guy named Vern Mummert. He used to come down all the time and he would look at some of our analysis and maybe question it or challenge it. "This is not what we got up at Bellcom."

A lot of us resented that, and a lot of us recognized that it had tremendous value, because again, with the schedule pressure that you had, it was very important to catch an error, any fundamental error, very early, so that you didn't waste a lot of effort going down the wrong path. So if you had somebody looking over your shoulder that's saying, "Hey, this looks screwed up," or, "This looks iffy," well, it's really a very complementary type thing, as opposed to being something that you ought to challenge.

In those days, you were looking at mathematical concepts and mathematical simulations, and developing those out of intelligent analysis and not based on eons of experience from other groups or other software or whatever.

So it was a new frontier and you always scrutinized your own work. And knowing that if it's not right, you're in deep trouble, and the program is in deep trouble. Because a lot of people were relying on the information that you were generating. So validation was just extremely, extremely important, and everybody along the line seemed to really realize it.

Let's see. Where was I going before I got off on that tangent?

WRIGHT: The lunar landing selection sites.

BECK: Oh yes, lunar landing selection site. Yes, that whole process was really laced with a lot of special interest type groups. Before the landing, there was just tremendous controversy. This was years before, but tremendous controversy as to what the surface was really like, even though we had some experimental spacecraft that had gone, but nothing the weight of the LM, and some scientists were saying, "Hey, you get up there, and it's going to be dust six feet deep, and that thing's just going to sink."

Other people would say, "It's going to be so rocky and so rough terrain that it's going to tip," and so forth and so on. So you had just a lot of controversy.

Oh, I know. You were asking about what you did between Apollo 8 and these other missions. Well, as you went through this site selection process, we were the people that provided the analytic data for that selection, okay, and by analytic data, what I mean is the scientists would specify the lighting conditions that they wanted when you arrived, within a certain number of degrees, like the incidence of the sun on the landing so they'd have good visibility, and they, of course—the geographical areas, or the lunargraphical areas, that they were interested in.

We would try to fly trajectories there that were within the performance capability of the vehicle. That's why all the landing sites are within a fairly narrow band of the equator, because that's where your basic orbit was, and so you couldn't get up to higher latitudes. So they would provide candidate landing sites, and we would evaluate those for the time of year, the day of the month, and look at the lighting, and then look at the end of the mission reserves to see if all the mission reserves were well within the capability of the vehicle. So that was sort of our function.

So after Apollo 8 and during Apollo 8, we were busily generating all kinds of parametric data for the selection of the different sites for the follow-on type missions. [Harrison H.] Jack Schmitt supported us a lot. He was a geologist, and he supported our group in helping with the landing site selection and sending proposals forward.

We would generate supporting data. It would go up to the next levels of management, and they would look it over, and the various interest groups would be there including the geologists and other scientists, and they would battle it out as to the merit of that particular landing site. If they wanted to go somewhere different, well, we sometimes would have to generate data that would say, hey, you can't get there within our performance capability, and that's sort of how the process went.

After the site was selected we would generate a reference profile to go to that site, and it would be evaluated by the Flight Ops [Operations] people and the flight crew. So that site would get approved.

Then, we would go into full-fledged operational planning phase and generate the operational data, including things like the target parameters for lunar orbit insertion, etc. So that's about the way that process went.

WRIGHT: Were you asked at some point to compute what it would take to go to the back side of the Moon?

BECK: Oh, that's what I was going to get to, yes. Jack Schmitt was something of a maverick, and he had this little group that he'd pulled together and we met at the Lunar [and Planetary] Institute [Houston, Texas]. He had gotten the rumors already that after Apollo 17, there wasn't going to be any more Apollo. So he thought it would be just a very valuable thing to go to the back side, and so he wanted some of our expertise to help assess the feasibility. So we met for a while, but then I think management got wind of it, and it was severed. [Laughs]

WRIGHT: Tell us about your personal experiences watching the landing on Apollo 11, your calculations being so precise that they were able to get where they needed to go.

BECK: Well, of course, it was infinitely gratifying, but from a collective perspective. In other words, it wasn't as much a matter of personal accomplishment as it was an Agency accomplishment. It was just extremely gratifying that the whole thing came off, and of course, a great deal of pride.

But looking back on it, certainly for me it is hard to even think in terms of personal accomplishment, because it was such a group activity, and you had such phenomenal leadership. Now, somebody like Glynn Lunney and Chris Kraft and [Sigurd A.] Sig Sjoberg and Bill Tindall and John Mayer, if he were here, and Carl Huss, well, I think they could look back on it and almost say, "This was partly because of me."

But somebody at my level, even though I had unique expertise, I am sure that had I not been there, somebody else would have done it. It was that sort of thing. But, I have other friends my age that have had other careers, and there's probably nothing comparable with the early days of the space program, in terms of a real opportunity to do something interesting and unique and pioneering. It really was a tremendously gratifying experience.

WRIGHT: A different type of reward for the agency and your family, other than the Apollo landing, was, of course, the recovery of Apollo 13 and bringing that crew—

BECK: Yes.

WRIGHT: Could you share with us what your involvement was with that rescue?

BECK: Yes, we can do that. Can we take a break?

WRIGHT: We can.

[pause]

WRIGHT: —Apollo 13. Were you ready to move on to there, or was there anything that you want to talk about?

BECK: I think, one thing, we do have to go all the way back to Langley after a while.

WRIGHT: Okay.

BECK: We hadn't talked about the MPAD parties yet.

WRIGHT: All right. We've got to talk about that. I'll make myself a note.

BECK: Apollo 13, the entire Agency was of course involved in that. As soon as there was an indication there was a problem, the back room activity got very, very intense. All of the "what if" type considerations and concerns required analysts to do trajectory computations including when should you do a midcourse correction, what type midcourse correction, and what systems would be utilized for it and all that sort of thing.

Obviously the center of gravity for that activity was in the Control Center. That's where the decisions were made, and that's where our management basically was. But the back room activity was intense—that's where the "what-if" computations were done. That information was fed to the managers who were asking the questions, "What if you did such and such at such and such a time?"

Generally that would require a lot of activity and a lot of computation to answer those questions, so that was an intense time and a very active time for everybody involved, because there were analysts all over the country. Especially the systems people, the hardware people themselves were working to see, well, what system had failed and exactly how did it fail and how did that jeopardize an adjacent system.

WRIGHT: Were you constantly being asked for inputs, or was there a certain time in the recovery activities that your group became more vital to bringing the crew back home?

BECK: Early on like as soon as the incident, well, then, we started running trajectories, return trajectories, to see exactly what kind of profile you could use and what midcourse corrections were you able to do, etc. Then at a certain point the attention was really in the Control Center, because they were evaluating systems and potential system failures downstream, and so you'd almost done all that you could do about a trajectory. In other words, it was on its way home, and the state of the vehicle was the question mark, and so that had to do with the systems people and the flight operations people.

WRIGHT: Did you have any thoughts that after Apollo 13 that the Apollo missions might be put on hold for a while?

BECK: I think that I at that time, maybe naively, thought that the success of the other missions, and even the success of the return, you would not jeopardize the other landing missions, because there was so much, so much inertia there. Things were going very rapidly, and I just couldn't imagine that they would—I didn't even think they would cut them off after Apollo 17.

Neither did Jack Schmitt, which sort of leads to another subject, too, with Jack, is that Jack and I were good friends. I lived over at the Bay House Apartments at the time, and he was a neighbor. So he and I used to have these long philosophical discussions about the space program and so forth. We both had the same pet peeve, that we couldn't believe that NASA, that the agency, and that the administration, could be so shortsighted as to not have a long-range space plan. By long-range, I mean like a thirty- or forty-year plan, and really have careful building blocks as to what you wanted to achieve.

So Jack was terribly disheartened at the cutting off of the Apollo missions, and I think that was part of the reason that he ran for Senate, because he thought that he could go to Washington [D.C.] and make an impact in terms of getting a long-range space program established, with a long-range budgeting process rather than this shotgun-type stuff that we're still doing today.

WRIGHT: How did the environment and the dynamics change after Apollo 13 and as you moved through the other lunar landing missions? Then, of course, when you heard that the program would be canceled, how did it affect the work?

BECK: Well, of course, it was just about devastating. There was a quietus there, a little bit like today, as a matter of fact. Things were just terribly uncertain, and it was the beginning of the political problems that NASA has faced all this time. There was some work going on relative to a Space Station. There was no rhyme or reason to it. There was no long-range planning. There was nothing laid out properly.

But after Apollo, we got involved with the concept of the Shuttle, and a lot of conceptual planning was going on there. I started working on what was called utilization planning for the Shuttle. I was not involved with ASTP [Apollo-Soyuz Test Project] and Skylab. I didn't have anything to do with either of those programs, or very little to do with them.

So I started addressing the concept for utilization planning, and what that really means is how does the agency really utilize its resources to accommodate what needs to be done with the Space Shuttle. The Agency decided that they were going to sell the Shuttle based on economic return, and they talked about a hundred Shuttle flights a year. So our group—when you start thinking about that, number one, just the average person which has good common sense would say, "That's stupid" and that's what it was.

Well, our group—oh, Gene Kranz and company, they were saddled with the responsibility that said something like, "How would you accommodate flying that many flights? Like that's two flights a week." Of course, if you even trim that down and say one flight every two weeks, see, that's ridiculous. You could just do a back-of-the-envelope calculation and show how much liquid oxygen that would be required to go down to the Cape; you'd have to have a train continuously running for the launch vehicles, not to say anything about all the payloads that you'd need to develop.

So anyway, we formed a group, and we coined the phrase, "utilization planning," and what that meant was the accommodation of the heavy flight rate; how do you do that? And that included flight design type work, the payload planning, payload development, launch facility preparation, all the logistics work, etc. Like how do you make everything flow together, okay?

So I started working on a planning concept, which related all the activities between JSC and Marshall and the Cape and the payload community and—well, those are the primary ones; there were some others, but they're the primary ones. How those organizations and those major groups would interface and what they would have to do in order to accommodate a high flight rate. That had to do with data transfer, like who sent what information to whom, etc.

So this utilization plan, it was an extensive effort, almost like a logistics plan is what it really amounts to. So we'd have to go Washington every two weeks or something like that and report to management up there how you were going to do this. Well, it was an awkward position for me to be in.

"This is totally unrealistic," I said, "If nothing else, let's say that you're talking about fifty flights a year. Look at the payloads. Where are you going to get fifty Shuttle payload bays full of payloads?" Just on the back of the envelope, I'd say, "At so many dollars a pound for payload, that's—." And at that time, it was sixty thousand pounds in the payload bay. I'd say, "You're talking about this many pounds of payload per year at this much money." I said, "It would take the—."

Their response up there was, "Hey, this group is not addressing the cost of the payload. We're just addressing flight utilization, how you utilize the Shuttle."

"Okay." So we went on and on and on and on.

Eventually, this evolved into a serious utilization plan, but it really amounts to the fact that if the Shuttle were successful and you flew ten flights a year, well, here's all of the interactive work that had to go on across the Agency to support ten flights a year. So this was how do you do integrated planning.

We developed these long wall charts of how all this information would have to flow, who would have to do what at what period in time prior to launch, etc. We worked with the DoD [Department of Defense], which was involved in a similar activity out on the West Coast. That was a career that took up several years. There were several good things that came out of it, and there was a lot of garbage that came out of it.

But one of the good things was that within MPAD, it really pointed out the need for an integrated planning system and good data management and good data configuration control. After all, we were into the computer age (almost). That was when we developed the concept of what initially we called the Mission Planning System, and then I think later on it was called an Ops Planning System. But anyway, what it did, it included all the disciplines within MPAD, plus crew activity planning. It included flight dynamics, consumables, crew activity planning, robotics, etc. All of the discipline specialists would use the same integrated planning system operating form a central database.

You were asking a while ago how you transferred planning information. Well, the solution to that in the days of the computer was to do it with an integrated system, and everybody operated from the same database so that you transferred vectors and all other data automatically. You had configuration management over this, and operated from a relational database. You would also have configuration management over all the software tools that you were using.

In the old days we were just lucky that everybody worked so hard and they were so conscientious, because we had no configuration management of the software. "Joe Blow" took a piece of software home at night and worked on it and formulated something that he needed, and he validated it; he checked it; he wrote it; he used it; and he transferred the information to another organization, and nobody was in position to question whether or not it was properly computed. Okay? And that was a successful process in those days because of the mindset of the people doing it. Today it would, of course, never work.

We developed this concept of the Mission Planning System, and we started integrating the different pieces into a single unit, into a single database, and that went on through the early Shuttle days and then up to the end of MPAD in 1990. Then that system was transferred over to Ford Aerospace [and Communications Corporation], and when MPAD was dissolved, I decided, "Well, this is a good time for me to quit NASA." I wanted to follow the Mission Planning System, which had been transferred over to Ford Aerospace for development.

Pete Frank was then working for Ford. I went by Pete's office one morning, and he started showing me some things. So they had already transferred the work of the Mission

Planning System over there, and so I was talking with Pete, and he encouraged me to come to work there.

So I had taken the Mission Planning System and I transferred to Ford. The prime customer for the development of the system was Gene Kranz in the Operations Directorate because MPAD had dissolved. So we had changed the name to the IPS, Integrated Planning System, and so that to this day is still being developed and is being used for the Shuttle activity.

So then Ford got changed over to Loral [Corporation], and then Loral into Lockheed Martin [Corporation]. I finally retired from Lockheed in 2000.

WRIGHT: How well did it work with NASA with that system going to a contractor? For so long you had been part of the government.

BECK: I think the contractor did a very good job. For me it took some "getting used to"—working for a contractor rather than NASA. But it worked well.

WRIGHT: Have you been involved in what they refer to as the Space Transportation Architecture Studies?

BECK: Yes.

WRIGHT: Could you share with us your experiences with that?

BECK: That was one of the encouraging periods in that era following Apollo. The STAS activity was by presidential directive, and what it really said was that NASA and DoD need to get together and come up with a plan that would give this country assured access to space, and that should be economical and reliable. The system should satisfy the space transportation needs of both NASA and DoD.

That started out as a two-year, $27 million study. There were four contractors involved, and it was managed by both NASA and the DoD. Head of the committee on the DoD side was the Secretary of the Air Force. On the NASA side was a manager from Langley. JSC, Marshall, [NASA] KSC [Kennedy Space Center, Florida] and NASA Headquarters were all involved. It was called a "racehorse" contract.

Four contractors were selected. They were Martin [Marietta Corporation], Boeing [Airplane Company], General Dynamics [Corporation] and Rockwell International [Corporation]. The study was initiated in 1985. The contractors were all given the same objectives for the study. The study included an overall plan plus operations and launch capability, logistics, vehicle design, technology development, etc.

So they were all given the same goals and the same schedule. The NASA/DoD committee would periodically review interim progress. That would involve periodic round robin trips to review progress at each controller site. Many hours were spent flying and reviewing tons of data.

But it was really a significant comprehensive effort. It was the first step that I'd ever seen for coming up with a long-range plan for what we ought to be doing in this country for a space program.

But what was very strange about it was that after two years, the study was terminated. The study was almost complete when we had the *Challenger* accident. And it was rumored that there was some high level disagreement between NASA and DoD.

In the study, the contractors had evaluated all of the different types of vehicles, including flyback boosters, expendables, etc., with a whole stable of launch vehicles, all the way from a low-lift capability to the full-fledged heavy lift for putting a total Space Station in orbit in one fell swoop. Had we been able to use a heavy lift vehicle for the Space Station we could have put the thing up in four or five flights and assembled it in orbit.

WRIGHT: What type of experiences, firsthand experiences, did you have working with the Department of Defense? Were you involved in any of the—

BECK: Oh yes. Very interesting.

WRIGHT: What can you tell us about that?

BECK: While I was doing some of the utilization planning in the time period after Apollo, the DoD was active in the utilization of the Shuttle. They had a number of payloads that they wanted to use the Shuttle for launching. We had interactive team meetings with the DoD, and they would send representatives from their different programs here to talk about Shuttle utilization and how we did the paperwork and the NASA/DoD agreements. We would develop a joint mission plan for a particular activity, and we'd show the interface between the payload and the Shuttle, and what had to be provided by the Shuttle to accommodate the payload, and what constraints there were on the payload operation, etc.

So we would have technical interface meetings with all the technical disciplines. It looked like for a long time that the Shuttle was really going to be utilized, and then there was a political decision, I guess, later on, that said that it wouldn't be. So now there's almost no DoD activity here at JSC.

But besides the regular Shuttle support activity, there were several higher level DoD payloads that came in that wanted to use the Shuttle, and so a handful of us were put on those teams to review the requirements of the payloads. It was interesting to see the difference between the way the DoD operated and the way NASA operates, in terms of their management structure, etc. We had a lot of interesting and sometimes heated discussions with the DoD, because with some of them, you got the idea that they weren't that enthused about utilizing the Shuttle. Then at other times you thought that they, different groups, were sometimes very agreeable and very anxious to use it. They were always concerned with the security aspects of it.

WRIGHT: What type of measures did you have to take to be part of that specific group?

BECK: For some of the select payloads, the security measures were very constraining. All the local meetings were held in a little vault room that was specially shielded and equipped over in Building 1. You went in behind a secure lock, and you couldn't take any notes, or you could take notes, but they had to be left in the room. You couldn't take any information out, and of course, you couldn't discuss it with anybody. That created some awkward situations with our relationship with our managers.

WRIGHT: During those days you spent planning, did you get involved at all in the early days of the station planning?

BECK: Only with the development of the integrated planning system, but not in the launch planning, or the assembly. I did do some assembly sequence evaluation but never really got into nominal mission planning. But I was following all the planning activity to see what the requirements imposed on the Integrated Planning System, the software that was being developed.

WRIGHT: Was that prior to launch?

BECK: Yes.

WRIGHT: Have you worked on anything regarding the station since it's launched or been a part of any of that?

BECK: Not to any extent.

WRIGHT: You have mentioned off and on during the session about the computers and how they changed. At what point did NASA start to have enough computer power to do the work that they needed, where you weren't having to go down to the University of Houston or the Medical Center, to stop using theirs?

BECK: I don't think that we got computer capability, at least our group didn't, until we came to MSC [Manned Spacecraft Center] on NASA Road 1, when they built the new building, and that was probably [19]'64. Perhaps the engineering group had a computer prior to the move to MSC. But we really didn't get the computer capability that we needed until we came down here. I believe that we had the 1620 up at HPC, but all of our work, and especially in the lunar area, HPC was not nearly adequate to do that.

WRIGHT: How was your area impacted by the change of technology, especially in the computer world, over those next years?

BECK: It was impacted tremendously. It's still amazing to me. I remember when they got to 128 kilobytes, that we thought that was a lot of memory, and that was probably adequate. And it's still amazing to me that we could have put a lunar landing program on a 128-kilobyte machine. Of course bigger and faster computers simplified the generation and documentation of planning products tremendously.

I remember when—and I don't know what year that was—the IBM 360 came out, and that was a real big machine. I always thought that a tremendously interesting story would be the history of the computer within NASA, and how we went from one capability to the next and what the impact had on efficiency.

WRIGHT: What other duties were you assigned during your career with NASA that were a bit unique? I believe one that we learned about was that you did some reporting to the International Aéronautique Federation.

BECK: The Fédération Aéronautique Internationale. Yes, that was a fun project. It was quite interesting. This organization is, of course, headquartered in Paris [France]. It's an old, old organization. They started keeping balloon/aircraft flight records way back when; I guess in the 1800s. So any long-duration record, they would validate and document. I don't remember what year now, but probably in the sixties, I would guess, they decided to include a section on space flight, though their prime interest was aircraft.

There were delegates from a number of countries. There was Germany, and of course France, Great Britain, Spain, Russia, the United States, and maybe some others. They met every April for a couple hours and representatives from the different countries brought documentation records of their space accomplishments for the past year. Starting out in the early days in [19]'60, Carl Huss was the representative from the United States. Carl quit, and he elected or appointed Rod Rose, and Rod was the representative for some time. Then when Rod quit, Rod appointed me.

The first time, I went over there with Rod Rose, and I met all the delegates. The Secretary of the organization was French. He and his wife and been federation members for many years. His wife was a fascinating lady. She was very active in the French underground during the Second World War, and she was a pilot herself. So just a fascinating lady. The meetings were held in the Federation Headquarters building in Paris.

The Federation offered three levels of awards, and the delegates would vote to decide which accomplishments would get which award. Usually it was for a crew member, for some outstanding achievement. During my term the United States was in an excellent competitive position because of the outstanding achievements during Apollo. We, of course, secured a number of awards.

WRIGHT: How many years did you do that?

BECK: I think probably six or seven, something like that. Quite a few.

WRIGHT: Did you have any other unique assignments that stand out?

BECK: I don't believe. Not really.

WRIGHT: You mentioned earlier you wanted to go back, all the way back to Langley, and we understand your reputation for hosting some parties.

BECK: Oh yes, the parties. [Laughs] Yes.

Well, I don't know how the tradition got started, but up at Langley I had a house out on Chesapeake Bay on Windmill Point, and it was a perfect party house. Several months before the Space Task Group moved to Houston we had a big party for the mission planning organization. They all came and we had a real party. That's how the "tradition" got started, and we continued it through the life of MPAD. We got together several times a year for a good time

MPAD was always a very socially active group. In fact, that was probably the outstanding characteristic of the Mission Planning and Analysis Division; it was so much like a family. It still is today. It was dissolved in 1990, but members of the group still get together for

MPAD luncheons every two or three months. We usually have about sixty people show up, and that's pretty good since it has been about fifteen years since the organization was dissolved. Most organizations don't have that many show up for social events while they're active.

But it seemed that once someone joined the group they seldom left. That was probably true all over NASA. They just stayed and stayed and stayed, so everybody got to know everybody very well, and they got to be like family. Really, a congenial group. That was one thing that made the work interesting. It was always a close team effort.

WRIGHT: Did you have many of your group go back to Virginia after you moved to Houston, some that just didn't want to work here?

BECK: Perhaps, but I don't know of many who did. But in the early years employment in the Space Program was such a wonderful opportunity that I doubt many would have given it up unless there were other circumstances.

WRIGHT: Before we close today, we'd like to ask you just share with us some of your most memorable times. Maybe when you think back on the times that you spent with your NASA family, what are some of your most favorite experiences or memories that you have that you'd share with us?

BECK: There were of course many. One of the most memorable experiences was Apollo 8. I am not sure exactly why—maybe it was just the circumstances of the day. Of course, for everyone, the first lunar landing was truly incredible. But also the early sub-orbital and orbital flights were exciting too. The unknowns were so overwhelming.

Of course the tragedy of the fire was very memorable. That was a horrible thing. It was a reminder of just how vulnerable we really were, and how we could make mistakes. The spacecraft was filled with errors and design flaws. The tragedy was a red flag emphasizing the importance of proper procedures and thorough testing. As bad as it was, it definitely contributed to the success of the Program downstream. Then the *Challenger* accident was indescribable. That was at such a rough time, anyway. I knew the crew personally. That was just so incredibly shocking.

WRIGHT: Where were you working or what area were you working with the *Challenger*, for the *Challenger* accident?

BECK: I was involved with the STAS activity and was working Shuttle Utilization concept development.

WRIGHT: Did the results of that investigation or the Rogers Commission [Presidential Commission to investigate the *Challenger* accident], did that affect any of the work or the studies that you were working on at all?

BECK: Only indirectly. It was really so hardware oriented that—of course, it emphasized the fact that everything needed to be checked more carefully, but by that time I think the spirit that was there during Apollo had somehow eroded. There was a totally different environment within

NASA. But there was still the NASA family view and feeling. It was such a terrible tragic accident. It was totally devastating to most people that I knew.

WRIGHT: What would you consider to be the most challenging time that you had?

BECK: I guess probably the most challenging time for our group was in developing the lunar capability. The translunar injection iteration tool that we were developing was a very difficult thing to get operable and to get reliable. It was so terribly sensitive. It was a massive iteration technique that had to be developed from scratch. To tune that software and get it to function was a very frustrating job and took a lot of midnight hours, and a great deal of precious computer time. Until the final phase of development it was never evident that it would work. We were always faced with the possibility that it may not work. And if not, how can we generate a lunar trajectory? That was challenging.

Another challenge was with the technology development associated with the STAS activity. It had to do with the NASA mindset at that time. What was very prevalent, and is still prevalent in the agency, is competition between Centers. I thought that some of the things that needed to be done were so logical and why that wasn't acceptable to the different centers and the different organizations and to management, I never did understand.

During STAS we had made a lot of progress in trying to get rid of "shotgun technology." Initially, we just had little groups all over NASA. Whatever they thought was a good idea, they would develop it, and they would get funding to develop it. What we tried to do across the agency with STAS was develop a coherent plan, such that we would lace all of this technology together and not shotgun it to death and not replicate efforts across different Centers.

We were making a lot of progress, and then all of a sudden it just blew up, and it had to do with the NASA mindset. You would have people from a center say, "Hey, this is not your business. Stay out of it." And that still goes on today, and it will probably always go on.

Then the other frustrating thing is, of course, the growth, the rapid growth of bureaucracy. Today, the decision process is very sluggish. We were making decisions back in the sixties and early seventies at the section head level that now are taken all the way up the chain.

You were talking about how you communicated. Well, you did it very informally, and decisions were made quickly and authoritatively with good judgment by good leaders, like John Mayer and Carl Huss and Bill Tindall and Chris Kraft and Glynn Lunney and Leonard [S.] Nicholson, Gene Kranz, [Maxime A.] Max Faget and Sig Sjoberg (and of course others). And they didn't have anybody to answer to, because of their capacity themselves. The bureaucracy just didn't exist, because we didn't have time and it wasn't tolerated.

But today, it's just like the space program has no thread of reason or rationale, and when you talk about challenges, that's the challenge. I don't know how you get around that with today's political environment.

But I firmly believe that if you really want technological excellence in this country, you need a focal point for it. A long-term space plan provides that focal point. The space program is a natural. It's not what you get out of the space program, but it's the fact that you have a focal point for the people and for young people coming up and people that are getting an education and growing in engineering, to say, "This is something that I'd like to be a part of." They don't have to be mainstream; they can be peripheral, with companies or whatever.

It is frustrating *not* to see a logical step-by-step, long-term space plan in place. It is frustrating to hear about the emphasis on going to the Moon and Mars and not see a working plan in place to assure us reliable access to low Earth orbit. We should have been working on a Shuttle replacement years ago as a stepping stone to future activities.

WRIGHT: Are there any stories or experiences or anything else you'd like to share on other people? We've had quite a few people talk about things that they've done with you. I just didn't know if you would have anything else you'd like to share.

BECK: We talked a bit about the family environment within MPAD. It was all really a hardworking group, but they sure played hard as well. We had our share of good times at the local bars and, of course, the happy hours and the postflight parties, etc. They were all phenomenal, and there were many, many stories related to those, that's for sure.

WRIGHT: And so many of you stayed and watched that dirt road that you came down first change into quite a big area, didn't it?

BECK: That, of course, was an amazing experience, to see this area grow the way it did.

WRIGHT: Well, we thank you for coming in and talking with us today.

BECK: You are certainly welcome. It was fun to reminisce. You certainly have an ambitious project. Good luck!

[End of interview]