NASA JOHNSON SPACE CENTER ORAL HISTORY PROJECT
ORAL HISTORY TRANSCRIPT

WALTER W. GUY
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is October 30th, 2006. This oral history is being conducted with Walt Guy in Houston, Texas, for the NASA Johnson Space Center Oral History Project. The interviewer is Rebecca Wright, assisted by Jennifer Ross-Nazzal.

We thank you again for taking time this afternoon to sit down and talk with us for this project. We'd like for you to begin, if you would, just share with us how you first became interested in space and/or how that interest led you to become part of the space agency.

GUY: NASA was officially formed in '58, and I graduated in '59, and in that interim period there was an aggressive recruitment program. I think it was aggressive; it was at the University of South Carolina [Columbia, South Carolina], which is my undergraduate school. It may be much broader than that, maybe not. I don't really have any basis of making a judgment.

But it was fairly aggressive, and the interviewer who came down was a fellow named Bart [E. Barton] Geer, who was a section head in the facilities side of Langley [Research Center, Hampton, Virginia]. His section was the Systems Engineering Section. He was a very dynamic individual, capable of creating a lot of interest in his area and the research facilities that he was involved with. That was really my first introduction to the practical side of NASA. You know, the newspapers had the fact that there was a NASA, and the Russians had just flown their Sputnik. But really it was the personal influence of Bart Geer that made me decide that that's where I wanted to work.

It was my worst offer, but I guess those things don't matter in the big picture. Until the interview, I had no real direct plans for going to NASA. I had interviewed with GE [General Electric] and Pratt & Whitney and IBM [International Business Machines] and Bell Labs and, oh, everybody. So I was not committed until after the interview.

It was a little strange—this doesn't have anything to do with history, I guess—but when we reported in at Langley, the agreement I had when I accepted the job is that I would work for Barton Geer. Of course, the Human Resources doesn't care about any of that kind of stuff, so they had this other assignment for me. But I said, "Wrong. I'm not swearing in until we get that settled."

They scurried around for about an hour or so and then came back and said, "All right." But it was an opportunity at the time, I guess, to be sidetracked, but I wasn't interested in being sidetracked. It all worked out great. He really was a good supervisor and had a lot of good work. I worked for him for a couple of years.

In that same time frame the Space Task Group [STG] had been formed, between '58 and when I got there, I guess, and then they were basically working Mercury. It wasn't long before that sounded like an interesting path, and I changed to the Space Task Group, and then later on it was decided to come here, so that's where we came.

WRIGHT: Did you have to apply, or did you just have to show interest to become part of the STG?

GUY: Actually, the system was pretty functional at the time. It wasn't really encumbered with a lot of bureaucracy. The Space Task Group had authority to grow, and you would go find somebody that was doing something you were interested in, and if they decided that they wanted to add you, then it just sort of happened. I don't really know any of the background.

Several of the people in my office, Systems Engineering Section, that I was in, went to Space Task Group, and so there was a direct path there. Jim Saunders [phonetic] was one. Bob [Robert] Parker was one. Dick [Richard B.] Ferguson was one. Saunders went to [NASA] Headquarters [Washington, D.C.] and never came here, because his family was in Maryland. Bob Parker came here and then went back. Dick Ferguson came here and stayed till he retired. Oh, and there were others I can't recall. I don't think that was all in my section. It may have been all in my section; I'm not sure.

WRIGHT: The work that you were doing for Mr. Geer, was that similar to what you went to do or was it totally different?

GUY: No, not at all. I was doing the pneumatic hydraulic controls for research facilities. Of course, I'm a mechanical engineer, so when I went to Space Task Group, my job was thermal control systems, which is still a discipline within the mechanical engineering area, but I was not in the facility part of the business at all. But pneumatic hydraulic controls have fluid flow, and it's the same general academic principles, it's just focused differently.

But, of course, spacecraft were a new thing; thermal control for spacecraft was a brand-new idea. We had to sort of figure out in space how are we going to get rid of all this heat. Mercury used a water evaporation system, just like you're out in west Texas; you can have a—I don't know what they call them these days, but we used to have a name for them.

ROSS-NAZZAL: Swamp cooler?

GUY: Swamp cooler, I guess. But that same principle works in space, that you can evaporate water and the heat that it takes to evaporate the water, you can use that heat as a rejection of whatever heat you've generated. So that's fairly straightforward, but water is heavy, and for long missions, that's not a very good answer. The concept of a space radiator was really the only other way to get rid of heat, and that was what I worked on, the first radiator systems for the Apollo spacecraft.

WRIGHT: Can you share with us how the group that you were working in exchanged ideas and how those ideas went into concepts and the concepts went into designs and development? Kind of, hopefully, share details of the process to kind of give an idea of the thought processes that turned into the testing area.

GUY: This is a very small group of people, and the section—I think it was a section; could have been a branch. I'm not sure; I think it was a section, but I'm not sure of that. That section was responsible for basically most of the spacecraft systems. We had propulsion people. I was thermal control. Jim Saunders I mentioned; he was environmental control system. Bob Parker was the fuel cells. Propulsion side, Chet [Chester A.] Vaughan was in the propulsion area. The landing system was John [W.] Kiker, [James] Kirby Hinson. Harold Benson was in the landing systems. There was a small group of people that covered all the areas, and we all were together.

Each person was responsible, and occasionally there were a couple of people in an area, but it was not uncommon to have one person be all there was. In my area it was only one. It was not a time of really having five or six people with ideas on how to do each job. The expertise of the group was used for integration. We were pretty heavily systems engineering focused, so basically our job was to create a spacecraft out of all these pieces.

It was [Maxime A.] Faget's division, and we had within Faget's division a group of people that did the integrated vehicle stuff. I guess Caldwell [C.] Johnson was there, and maybe that was Owen [G.] Morris'—they probably were branches in those days. I guess I was in a branch, and what I'm talking about now was another branch. I think it was Owen Morris that was maybe head of that branch. I'm not sure of that. I know Caldwell was in it, Will [Willard] Taub, a lot of good vehicle-level people.

Of course, this branch that I'm talking about had all the systems, like the propulsion system and life support system and all of the different systems. I've really forgotten what else was in the division. It was called Flight Systems Division; that's what Faget's division was called. I don't have a good mental picture of the total organization; I don't know what else was in there. My particular organization was headed up by one of the engineers that left Bart Geer's section, Dick Ferguson. Basically I worked for Dick, and all the systems worked together, again with the vehicle-level branch, to work out the details of a vehicle design.

I think later, a lot later, after we came to Houston, where we sort of grew up, instead of having two or three people doing propulsion, there was a division doing propulsion and power and fuel cells, that kind of stuff. I don't remember how big it was—a hundred people or so, maybe larger. From that point forward, of course, there was a lot of collaborative design activity going on, but the Apollo vehicle was basically designed pretty much by a very small, small group from the government side, and then that was put into the statement of work, and then North American [Aviation] won that.

When they won it, it was a lander; everybody thought it was going to land. That's the reason my radiator job was so hard, because when you are on the surface of the Moon, it can be very, very hot. I was all concerned about how to protect the radiators from all that heat while getting rid of heat. Worked on several designs; for the surface we had to add a vapor compression system, which is like your home air-conditioner, because we couldn't reject the heat.

It's like Houston. If you want your house seventy-five, and you're trying to reject the heat to a hundred-degree outside, you have to have some help. It's not going to go that way naturally. Heat goes only in one direction, from hot to cold. It does not go from cold to hot.

When the statement of work went out, as I said, the Command Module/Service Module, landed on the Moon, and we had to have a radiator system, a heat rejection system, that would work. Shortly after we came here, [John C.] Houbolt, I think was his name; I don't remember. Was a Langley, probably, GN&C [Guidance, Navigation, and Communication] wizard; I don't know. I never knew him. But he proved that lunar orbit rendezvous with a lander was a much better approach, so in midstream they agreed to change their mind and modify the contracts and started over again. The Lunar Module was born.

During that phase we did a lot of work. When we came here, as I said, we turned into divisions. I ended up in the Life Systems Division. Life Systems was recognition that the division had all the medical staff and all the doctors, and it had all the engineering for the life support and the thermal control. It also had the spacesuits, both the vehicle spacesuits and the lunar surface spacesuits. It had quite a broad responsibility. It was a very large division, several hundred people. Over time they sorted and moved the medical people out. We had the seats.

We had all the medical equipment. We had the crew equipment. We did the clothes, shavers, and toothpaste and whatever else. It was a large division.

By that time the organization had sort of centralized itself so the branch that I ended up in was a branch that had the life support and the thermal control. As I mentioned, Jim Saunders had the environmental control, but he went to Washington, so it came here. I sort of took over the whole area of thermal control and life support for the advanced [missions], looking at the future spacecrafts, looking at the Lunar Module, and doing the engineering trade studies and everything.

We developed a set of subsystem followers; subsystem managers is eventually what they were called. But it was technical people that basically monitored what the prime was doing, what North American was doing. We still have them today, subsystem managers, but that was sort of a new thing at the time. We were all in the same division, but I was sort of an institutional resource to those subsystem managers.

The radiator system and the environmental control was grouped on the Command Module, and Frank [H.] Samonski had that. The Lunar Module, Dick [Richard E.] Mayo had that. I did the analysis and the trade studies and built—of course, computers were a little on the new side in those days, but we built computer models of all the systems and did the analysis and evaluation. Of course, for all testing you do pretest predictions and then posttest analyze the data, that's the kind of work that I did.

I had a section within that branch, and there were two other sections. There was a Lunar Module Environmental Control System Section under Dick Mayo. I think they were all sections; may have been groups. We went through a lot of different names. They had units and groups and sections, and eventually we got rid of sections. I don't really know what the deal was. But I was lead for this analysis trade study activity, and it was a Lunar Module and a Command Module. We also, I think, had a Test Group. I'm almost positive we had a Test Group within that same branch, where we did testing of the environmental control systems.

Organizations were a lot larger then, because the theme was a branch had two or three sections, and a division had three or four branches. So a couple hundred-person division was not unusual. That's all collapsed down now. Of course, we don't have sections anymore. We go through cycles as to what size a branch ought to be. I guess we're back on the smaller size now, which I think is the right answer.

Anyway, the real challenges that we faced early were associated with following the design activity of the prime contractors and ensuring our project managers that what was happening was again of the fidelity needed to build a spacecraft. We were sort of the technical conscience for the program. We had no direct authority in terms of we couldn't go out and spend money. Only the program manager could do that. But we were sort of the confidante of the program manager; we were insiders.

Since we had done a lot of the precursor work, that put us sort of a step ahead in terms of monitoring the contractor, and then we did a lot of in-house testing and a lot of in-house analysis, and it kept us tuned. We were an asset even to the prime. Now, occasionally they wanted to make money and deny what we were saying is right and see if they could get the program to pay for what we wanted. But, in general, I think the program managers were, from my view, they were very astute, and I don't think that the government got snookered very much. I do think that there was more money then, maybe that was just all an illusion, and I couldn't see any different.

But on several occasions we'd end up telling the project manager that something wouldn't work or that the design was not going well or needed some more testing and whatever, and you'd get a little gamesmanship on the contractor's side, because they wanted to be paid for this new job that we just thought up. Of course, the challenge was to say, "You can't get the job done without it. You've signed up to do the job. It's not new work; it's old work." There was a lot of that that always happened. But, in general, I think we were treated as insiders; I know we were treated as insiders by the project manager, and I think we were treated as someone who couldn't be ignored by the prime [contractor], also.

WRIGHT: Do you recall an incident or an event when you were testing that, of one of the things that you just described where you called something to the attention of the prime and maybe even the process. Did you have to go to the program manager first, and then they called it to the attention of the prime, or were you involved?

GUY: The best way to work, of course, is just to get the prime to agree that whatever you think is necessary is necessary, and then it all gets done, and you don't have to talk to anybody. But if their management believes that they can treat it as added scope, then, of course, they try to treat it as added scope. But the first step is always just at the working level. We traveled, oh, probably once a week is not uncommon, and once every couple of weeks was almost always. We were there a lot.

WRIGHT: This is out to California or to other places?

GUY: Both. I went to Bethpage [New York] and California. It depends on which one I was supporting. Downey [California]; it was Downey in those days.

An example, there were, I guess, a lot of examples of suggestions or requests or inputs that we made that were accepted, and I don't know that I could come up with any specific one. There were thousands of them, probably. But on occasion we'd come up with what we thought was something significant, and the contractor would not agree to it. I guess maybe one of the earliest ones of those, I mentioned earlier that water evaporation was a technique that was used, and the generic term was water boiler.

Apollo had a water boiler. It had a problem, in that the way you control the water boiler is you control the pressure inside the water boiler. If you keep pressure like in this room, you can put water—there's your water. You can open that; nothing will happen. All the water will stay in there. If you start reducing the pressure, at some point that water will start boiling. You can control whether it boils or does not boil by the back pressure. The dilemma occurred, though, is that you have to keep feeding water to this water boiler, because after it evaporates, then it needs more.

Well, that turned out to be a problem, and the solution that [North American's] subcontractor—AiResearch was their subcontractor—had really never would have worked. They tested the boiler upright, like your water bottle there is. If you lay that water bottle down or turn it upside down, that water is going to come out. They said, "That's an unfair test. If you turn it upside down, that's an unfair test, because gravity is pulling it out, where in space there's no gravity."

Well, we did the analysis, and we said, "It doesn't matter. In space it's going to come out anyway, and now and then it's going to freeze up, and it's going to block the duct with ice, and then the water boiler is dead. You can't use it anymore."

They disagreed; said, "No, it won't happen."

We told the project manager—it was Joe [Joseph F.] Shea at the time—that it's not going to work.

He said, "Well, what's it going to take to work?"

We said, "Some design mods [modifications]." "But," we said, "they're not interested in even working the problem. The only way to prove that it will work is to test it upside down."

Of course, the contractor was saying, "That's ridiculous. We can't test it upside down. It will never work that way."

We got into this big, long argument, and they didn't want to change anything. They said, "If you want it changed, direct us to change it."

It turned out that schedule was also getting to be a big problem for us. Of course, Apollo was a very tight scheduled program. We were trying to get to the Moon before the end of the decade, so it was a very tight scheduled program. I don't really know all the details, but it turned out to be a big deal in terms of the amount of money and the schedule margin that we'd be giving away. If you just believe the contractor, you can just keep marching along. If you don't believe him, then you have to start over, get a design that will work, test the design. It really put a real burden on the system. Again, I don't know all the details. I was young and green.

But anyway, Joe Shea said, "We've got to go to Washington." The subsystem manager, Frank Samonski, and myself, we went to Washington, because I had done all of the analysis of the problems and everything, so Frank and I went. We were sitting on the side of the room, and in the room was all the big wheels, the Marshall [Space Flight] Center [Huntsville, Alabama] Director [Wernher von Braun] and JSC and Faget was there and the program manager. Oh, everybody was there. It was a big deal.

Joe was explaining to them that we had to do this, and we obviously weren't very impressive to these senior people. I couldn't have been more than five or six years out of school, something not very much.

They said, "Are you sure? What are you basing this on?"

He said, "I've got the best there is. They say it won't work."

They grumbled around a little bit, and so finally they said, "All right, go ahead and do it."

Faget called me over, and Frank, too, and he said, "Okay. You've got your decision. Now you go to the West Coast and stay there until there's a plan in place and this thing is under way."

We did. We got on an airplane and left and went to the West Coast. They redesigned the boiler, and you could turn it upside down, it would work. It never gave any trouble. Nobody believed us, though, really. They said, "Well, we can't take the chance, but they probably are wrong." It turned out that the schedule was such that they had to fly that boiler that wouldn't work one time. It was on an unmanned flight. And lo and behold, it froze up the steam duct. [Laughs] But it was an unmanned flight; it didn't hurt anything. But anyway, it made us feel good that what we had said was true was true.

But there were several occasions that things had to be taken into the hands of the project manager and you had to do what you had to do. The same thing happened on the radiator. When the Command Module and Service Module first started, as I said, they were going to land. When they changed that, then that sort of gave two configurations. There was the early configuration which just had the Earth orbit check-outs and flights and then the flight that went to the Moon. The radiator system—which again was my old home—the radiator system had a design that wouldn't work. The reason it wasn't obvious to everybody that it wouldn't work is that the analytical tools hadn't been developed yet, so everybody was doing steady state analysis and guessing.

It turned out one of the very first things I did when I got here in Houston was I put out a contract to develop a new analytical tool to evaluate fluid flow in radiators, and there was no other tool; it was the only tool in the world. We used it to do our analysis, and our analysis said this thing is not going to work. The Apollo vehicle had a single fluid. The fluid that went up to cool the crew went down to the radiators, [the] same fluid. In the Shuttle that's not true. They have water in the cabin to cool the crew, and they have Freon outside.

But the concern was that at the time, simplicity, of course, was a big parameter, because weight was a big parameter. Everything you took with you to the Moon, you needed launch vehicle capacity and you needed Service Module braking capacity. You'd need to brake it into the lunar orbit. You needed to push it back out to get back to Earth, brake it into Earth. Weight was really, really critical. A simple system was a good deal, that was a good idea.

You couldn't put water. Water freezes; it doesn't even work in water pipes; it's not going to work in a radiator. It's too cold. They used an equivalent of an antifreeze solution; used water glycol, which was the antifreeze. In those days it was believed that water glycol and water, if it leaked, it didn't hurt anybody. I guess they determined later that there were some adverse effects; we don't do that anymore. Maybe there were some adverse effects, but it wouldn't kill you. That was the way we flew, anyway.

It turned out that that antifreeze would let it slush up; wouldn't let it freeze, but it would slush up. Well, when it got to be slushy, it didn't flow the same as it did when it was pure liquid. It's like when you pour antifreeze out of a container, it's just like water. When it gets really, really cold, though, it's like a slush, like you go to Kmart or whatever and buy you a slush. The problem was in this radiator design some of the panel would get slushy, and some of the panel wouldn't, and all the places that got slushy would stop flowing. The liquid wouldn't flow anymore, and you'd lose that part of the radiator. Our analysis showed us that.

[North American] refused. They said, "That is wrong." They hired this professor from someplace, and he said we were all wrong. We were still probably only a year older than we were the last time, but we said it wouldn't work.

The program manager, who was Joe Shea still, he took us out to [North American] and had a meeting with the big leadership. It was Dale [D.] Myers in those days. And said, "Your radiator won't work. You've got to change it."

They said, "Well, the only change we know is to go to two fluids, and we'll put water inside and some other fluid outside, and redesign the whole thing, and we need a lot of schedule and a lot of money and a lot of everything."

Shea challenged us. He said, "I need another solution. You design the radiator."

We designed a radiator, and we designed a radiator that took advantage of the fact that it slushed up. It would slush up when we wanted it to and not when we didn't want to. It's all very complicated, but the bottom line was that we were able to use a single fluid, and we were able to design a radiator that would, in fact, work. We were able to not only design it, but get them built and tested right out here in the thermal vacuum chambers in this building, and they worked. We had test data. We had analytical data. We knew they would work.

So went back out to [North American]. Shea says, "This is the design. This is what I want."

The management out there said, "Well, the group that's responsible for the life support system, they said they're dug in." He said, "If I give them this job, they're going to find a way that it's not going to work. The only thing I can do is I can take the radiator away from them and give it to my Project Engineering Group. What about that?"

Shea said, "Whatever you have to do."

He did; took the radiator away from those people and gave it to another group. They took our design and—the contractor we'd been using was LTV or Chance Vought; I don't know what they are, they've changed the names, Ling-Temco-Vought, Chance Vought—and they used the same subcontractor. They knew how to build it. They built it. It worked like a charm.

Again, when they flew that radiator in Earth orbit, because we had [Apollo] 7, 8, and 9 were all Earth orbit missions—the radiator, they had to baby it along, because it would freeze up. They were in able to Earth orbit to rotate it and keep the Earth helping out the thermal situation, but it would have never made it to the Moon.

But that's really just some hero stories about the relationship that we had with the project manager. He believed us. Looking back, I don't know whether I would have or not, but he did. I think it was a time when things were new enough that there wasn't an established peer group that have done this kind of work two or three times before, and you know, you can sort of spread out and get a lot of opinions. You pretty much had to go with the insiders, the people that were there and did the work. As long as they were competent and did the work well, I think decisions were made.

You've talked to a lot of people, but I think there were propulsion decisions made. I know the Lunar Module engine was changed out. They picked a prime, and they took it away from them and went with a second design. Maybe it was a Rocketdyne design; I don't remember what it was. But I do know that they changed subcontractors after the program was already let, because of a NASA activity to design a different descent propulsion engine. It was an opportunity to work in a relationship where you had an important role, and you were able to get respect, job satisfaction, for doing that role. I think that was really the key.

One more thing I can remember; it was during the Shuttle Program. Let me back up a little bit before I tell you that. When the Shuttle started, we were a lot smarter. We knew both the environmental control systems and the thermal control systems.

The Shuttle has a very elaborate thermal control system. It has multiple loops. It has eight radiators, four of which are mounted and four of which are deployable. It has a new water boiler that's not a water boiler; it's called a flash evaporator. It's a brand-new design, because we didn't like that other design. Brand-new design; it has two versions of it. There's a high-load version; it's called a topper. It's a very sophisticated thermal control system. It has two loops; Freon outside, water inside.

But we were smart enough to know that that's what it was going to take to have a Shuttle thermal control system, and the program was smart enough to listen to us to let us finish out the design and do testing. We tested it out in the chambers here also. Before the contractor got committed, we already had test data on a design that worked, the flash evaporator. We developed it and knew it would work.

Turned out the contractor picked the wrong sub [subcontractor]. We knew the concept would work, because we had already developed it and tested it, but the sub they picked didn't have any experience with it, and they suffered mightily with struggling to try to get that thing to work. And it's still probably the weak link in the thermal control system. It has a very complicated control system that's viable, but it's not really a straightforward system, and it's not the way you'd do it again today. But it works.

But it still freezes up occasionally. We've figured out a way to thaw it, though; we didn't crater the mission just because we froze up the flash evaporator. That was another aspect of the testing. I think Jim [James R.] Jaax was responsible for the testing of that thermal control system; he understood that.

Probably the biggest test we ever had, which leads to the point I was getting to, is that we went to Aaron Cohen and said, "Aaron, you don't have any integrated thermal test here, and you've got a very sophisticated thermal control system. You need to put it all together and test it all together."

Of course, Rockwell was able to prove that they didn't have it in their proposal. That was true. But it was needed; they sort of compromised, and you'd have to ask him, but I think he probably paid for it. I know he paid for it, but I don't know whether it was considered part of the original contract or whether it was an add-on, but I imagine it was an add-on. The only difference that makes is it's cost plus; the government pays for it. But whether they got fee on it, I don't know.

But Rockwell did agree that an integrated test was a good idea. They wouldn't go as far as say it was needed, because if they said that, then they had to do it without a change order. But they did say it was a good test, desirable test. We put it together. We had a full radiator system, flash evaporators, all of the heat sources, because that system cools the power system. It cools the hydraulic system. It cools the APUs [Auxiliary Power Units], payloads, the cabin; cools everything. It goes everywhere and cools everything. We tested it right out in Chamber A, the large chamber out here. That was another time in which the program listened to us.

When we started the era of "there's going to be a Shuttle,"—and we knew that pretty early, because it was Faget's baby. He wanted a Shuttle, so we figured out there was going to be a Shuttle. Every subsystem sort of started worrying about what they're going to look like. We did a lot of breadboarding and a lot of component testing and subsystem testing.

I was able to get an excess chamber from Langley. Langley used to be in the life support business, but they got out pretty early on. I don't know the dates, but I'd say it was pre-Shuttle. They were out of business by Shuttle time. They had a chamber that was about the right size for the cabin. It was square, but it was about the right size, so I got that off excess and set it up over in our laboratory building.

I was still in—it was called Crew and Thermal Systems then. It went from Life Systems—we got rid of the doctors, and then it became Crew Systems, and then the thermal became much more important in terms of—not important, but a bigger part of our activity. Then it was Crew and Thermal Systems Division.

Our laboratory facility—I set it up there. We did a lot of life support testing. As I've already mentioned, we did the radiator testing here. We knew we wanted a Freon radiator system. We knew we didn't want the water-glycol, and Freon turned out to be the best fluid. We knew, based on our trade studies, that that was the right answer, but we had no experience with it. Nobody had any experience with in the radiator. Space radiators were a new thing.

I didn't mention Gemini radiator. Gemini radiator, it was a brute-force radiator. Gemini was a brute-force program. They had no schedule, and they had some things they needed to do. There was no finesse in Gemini. Gemini was a brute-force design. What they decided was if they had only one tube, that as long as they could push the fluid through it, they had it. None of this parallel tube stuff. And they did. They picked a silicon fluid, and it took a lot of power, but Gemini was only an up-and-down vehicle. Didn't have to worry about the weight constraints of going to the Moon; they had enough power to pump it. They pumped it. It worked fine. But it was not a very high end radiator system.

But when we came to Shuttle, Shuttle in those days had a thirty-day mission; part of the statement of work, thirty-day mission. You had to be very conservative on your power usage, because if you were going to stay up there thirty days, that's a lot of power. We wanted an easily pumped fluid, and we wanted a fluid with very, very low viscosity so that it wouldn't freeze up, wouldn't change density.

It's the difference between syrup and water, right? What you want is a fluid that stays like water all the time so it's easily pushed along. If it turns into syrup, then it takes a lot of pumping power to pump it, and if it's in parallel systems, where you have parallel flow paths, and some of the paths are syrup and some of them are water, the syrup will die. It won't pump through. It will just stagnate.

We knew that we needed a parallel-flow system that would work, because the Shuttle—there are no Shuttle pictures here. If you remember the Shuttle pictures, when it opens the payload bay doors, the entire surface of the inside of the payload bay doors are radiators. There are lots of tubes; they've all got to stay alive. We needed a design that would work and testing that would work.

In those days we needed more heat rejection. That was not enough area. Heat rejection and area go together. The bigger the radiator, the more heat it will get rid of. There was not enough area there. The front four panels, we designed so that they could lift off the doors, and you could see the bottom half of them. You could get bottom and top radiation on the front four panels, and only top radiation on the rear four panels. We'd never had a two-sided radiator before, either. We did all the testing here.

As I was mentioning, though, in the laboratory in Crew and Thermal Systems, we did the initial life support testing, and we also did the beginnings of an operational test vehicle. As the prime and the subs [subcontractors] began to build the Orbiter and certify the systems, they would end up with test hardware left over, and we could get that test hardware. If the program manager agreed, we could get the test hardware. We always got in line and got the test hardware.

Rockwell had a large spherical chamber. Their intention was to outfit that as if it were a crew module. Now, this was a cylinder; you obviously know the Orbiter crew module is not a cylinder. They foamed off the parts of the cylinder they didn't need, and what they had left was the right shape. Then they built the interior structure in there, and they were going to put systems in it. They were going to run all their wiring and their coolant loops and everything.

It turned out—for a reason I can't tell you—they decided, after they built the chamber, after they'd built the internal structure and foamed it, they decided they didn't want it, and they were going to junk it. I went to Cohen and told him that I needed it, and I would set up a operational test capability. He gave it to me, and I set it up in a laboratory. Then I got the test hardware after the prime was through testing it. I brought it in, and I integrated it in. Basically I had a full-up cabin with all the environmental control system.

Then when the airlock came along—the government is the only place you can do man-testing. None of the contractors have man-test capability. It got too expensive years ago, so they gave it up. When the airlock came along, I told Cohen, "We do the man-rating in the airlock here."

He said okay, and we connected it to this can that we had. We have this pressure chamber outfitted correctly for a cabin, and of course, the airlock's in the back of the cabin, that's the way the Shuttle is. We integrated the airlock in, and we could test it at vacuum. We hooked it into the pumping systems that were in the building already. We did those tests for the program.

We also got the mechanical systems, like pumps and the fans and water separators were also a rotating device, all the rotating equipment. We got that equipment in and put it on tests so we could see how long it would last. The contractor had to test it a certain period of time to prove it was okay, and then we continued the test until it failed to see how long it would last, because [the] Cape [Canaveral, Florida] needed maintenance data. They needed to know how many spares to have and when they should worry about replacing the spares. We did that testing.

Also, operationally, as the vehicle was started being put into use, there were a lot of issues that developed associated with the EVA [Extravehicular Activity]. The original plan for going EVA, since the Orbiter cabin was just like this; open the door, it's okay. But when you go EVA, you have to prebreathe. You have to get rid of all this nitrogen. The doctors were getting more conservative over time, and the prebreathes were getting longer, and the crew was getting very tired of just sitting around with a mask on their face, not being able to do anything.

They came up with a protocol that said if you can drop the pressure in the Orbiter and let the crew basically spend the night at the lower pressure, then their prebreathe can be a lot shorter. The system wasn't designed for that, obviously. It was designed for atmospheric. We did all the testing and the procedures to allow the changes to be made. Again, we used the test article that we'd put together for the program.

I'm not sure whether Cohen was still there or not. As you know, the Shuttle program managers changed over time. [Glynn S.] Lunney, it may have been Lunney at the time. I don't even remember who the program manager was. But we did a lot of testing, operational-type testing.

Also some of the final certification, the vendors didn't have good test facilities. The regulators and relief valves, they were built at a company called Carlton [phonetic], I think; I think it was Carlton. But they had little chambers, little test chambers for hardware. That chamber would be half as big as this table. A relief valve or a regulator really, if you try to operate it in a small volume, it doesn't act the same as it does in a large volume. We had the only real large volume that was exactly the right size, and we could vary the pressure in it. We did a lot of certification tests; verification, I guess they called them. The hardware was essentially certified, but it just hadn't been put in the right operational environment, so we did those for them.

We had a lot of operational activities associated with looking at some expendable problems. The lithium hydroxide, which is the way CO_2 is collected, is a chemical. You basically use it up and you throw it away. The program became concerned over logistics. They asked us could we look at a subsystem that could be basically mechanically used over and over again, and not take up lithium hydroxide that basically took a lot of space and then you threw it away when you finished using it. Again we used the facilities we had to develop a system like that.

It turned out that the longer missions they were worried about, they only flew two or three of those. They flew maybe three. There was like a twelve- or fourteen-day one and a fifteen- or sixteen-day one. That's as long as they ever went. They did use this system during those missions, but when it went back to the shorter missions, they went back to the lithium hydroxide. I guess within the fleet there's still the ability to do that, but probably they'll never do that again. Over time they gave up the thirty-day mission and decided not to do that anymore.

WRIGHT: Since you've been talking for a few minutes about testing, I was going to ask you to maybe share with us the evolution of your testing facilities that you used. You mentioned earlier, too, that you had had a contract for analytical tools. So if you could share with us how you knew what you needed to develop, and how that development came along, and that you would have the right tools and the right facilities to be able to test these components.

GUY: The original facility had only one new chamber. The original facility had a leftover chamber—I think it was a Navy chamber—it was the eight-foot chamber that we did a lot of the early testing in. Mercury-Gemini kind of testing was done in it, and actually, it was used for the Apollo-Soyuz [Test Project], the tunnel. We were responsible for the adapter tunnel. The Apollo was a five psi [pounds per square inch] pure oxygen. The Soyuz was an atmospheric fourteen-seven mixed atmosphere. We had to bridge the gap, so we used that same chamber there.

The twenty-foot chamber was the new chamber. I say new. I really wasn't personally involved in that, but I believe that was new. You might get somebody to say that's not true, that we might have gotten that from someplace else, too. I wouldn't challenge them if they know that, but I think that was a new chamber. Everything else, we basically got from someplace else. The chamber that we used for the spacesuit work was a leftover Grumman chamber. Grumman had a chamber they built up for the Lunar Module. We got that chamber down, and we used it for all our EVA work. Of course, it had a Lunar Module aspect to it, so we did that.

Inside the twenty-foot chamber I got a spacecraft, a structural spacecraft, from Rockwell, and I put it inside the twenty-foot chamber with all the systems. In the Apollo period we had an Apollo environmental control system, and we had the LM, Lunar Module, life support system, both in chambers in our building so we could do testing on those. In fact, when the [Apollo 13] accident occurred, those were very, very critical, because what we had to do was we had to use the Lunar Module, which was [in] our ten-foot chamber, we had to use that as the test bed for the modification that we made.

The real problem—from our standpoint—the real problem was we had to collect CO_2. If we hadn't collected CO_2, they would have died. We had to collect CO_2. The Lunar Module was only a two-day vehicle; this thing happened on the way out, so we had to go all the way out, loop around, and come all the way home. Two days' worth of lithium hydroxide collection wasn't going to make it. We had plenty of lithium hydroxide in the Command Module, but the Command Module had no power. We had shut down, because the only power left was a very short life battery. It was dead, but we had the lithium hydroxide.

We had to adapt the lithium hydroxide from the Command Module and from the spacesuits. We had to adapt it so that it could be used in the Lunar Module, and, of course, the adapter had to be made out of whatever they had on board. We did. We developed a way to do that and tested it out there, and then communicated up to the crew how to make their own, and they did, and they got back. Now, that's not to say that was their only problem. They had lots of problems. But from our standpoint, that's what we had to do. It was not an oxygen issue; it was really a CO_2 issue. They had to get rid of that CO_2.

I just mentioned how we got the Shuttle chamber. I also told you how we got the original Shuttle chamber from Langley. When Station came along, we took the same principle that we had for Shuttle, which was the airlock man-rating, and Station has an airlock, too. We convinced Station that we could do their man-rating in conjunction with our twenty-foot chamber. We did the Shuttle man-rating with the eight-foot chamber, but both had pumping systems; it didn't make any difference. If you do the man-rating, what that does is it gives you a capability then not only to do troubleshooting if you have problems, but a place that the crew can get firsthand experience.

Now, the crew has lots of trainers sprinkled everywhere, but they can't go to vacuum. They're stuck in a building someplace. An airlock, obviously, at one end of its use, it's just like the environment of the cabin. At the other end it's pure vacuum. If you want that kind of training, you need a different kind of a place than a mockup in some building someplace. They're still doing training today in those two facilities. All the crew, before they go EVA, go through that test capability.

The only other change is the chamber I got from Langley. After we finished the early Shuttle testing and went on to the Rockwell chamber that I got, I moved that Langley test chamber out to the adjacent building, and we set up regenerative life support testing in it. We grew lettuce and, I don't know what all they grew. I set that up before I left, and they continued it. Somebody else could tell you what they continue to grow in it. But we grew lettuce, the idea being that if you're going on a trip to Mars, it takes 500 days; that you wouldn't want to eat out of toothpaste tubes for the whole time; it might be nice to have a salad. Also if you were on a planetary habitat of some kind, you'd certainly want the ability to grow some kind of vegetables. We were working on that. And it becomes part of the life support system, because plants absorb CO_2 and give off oxygen, so you need to take advantage of that.

WRIGHT: How has the process of testing and analysis changed from the days that you first started, just through the Apollo days up to the Shuttle time?

GUY: I think only in magnitude. When we started out, you remember I told you we designed the radiator for the Apollo vehicle, the Command Module that went to the Moon. The computers were in a state at that point that to make those computer runs—because it was an orbital transient; it took ninety minutes to make an orbit, and the orbit processes, and the environment changes. The heat loads change as the vehicle flies. Then you have to go to the Moon, and that's three days. Then you go around the Moon, and that's a couple of days there. To do the analysis, the computers weren't very fast, and the analytical tools were not as sophisticated as they are today. But even if they were as sophisticated, it would take a long time.

Basically we'd rent out the computer. Building 12 was full of computers. We called them mainframes in those days. It was full—the floor, the entire center of the building, was full of mainframes, and there were offices around the outside. They basically had a system where you would give them a computer run, and then they'd run it for you, and they'd give it back to you in a stack of paper or whatever. We would get weekends. They would give us the entire facility for a weekend, and we'd just camp out and run analysis, because it took so long.

The problem in those days was that you couldn't tell until you got the answer whether you really had a good run or not. You might run three, four hours, and then get the answer and find out there was a mistake someplace. You'd trash that, fix the mistake, and start over again. It was not really optimum. As time went along, the computers got faster, the analytical techniques got better, and you could begin to run batch runs in two or three or four hours, or overnight. And then keep going, and as computers get faster, analytical tools get better. In fact, we used to have—you remember the term Cray?

WRIGHT: Yes.

GUY: Well, of course, there's still some supercomputers around, but that's almost unnecessary now. All the stuff that you used to have to run on something like a Cray, you don't have to anymore. You can run on PCs [Personal Computers] if you want to.

I think it's only magnitude. There's just more and more and more of it. Early we were limited by the tools, the development of the tools, and the time it took to run the program. You ran pretty small sets of runs, just because you couldn't afford the time to run the more sophisticated runs and longer runs. I think that because of that, the more part, I think there's a lot more trust, and there's a lot more dependence on analysis these days.

I think you'll find, though, that there will always be a balance; that no matter what analysis you do, there are always unknowns that you have to make judgments as to how are you going to do this particular analysis, what are the boundary conditions, and whatever else. I think that you need some test anchoring to have full confidence, and I think there always will be. But the flip side is true, too. Every test has a bunch of compromises, too, and the analysis is a good way to extend test data to conditions that you can't really test, because there's some limitations of the facility or whatever else. I think it's recognized now as a mandatory mix; that if you ever do it all one way or the other, you won't be completely confident.

WRIGHT: You mentioned earlier a couple of times about the relationship between your group and your project manager. It sounded that you had full access to be able to talk to them very frankly and very bluntly.

GUY: Oh yes.

WRIGHT: Can you tell us about those situations and how you were able to reach them and to have them, because there were so many people working on the program at that time.

GUY: I wasn't sensitive to that. At the time I never felt that I couldn't get an audience, couldn't get with the program manager. It just never was an issue. I guess it was due to the personalities, maybe. I mentioned Joe Shea several times, because he was there in the early era, but after the fire he was gone. Of course, at the fire we hadn't even flown Earth orbit then.

George [M.] Low came on. Of course, he had to rebuild relationships with the contractor, and then we had to rebuild our relationship with him. But he was a really smart man; He was a smart man. He did that, and he took over. We worked with him. As I said, we were his ability to know that his contractor was doing the right thing. So fencing us off and being busy and not being able to talk to us, those things never really happened. Now, we had a lot of independence. We didn't do "Mother, may I?" But if there were issues, I cannot recall feeling that you couldn't get to the program manager.

I was never a subsystem manager. But some program managers, and very likely Shea and Low both, had weekly meetings with their subsystem managers. I'm positive that Cohen did. By then I was in the management system, and I was sending my project managers to the meeting. But early on the people that I had were the analysis, the test managers I had those kind of people.

But then after I got to be in charge of the division—it was in 1980; Shuttle flew in '81, I guess—so I sent my subsystem managers to the weekly meetings. They got access, continual access. Any of the subsystem managers can tell you about the other managers, but I think they had weekly meetings with them, too. I think they knew what was going on. That was how the managers managed was with the insight that they got from their subsystem managers.

WRIGHT: You had just mentioned about Joe Shea leaving after the fire, after Apollo 1. How did the accident with the Apollo 1 crew affect what you were doing?

GUY: Before the vehicle fire, there was a fire with the environmental control system. It happened out at Torrance [California], I think. I was assigned to be on that investigation group. It was my job to figure out where the fire started, based on the data. Trace it through the system. That's what I did. I had already been introduced to fire and the conflagration paths and that kind of thing.

When the fire happened, I got a call from a fellow in the Program Office, Jerry [W.] Craig, who said that Shea had called, and wanted he and I to go learn all we could about fire. We figured out who knew about fire, and we went to visit them. Then we synthesized what we learned and took it to KSC [Kennedy Space Center, Florida] and spent about a week down there with Shea, because he wanted to understand what he should do to react, whether he was in a situation where he could control the design and make it safe, or whether he had to just start all over again on the design. We went down and, as I said since I had done some of that work before on the life support system, I also looked at some of the data on the fire and all.

My division was responsible for clothes and, of course, the life support system and some of the crew equipment, shavers and toothbrushes and tape players and stuff like that. All of that, of course, had to be fireproofed, and the clothes; they had to develop a new fabric that wouldn't burn. The division was affected, but I was not in materials development or in any of that kind of stuff. My only contact was with the diagnostic of what might have happened and advising a little bit on the physics of fire.

I do remember, though, that we came back—actually, I flew back on the airplane with Shea when he came back to debrief the Center on the—that must have been fairly early. It must have been maybe after about two weeks, maybe, or somewhere in there; I don't remember exactly when that was. I think we may have gone down there more than once. I don't remember how many times we went down, to tell you the truth. But anyway, we went to [North American], because they were concerned about design changes.

I think the conclusion finally was that it was probably a wiring problem of some kind. Something shorted. Of course, I don't have any knowledge as to exactly whether they thought it was a design—some failure that occurred, or whether the crew or the technicians or something had damaged a wire or panel or whatever. I don't have any insight into that at all.

But [North American] was interested, and I do remember we went out with a group, and John [W.] Young was in that group. John was, of course, representing the crew, but he was pretty much a—maybe skeptic is too strong a word, but he had not jumped on this, you know, "This is the world's worst design, and let's junk it and start over." He really believed that we could get there from where we were.

Of course, we did. There were a bunch of changes made, but they were done very quickly, and we were back in flow again. Again, I don't remember the time frame, but we obviously got to the Moon before the end of the decade. It didn't take very long to do the redesigns and go.

The hatch, I think, was the biggest redesign issue. The hatch didn't open. The pressure sealed it. That wasn't any good. I think that probably was the biggest thing. All the flammability changes that were made were pretty much add-on. They were able to pot the electronics so they wouldn't burn, and wrap nonflammable materials. They were able to do a lot of things like that that let them use a lot of what they had. I don't think there was any wholesale redesign of the systems. A lot of protection done. But the hatch, I'm sure, was designed over.

WRIGHT: Earlier you stated that after the unmanned flights, there was a change in the radiator.

GUY: No, it was the water boiler.

WRIGHT: The water boiler.

GUY: Yes. Now, there was only—I'm not sure. There may not have been but one. Couldn't have been more than a couple of unmanned flights. But, yes, they did. There was a design change.

WRIGHT: Then as you monitored the manned flights after that, did you find more and more, [or] you were pretty set and ready to go for the lunar landing?

GUY: No. The lunar vehicle, I don't remember any changes that we made in our area. In fact, we didn't make any changes in the Earth orbit vehicles, either, that I can recall. But they did have to limp along, because the radiator wasn't very good. They did have the better flash evaporator by then.

But the systems, they were tested well and designed well, and they worked well. That was our job. Our job was if the contractor does it right, then that's good, and if the contractor doesn't do it right, then it's our responsibility to tell somebody and get it fixed. And that's what we did. If it ever didn't work, they came to us and said, "Why didn't it work?"

We couldn't say, "It's not our fault. They did it." That didn't work. It was our design. We had to own the design. That's a phrase I use these days. I never used that phrase then, but we owned the design. It didn't matter who invented it, but by the time it was certified on the vehicle, we owned it. We're the ones that said, "It will work. It's been tested adequately, designed adequately. It will work." Therefore, on those things that we didn't believe that, then we had an obligation to get to the project manager early and say, "You've got a problem here. You have to do something."

WRIGHT: You talked about your area doing integration. How was it set up that you did integration with the rest of the areas, so that everything met the schedule to get to the Moon by the end of the decade?

GUY: I didn't have anything to do with that. My integration work was all within the systems that we had. The only exception to that would be in the thermal control system, because everybody's thermal control system services electronic boxes. It services cold plates. It services potential power supplies. There's that integration that you have to have. But the spacecraft integration really was [North American's] issue. They had to make everything fit, and as long as it all fit together and each subsystem said, "Mine's okay; I still work," that was really how the vehicle integration was done.

We did have a Spacecraft Design Division after we got here. I mentioned it was a branch at Langley. It became a division. It took a lot of different forms, though. It sometimes would have aero [aerospace] and GN&C kinds of stuff, and sometimes it wouldn't. Sometimes it had a lot of mechanical systems, and sometimes those were in our Structures Division, and it didn't have them. It had a lot of different forms. I guess the only piece that was consistent was the piece that looked at the next spacecraft.

Like when the Shuttle came along—it wasn't called Skunk Works in those days, but whatever it was called, out at Building 36, we were out there for months and months doing detailed design work on Faget's design for a Shuttle. We did a similar activity pre-Station, although politics were beginning to filter into the process, and that really wasn't done maybe quite as well as the Shuttle was at the vehicle level. But at the subsystem level we were still getting good support. I had a full regenerative life support system in the twenty-foot chamber, test-worthy for Space Station. We had already designed a radiator system and had flown on a Shuttle flight a sample radiator, one like we wanted to fly on the Station. We had a lot of the subsystem homework done.

At the vehicle level there's not as much work done, because, as I said, the politics had separated the Station into lots of little pieces and sprinkled them all over the country. We called them work packages; everybody had a little piece. The theory was if you do your piece right, and you hook it together, it will all work. The big-picture systems engineering, I think, was diluted some for Station.

But Shuttle, I think, had good systems engineering. In fact, we insisted. We got to write the statement of work in those days. In fact, we did for Station too, except they changed it after they issued the contract. But anyway, for Shuttle we wrote the statement of work and said how we wanted things to be, because we'd done a lot of homework here. Therefore when the proposals came in, they were pretty much the Shuttle, pretty much what we wanted.

As I said, there were some issues where the contractor would agree with the concept and then pick the wrong vendor, so that who they picked really didn't have the experience base to go implement it. But that's the competitive procurement; it's that way. You get what you get. You pick a prime, and you get the subs that come with whoever you picked. Some are good and some are bad; maybe not bad, but less experienced.

WRIGHT: Not as good.

GUY: Yes. That happens a lot in any new spacecraft. Lockheed, of course, just got selected for the CEV [Crew Exploration Vehicle]. I haven't heard enough talk to know, but I would imagine some of their subcontractors are exactly the ones that we would have thought were the most competent and some probably weren't. I don't really know the details of the CEV.

Throughout all of this the spacesuit was coming along. When we started—I've mentioned several times, but I didn't really explain it very well. This group and section and branch—it was eventually a branch—that I was responsible for never had a single system priority. I worked just as much on the Lunar Module as I did on the Command Module, both the thermal and the life support.

We did testing—I mentioned the water boiler and the flash evaporator. The Lunar Module had a water evaporative system; it was called a sublimator. Different again; it's ice. You create a layer of ice, and you sublimate the ice. That's how you get rid of your heat, and that same concept we used on the spacesuit.

But anyway, I supported Lunar Modules. I supported Space Stations. I supported Gemini, Mercury. Whatever was a program, I supported. I got to see all the programs, and my area was basically analysis, integration, and test management. So that area for all the programs, we did.

For example, the spacesuit, we bought the Apollo suit from ILC [International Latex Corporation]. We bought the backpack from Hamilton Standard, and the integration was done in my organization. We had the specifications. We had the ICDs [Interface Control Documentation]. We did all the integrated testing. We did the certification program. We did it first in the eight-foot chamber, and then out here in Chamber B, we did the final thermal vacuum certification. Skylab, the same way. Shuttle, same way.

That activity was a good location to be able to interface with all the programs and be able to cross-fertilize what you learned in the next program so we didn't make the same mistakes. We knew what to be sensitive to, what not to be sensitive to, what to worry about.

Of course, we always were dealing with the prime. You dealt with the prime based on who the prime was. Some were different. Grumman was a lot more thorough as a nature. Of course, I'm sure it varied from system to system and person to person, but in general Grumman was a very conservative engineering company, and they were very thorough. They also had an advantage, I think, because their schedule—they were selected after the initial selection, and I think everybody realized they were late and they needed maybe the benefit of a good bit of money and a good bit of freedom in their engineering. But they were a good company. I think the Lunar Module was a well-designed vehicle.

As we worked through the different primes—of course, MacDac [McDonnell-Douglas Corporation] came along. It was McDonnell, I guess it was, in Gemini, and then McDonnell Douglas out on the West Coast for the work package, our work package. You worked differently with whoever the contractor was and whatever their attitude was. But I think it was always a synergistic relationship, where we felt like we were part of the team and they felt like they were part of the team. They had the job of doing, and we had the job of making sure that we thought that the product was going to succeed.

Anyway, back to the suits, the original suit, since I was in this sort of analytical section, the original proposal evaluation for the Apollo suits—and the backpacks; they were separate in those days; we did finally create a bond there between the two companies, but originally it was two contracts. But the original proposal evaluation where everybody was saying what they were going to do and how they were going to do it, that was basically evaluated inside the division I was in. Of course, these days you couldn't do that. It would be a source board, probably Headquarters-run, that kind of thing.

But in those days that's really not the way it was, and even through the Shuttle EMU—that's what we called it, the Extravehicular Mobility Unit. Even through the Shuttle EMU, it was a source board run within the organization. I was the source board chairman of the Shuttle EMU. We prepared the statement of work—we, the division. I was responsible for the systems engineering integration, and, of course, we had a branch that was responsible for the backpack. Another branch was responsible for the suit.

The three of us put together a statement of work and issued it, and eventually, of course, we got the proposals and all, and I was the chairman of the source board. [John F.] Yardley was the selecting official. I had to go to Yardley and say, "This is what we decided."

We always believed that we had the experts. We had the suit experts. We had the backpack experts. We had the analytical experts, the test experts, and we did all man-testing here. We never did any man-testing anywhere else. I shouldn't be quite so bold. Probably early Apollo there might have been some man-testing in one of the contractors, but very soon that was viewed as not a good deal, because an accident where crews would have an accident in a vacuum chamber was viewed very early as being a problem that we never wanted to face. The government took over man-testing.

WRIGHT: Did you ever find yourself being a test person, or you were always on the analysis side?

GUY: No, no. I owned all the test managers. I've been test manager lots of times. I was test manager for the Apollo EMU certification program. I did the test profiles. My test team managed the test.

WRIGHT: We're going to stop for just a second and change this tape out.

[Tape change.]

WRIGHT: Before we move on through the Apollo time, I want to talk to you about a couple of things. One was your move to Houston. That was a change not only in the NASA environment, but for you personally. Can you share with us what that was like to move, and was there any delay in services in what you were trying to do in your schedule by trying to move facilities and being part of a new facility as well?

GUY: I was, of course, only out of school two years, three years, not very long, so the move was just something interesting. My home wasn't Virginia, anyway. This was a longer drive back to my home, so there was some thought about that, but in those days I drove it straight through when I went, anyway. It was only one day; it just took all day.

But I don't think the move was anything at all. I had just gotten married, and it didn't seem to be any issue for my wife, either. I don't think either of the parents thought it was a good deal, but we had already picked our fate. We wanted to work in the manned spacecraft program, so we did.

As far as the facilities, actually, that was rather well done, from my perspective. I didn't have anything at all to do with it, of course, but we were in what was called the Lane Wells Building. I don't know what it's called today. It's still there, I think; I don't know if it's still there or not. Anyway, but it was a building—probably out of the oil patch thing—that had a nice front-end office building and then a rear high bay area for testing, and it was on a fairly nice piece of property.

I think we brought in trailers and stuff; I don't remember exactly what we did. But we had really a pretty good area to work in. I thought we had probably the best building with respect to accessories, parking and all that kind of stuff. We had our own private parking. It was not a classical office building, like the glass walls. It wasn't that kind. It was a nice place.

As I said, we had the ability to test almost instantly. I was able to get a computer. As I said earlier, the computers were not too swift, and they were pretty large. They called it 1401 in those days—an IBM 1401, that we put in a small building outside the main area, and that was our computer. Then when we moved on site, I got a much larger computer. I had a satellite computer station. Moved over in Building 4, and I had a satellite computer station.

I mentioned the bank of mainframes. They would have satellite stations around that you could run some of the smaller stuff on. It was still a whole big room full of a computer, but it wasn't that powerful. Up until the mainframes died, well, I always had a mainframe as part of my core capability.

I said analysis, and analysis is a sort of a multi-use word. A lot of the analysis was not number analysis, but was trade studies, was evaluating different schematics for functionality, and looking at mass balances and power budgets; the whole spectrum of analysis, not just doing equations and running numbers, although we did that, too. But we were very early in the modeling world. Actually, the only thing that preceded us physically was NASTRAN [NASA Structural Analysis System]. NASTRAN was invented not long before we started, and it was basically a structural analysis tool.

Our problem was the structure stays put, and our fluid systems move, so we needed a different way to look at analysis. Where structural analysis, even the thermal part of structural analysis, is basically a static problem, is that if you whatever, heat up this part of the table and it was made out of metal, then you could watch and if you keep putting heat here, it will eventually get warmer and warmer and warmer. At first when you put heat there, you'd put your finger, and you'd feel nothing. But if you wait a while, then you'll begin to feel, and you move your finger out. It's sort of a confined transient problem.

But if you've got fluid going back and forth all this time, and you keep changing the environment, which is what prevents heat from being rejected, then it's a multifaceted dynamics problem. Our tools had to be completely different, and I think that there are a lot of systems that require fluid system analysis, and that was really in its infancy at the early days. We were sort of leading-edge. And, of course, the aerodynamics had to be worked. We didn't do that, but the aerodynamics people had to develop their tools. Of course, the structural people already had started theirs. We created environment tools.

There was an environment tool called TRASYS [Thermal Radiation Analysis System]—it was an acronym. I don't remember what that stood for. I don't know what it stood for. But it was a way to look at, if you're in orbit or going to the Moon or whatever, what is the environment that you're flying through, so that you could design thermal systems to operate within that environment. The tools were really coming on in those days, and I think that that's—the real early difference is the recognition that that was the right thing to be doing, that the seat-of-your-pants engineering was not really the right thing.

It's really hard to express that. I feel like discussing corncob pipes or something. The first programming program I took was in machine language. We did things like how do you take the sine of something. Well, obviously, FORTRAN, all you did was say, "Sine of the angle," and it did it all for you and you didn't do anything. But FORTRAN was still in the future when we started. It wasn't long; FORTRAN came along very early. But we didn't start using it, but some of the very early programs, which were machine language programs, and then FORTRAN came along. FORTRAN was really an enabler. It was an engineering way to program. It was really a miracle.

But I think the speed of the computers, the newness of the field—people had been doing things for years and years and years the other way, so why should they do it this way? "Well, we'll just do it like we've always done it." There were already steam boilers, and there were already pump fluid systems. Those things weren't new. It's the way in which you evaluated them, determined their merit; that was what was new.

WRIGHT: Your work that you had been doing for almost ten years came to realism and a successful trip for Apollo 11. Where were you when they landed on the Moon for the first time, and how did you feel about knowing you were able to participate in that success?

GUY: I was in the Mission Evaluation—MER, it's called—Mission Evaluation Room. From the beginning, the concept of the Mission Evaluation Room—there was a sort of a triad. The MCC [Mission Control Center] has always been MOD's [Mission Operations Directorate] domain, and they had their own back room with its experts, except to us that was a joke. They're not the experts, we're the experts.

Well, then there was the Engineering Room. That was MER, where we were, all the smart people; but we didn't get to talk anybody, because you had to be MOD to talk to anybody. Then there was the Program Office, who thought that they ought to have a place somewhere in this, because it was their spacecraft, so they had their own room. I've forgotten what they called it, the—SPAN [Spacecraft Analysis Room]. SPAN, I think. I don't know what stands for, either.

The bottom line is that we were there for every flight. In the beginning we had all the systems broken out. We had data coming down from all the systems, and we manned the data consoles, and we had com [communication] systems and whatever else. But we were limited in our ability to communicate with the MCC, Mission Control Center. But we had power only through the SPAN. We had intelligence by ourselves, but if you could use the SPAN, then by that time you got up to the program level, project level, and you could exert a little influence on the system. They were part of the loop, so we reported to the SPAN to get things done.

We, of course, knew the back room people and even some of the front room people, and we had the ability sometimes to talk to them and coach them a little bit. But that era was really all about not wasting the expertise that we had. Being a part of that environment was really important, so that nothing serious could go wrong that we weren't able to try to influence systems-wise.

I will admit that the operational focus or perspective is certainly different than ours was. I'm not sure we would have been very good flight controllers at all. But I do think that without some of the insight that we had—for example, I mentioned the freeze-up of the flash evaporator. We had developed test techniques to thaw it out. The MOD didn't know about those. When it froze up, they said, "Now what are we going to do? Are we going to come home?"

We said, "Oh, no, wait, wait. We know how to do that." We called Jim Jaax in, and he told them how to do it. We did.

It was a loosely knit team, I guess you would say, in the beginning. My job, I was always part of the MER. I was shift manager on one of the shifts throughout all Apollo. We had three shifts a day, obviously. The team that our division put forth had spacesuits. It had life support. It had the thermal and all the crew equipment. I was a shift leader for one of those shifts.

Then when we created the EVA Room for the lunar surface activity on Apollo 11, I moved to be responsible for the EVA Room, because I had done all the integrated cert [certification] testing for the system, so that's what I did. For each EVA I manned the EVA Room. We had all the experts, the suit experts and backpack experts and tool experts and all. I was always there.

WRIGHT: And I guess as the missions changed, your expertise, I guess, kept evolving, whereas once they started doing more tool work and excursions were farther—did that affect you in how you tested the suits?

GUY: The one thing that I didn't mention earlier is that my organization, which I said was test management and analysis and integration and all, we also had the Design Group. So when things like the EVA tools and particularly the larger, like the satellite retrieval stuff, we did the WESTAR/PALAPA. We did a satellite retrieval on it. We worked on a Solar Max [satellite] capture system. That was all done by the same branch. But by then, by the Shuttle era—I wasn't branch chief anymore; I was division chief then, but my branch chief was responsible for that. It was Noel [C.] Willis [Jr.] and then Will [Wilbert E.] Ellis, I think was the order.

The whole idea behind the test programs, though, was really utility. I think most of the tools weren't particularly sophisticated; it wasn't like no one had ever made a torque wrench before or a mechanism before. But there was a lot of utility work, where we would take the devices and put the crew in a test environment, and have them use the device and give us feedback as to how the design should be. We did a lot of neutral buoyancy testing. We did a lot of K-Bird [KC-135] testing; a lot of just sea-level testing. And we did suited operation all the time with the tools and with the equipment to make sure they worked.

In fact, we started a problem-uncovering scenario that we, I think, still use today, and that is that we, for any significant EVA equipment, we always run a thermal vacuum test out here in Chamber B with the crew, suited and inside the chamber at thermal vacuum conditions to make sure that the crew can still use the device under the right environment; that the crew is very important to that operation. Because we've already tested the tool in a chamber all by itself, so we already have some information, but to put it in with the crew and have the crew activate it and use it just like it's going to be done. We always learn something.

That's really the more significant, I think, aspect of the testing. I think the other testing is necessary, but the difference is if it's going to be used by an EVA crewman, you'd better do that last step, or you don't know what you've got.

WRIGHT: Was there anything significant that came off the missions that caused you to make any significant changes for the next?

GUY: Several times we had some findings. For example, the cold environment was really too cold for the Shuttle glove. I went through a lot of work to try to develop a system where we keep their hands warm. The early Shuttle flights, the hands of the crew got really cold, and when they're cold, of course, they don't do their job very well, and also it's very uncomfortable. It's all bad. There was a lot of work done there.

As far as the design is concerned, I think that the design involved a lot. We would have areas that would chafe the crewmen, and we'd have to repair or make mods to make it more friendly, anthropomorphically friendly. We've had fogging that we had to take care of in the visors. A lot of small findings. I can't recall anything specific.

I guess the display on the chest, the Shuttle suit has a display unit on the chest; of course, the Apollo unit did, too. It's thermally sensitive. I think it would get too hot, and you couldn't read it. It would fade the display out, or the display would go out. So there have been those kinds of things. The cooling control, I think, was too gross. It needed to be finer so they could adjust it better to stay comfortable.

Again, I'm really not the right person to talk to. Did you talk to Jim [James W.] McBarron [II]?

WRIGHT: Yes.

GUY: He's the right person to talk to for that kind of stuff.

WRIGHT: Okay. All right. Well, we're getting close to the time to stop.

GUY: Yes, it looks like we are.

WRIGHT: This may be a good time, and when we come back, we can pick up with Apollo-Soyuz and Skylab and go from there.

GUY: All right.

WRIGHT: All right. Thank you so much.

GUY: Sure.

[End of interview]