NASA JOHNSON SPACE CENTER ORAL HISTORY PROJECT
ORAL HISTORY TRANSCRIPT

ROBERT C. RIED, JR.
INTERVIEWED BY KEVIN M. RUSNAK
HOUSTON, TEXAS – 7 FEBRUARY 2002

RUSNAK: Today is February 7th, 2002. This interview with Bob Ried is being conducted in Houston, Texas, for the Johnson Space Center Oral History Project. The interviewer is Kevin Rusnak assisted by Sandra Johnson and Jennifer Ross-Nazzal.

Thank you for taking the time out to spend with us this morning.

RIED: My pleasure.

RUSNAK: Good. Well, if we can start out, tell me about any interests you might have had in aviation or science growing up, the kinds of things that might have led you on to a career path that took you to the space program.

RIED: Well, my father was an engineer. He passed away when I was very young, but I felt like I wanted to be an engineer. He was a practical engineer. He had his own company.

So when I went off to college, I focused on being a practical engineer and particularly in materials metallurgy, which is part of his expertise. I discovered that I'd probably be fifty years old by the time I was an expert in that area, so I slowly became, if you want, more interested in science, more theoretical things.

I had very little money. I was interested in pursuing graduate work. I also had a girlfriend. I was going to get married. So I was looking for a position for a couple of years to make some money and then go back to graduate school. I had used some NACA [National Advisory Committee for Aeronautics] reports in my work in undergraduate school, and I was very impressed with those reports. They were sort of in between the journal articles and textbooks, and they were very complete. Of course, Sputnik had come along and space activity was starting, but I was so focused on my studies, I really wasn't aware of the broader world at the time, you might say. I was interested in doing research and gaining an understanding of things.

When I interviewed NASA, I looked at it as the NACA. Like I said, I was interested in doing research, so I asked if that's what I'd be doing and was told yes. So I accepted a job offer with them. It was not the most lucrative, but it seemed to be the best foundation for what I thought I wanted to do.

Came to work at the Space Task Group at Langley [Research Center, Hampton, Virginia] and at the time I didn't realize the difference between the Space Task Group and the Langley Research Center. I was interested in doing research. So after a few months when I figured all this out, I was trying to transfer to the Langley Research Center and had a very interesting opportunity there.

But Bob [Robert E.] Vale, Aleck [C.] Bond, called me in and said, "You don't want to do that. We've got some real challenges here in space flight." They interested me in a particular challenge on the Apollo. By the way, my very first job when I went to work was working on the Apollo. That was in 1961. It was sort of, well, looking at the configuration and the aerodynamics, but I had no view as to what Apollo was to become at the time.

In any event, there had been some newspaper publications that one could not bring men back from the Moon because of the thermal radiation from the gas cap. It was a nonequilibrium phenomenon, which I had done a little work in nonequilibrium when I was an undergraduate, and it was a real challenge. They said, "Your job would be to take two years and understand this phenomenon and let us know, indeed can we come back from the Moon or will it burn up the capsule or whatever. We don't really understand what this is." That was exciting, and also the time frame was right, because I wanted to go back to graduate school.

So I started to pursue that. There was a contract that had already been under way to AVCO [Corporation] Everett [Massachusetts Division] research organization, which was an outstanding small elite research organization. They had shock tubes that were far in excess of anything that NASA had at the time and were most appropriate for understanding or doing diagnostics on very strong shock waves.

Briefly, at equilibrium for Apollo entry, the gas cap at equilibrium is about 10,000 degrees Rankin or 6,000 Kelvin. That's equivalent to what the apparent temperature of the sun is; that is, the sun's radiation is like black body radiation from about 10,000 Rankin. So the equilibrium temperature of the gas in front of the capsule coming back from the Moon was basically the same as that. But the nonequilibrium temperature right behind the shock, if one does a quick calculation, is like a hundred thousand degrees. That is what gave rise to the predictions that the radiation from that high temperature would burn the capsule up.

Well, it turned out to be very interesting phenomena. There were diagnostics made at AVCO and other places, but primarily at AVCO that could get the higher shock speeds, characterizing the radiation behind the shock. What happened was the temperature indeed went to very high value, but all the energy was in translational form, not in the excited states that are needed to radiate. So the radiation, indeed, would shoot up and exceed by two orders of magnitude the radiation from the equilibrium gas, but it happened very quickly, and so the total integrated radiation coming to the capsule was limited. It turned out actually that the equilibrium radiation was more important to the heating to the capsule, comparable to the convective heating, which was all new at these high velocities, if you want.

So my first real challenge was to understand that. AVCO did all the real work. They took the data, did the basic analysis, but the idea was for me to take that basic information and apply it to the entry of Apollo and understand what the heat transfer would be so that we'd understand better what the thermal protection or the ablator characteristics had to be. So that was my first job, and it was a lot of fun. I really enjoyed it. I felt like indeed I was contributing, although once we understood it, there was no doubt that we could bring people back from the Moon, but initially there was a question, like lots of other questions.

It was also the first time I ran into the bureaucracy, if you want. This contract had been in place prior to my joining that particular group, and it ran for two years to get adequate data to understand that, which was my time constant for understanding it. As I mentioned before, we then understood that the equilibrium radiation was more important to the heat transfer and we needed an accurate assessment of that radiation for the heat transfer to the capsule. So I'd asked for an extension of the contract to AVCO to get some additional spectroscopic data and characterize the equilibrium radiation.

Well, I don't remember the details, but it turned out that someone in Washington had decided—and at this point we had moved to Houston, it was the Manned Spacecraft Center—that we don't need some young kid at MSC monitoring a contract with these great people at AVCO. This is more for our senior scientists at the research centers, and so they, which completely oblivious to me, had pulled a maneuver where they had a contract with AVCO and when I tried to renew the contract to get the information that we needed for the heat transfer to the Apollo, I got a telephone call and said, “You can’t do that. We already have a contract with them.”

So I had to explain to the people in Washington the need for this information for Apollo, and they modified their contract a little bit so we eventually got it, but there was a year delay, unfortunately, so that hurt the design a little bit. But their intentions were honorable. MSC was new, it was a project center, they don’t do research, so they don’t need this basic information. But as a matter of fact, we needed that very basic information because it was far beyond our experience in terms of thermal radiation from high temperature gasses. We needed accurate numbers, and it was way outside the experience base. We eventually got it.

The Langley Research Center did a project, staying with this radiation heating, where they had a small replica of the Apollo, and they fired it into the atmosphere. Its whole purpose was to make measurements of the radiative and convective heating. It was a Fire One, Fire Two project. It was a Fire Project. They had essentially a radiometer at the center, a little window, with a calorimeter behind. I think that might have been what we put on Apollo, but in any event, they had an instrument on Fire to measure radiation at flight conditions comparable to the Apollo. They did that.

Unfortunately, the practical things always get to you. The first flight, the data was extremely strange, it varied up and down, up and down, and the magnitude was not at all what we were expecting. It turned out in order to get to the lunar return conditions, they went up essentially to orbital conditions and then had a stage that fired the capsule down into the atmosphere, and they had a spring system that separated the two. All of the calculations showed that it would clear nicely.

But what wasn't anticipated evidently, as I understand it, was the wake from the capsule gave less resistance to the kick stage, and so it basically rode the wake up and clobbered the capsule. So when the capsule was coming in, it was doing this, and as a result, the center of the capsule, which was to be at stagnation point, was off center all the time and nutated around. Because of the oscillations, the characteristics of the gas that it was looking at were changing all the time, so the radiation, which is very sensitive to that, was going up and down.

The second flight, however, was very good. They had a better separation mechanism when they understood what had happened to the first one, and we had some beautiful data. That actually formed the best basis for the correlation of the nonequilibrium radiation. It was pretty much what we were expecting, except it, as I mentioned, simulated as best they could the entry of the Apollo capsule, and what could not be done in the shock tube on the ground facility was to get to the very low densities at high attitudes. What we discovered with Fire was that there was a phenomenon what we termed collision limiting. There were not enough collisions to excite the energetic states of the nitrogen and oxygen atoms or molecules so that the radiation never actually occurred at the very high altitudes. As we came down in peak heating, we did get the radiation.

So I correlated that, and later on on the test flights of Apollo, 501, 502, that came in simulating the lunar return entry, the correlation was quite good. In fact, Chul Park, a fellow from Ames Research Center [Mountain View, California], I'm bouncing way ahead here, we had different views as to what the nonequilibrium radiation phenomena were. In fact, we had planned to have an aeroassist flight experiment here fairly recently before I retired to look at the radiation at higher velocities and higher attitudes. We were interested in designing, just like we have a shuttle to go from the Earth to lower earth orbit, we had designed, had a concept for a vehicle that would run from lower earth orbit up to geosynchronous orbit, where most of our assets in space are, to provide service and repair and so forth, and then just come back to lower earth orbit, never come to the ground.

Now, that was a challenge to get to a reusable system because of the heat shield. It was a challenge to develop a reusable heat shield for the Shuttle. We did master that, but we're really pushing the limits of materials, material temperatures. So now to come back from geosynchronous, which is like coming back from the Moon, it's far more severe than Earth entry, we really had to understand that environment quite precisely to design a vehicle to do that.

In any event, we had planned an aeroassist flight experiment which was a small capsule, kind of like the Fire was to Apollo, okay, to verify our understanding of the entry environment and the thermal protection performance. Chul and I had quite a disagreement about what was going on. I just recently received a paper from Chul, and he has concluded that the difficulty in terms of our dispute—he's very much a theoretician, does outstanding work, but his results and my results were always quite different. I had basically a more empirical approach, if you want, basic phenomenon, but empirical approach, and the correlation between the Fire and the Apollo results, I had a lot of confidence in that. But his theoretical calculations were significantly higher. Well, I just, as I said, received a paper just recently from him where he has now sorted out that the problem was actually in the National Bureau of Standards f numbers. This is a characteristic associated with the probability of a radiating states for atoms and molecules. With some new computations, they have upgraded those, and so now we're very much in agreement. [Laughter] That's just sort of, if you want, my specialty in radiation.

RUSNAK: Of course, you can say it wasn't his fault that his numbers were poor.

RIED: No. No. We're now in complete agreement. Well, he does things that I could not do. He's quite a brilliant theoretician and with the computational power that exists today—we were working on vacuum tubes in Apollo days. Nowadays, in fact, when we get into the Shuttle, we'll talk about some computational fluid dynamics [CFD] there.

But in any event, the radiation was a real challenge. When I completed it, I was ready to go back to graduate school, and NASA basically said, "Well, you can go part-time here at Rice [University] or U of H [University of Houston] and continue working. We'll give you a little time off." Well, by this time I was married, I had at least one child, and I still didn't have very much money, so that was a great deal, and I really enjoyed the work with NASA.

On the Apollo, I was focused pretty much on the thermal radiation. When the Shuttle came along, I had broader responsibilities, and there the challenge was to develop a reusable thermal protection system [TPS]. On Mercury and Apollo, we had a very thick ablator, and it basically is a material that's consumed, if you want, as you come in. For the manned vehicles, we come in at high altitudes, relatively low dynamic pressure, so that astronauts do not experience too high a deceleration as opposed to, for example, in a missile that bore right in at very high pressures. So even though it's all entry heating and thermal production, the two areas are actually quite separate. The thermal protection on the manned vehicles, most of that energy that is received as heat transfer is reradiated, as opposed to at the high pressures the penetrating entries, it's more ablated, if you want. The material is consumed.

So that led us to with the Shuttle challenge, the big challenges on the Shuttle were the avionics, the reusable engines, and the thermal protection system. I never had responsibility for the thermal protection system, but I did have responsibility for the environment, the heat transfer convective and not much radiative for Shuttle.

So on the Shuttle, which was a big engineering challenge in many respects, to build a reusable vehicle, on the thermal protection system, we were really pushing the edges of materials. The candidate materials when we started really would not do the job. The ceramics that were later developed in concert with the Shuttle just barely made it. We got a lot of bad press about these fragile tiles, but the problem was the high temperatures that we experience. The first requirement was to make the vehicle very large, which reduces the temperature. The heat transfer is inversely proportional to characteristic dimension. So for a large-scale system, you have a better chance, if you want.

But what I learned from Apollo, even though I was responsible just for the radiative heating, we came back and about half of the ablator was charred on the Apollo, order of half, except for one region near the front where there was a pressure gradient and you actually had flow through the ablator, and that we had just about the right amount of ablator in that region.

But anyway, for the Shuttle, we couldn't afford two thicknesses of thermal protection system. It had to be reusable. Looking at Apollo, I remember the meeting with Joe [Joseph F.] Shea and Dale [D.] Myers. Dale was at Rockwell [International, Corp.]. Joe Shea was the program manager at Apollo. A presentation was give and by [R.] Bryan Erb, and it was entitled "To Shea (Touché) and Myers." [Laughter]

But in any event, the conditions for the design of the Apollo ablator, there was just a little bit of conservatism in each area, that is, the heating environment, the material properties, the temperature limits from the material standpoint, the structural temperature limits, the trajectories, in terms of where we might fly. On Apollo, that was a crash program, and we didn't have computers to do all sorts of Monte Carlo simulations. They were just starting that type of capability. So the environment was established for the design of the thermal protection system, which was obviously what I'm discussing, was established on a basis of what are the limiting trajectories. The one limit was to penetrate in at the limit of the deceleration that an astronaut could stand. Obviously if you went any deeper than that, the design didn't matter.

The other limit was if you stayed in the atmosphere for a long time such that if you were just a little bit higher you'd skip out and be gone for another two weeks, if you want, which we also could not tolerate, it was a narrow window. But the environmental limits in terms of the heating to the vehicle between those two were quite significantly different. In fact, the [AS-]501 simulated fairly closely the one, the severe environment, and the 502 simulated the other, which was to fly along as long as you can. That's lower heating, but a much higher heat load. Those were the two conditions for the design of the thermal protection system. Very conservative.

But the real conservatism in the design that led to twice as much ablator as we needed was not in any one area, but rather each area had put in their 10 percent, and when you take all the different areas and multiply it all out, that's a factor of two. So having learned that, what we did on the Shuttle was we tried to avoid what I called a compound conservatism. In other words, the system design involved a statistical consideration of all uncertainties in the input. Then, if you want, the program manager could design whether he wanted a one sigma or two sigma or three sigma heat shield.

That was kind of a novel concept in spacecraft design and so forth. We never really got a hundred percent approval because it was a little different from designing airplanes and spacecraft, but that's effectively what we did. As a result, we ended up with a thermal protection system which was just right, in many respects. Nothing's perfect, but we certainly didn't have twice the thermal protection system that we needed on the Shuttle, as we did with the Apollo. So that also was a big challenge.

My area of responsibility, again, was the environment. There's no significant thermal radiation, but we had to understand the gas, the gas flow around the vehicle. It was very three-dimensional, far beyond the ability of wind tunnels to simulate on the ground, and so we had to do some significant analysis and get to a good understanding. Now, that type of challenge, if you want, has been with us all the way through the space program. In the early days of Mercury, which I didn't work on, that was an all new environment. I mean, we had zero experience in terms of what the environment was and what was needed for thermal protection.

Then we went to Apollo and essentially twice the energy, more than twice the heat transfer and heat flux. With the Shuttle now, trying to develop a reusable system, again, it wasn't a simple capsule. It was a very three-dimensional flow. Apollo was also three-dimensional, but it wasn't nearly as significant as it was with the Shuttle. During Apollo, we had done some very rudimentary computational fluid mechanics, which is the current buzz word, if you want, just for the radiation, because the radiation is a very strong function of the temperature. As you get flow around the vehicle, the temperature is highest in the stagnation region, characteristically, and then falls off slightly as you go around the vehicle, but that slight fall-off was a tremendous fall-off in the radiation heating.

So we had to model the gas flow around the Apollo vehicle. Now, for the convective heating what one needed it was the surface conditions, and we had a lot of insight from the wind tunnel as to what that was. For the flow around the vehicle, we were not close to that condition in the wind tunnel and so we had to do a lot of jerry-rigging and engineering, if you want. I mean, it's based on a fundamental understanding of what's going on, but the ability to compute all that didn't exist.

So, for example, the shape of the shock around a vehicle, we would take a schlieren in the wind tunnel, which is characteristically like a Mach 8 condition. We had to go to Mach 33. This meant that the density relative to the free stream in front of capsule increased by a factor of three at least so that the shock approached the body. In the limit, if you go to infinite Mach number, the shock basically conforms to the body. So we approximated that and then made calculations of the temperature around, so very rudimentary flow field calculations in order to compute the radiative heating.

So I was a little aware of the importance of understanding the entire flow for radiation for Apollo. When we got to the Shuttle, the complex geometry that we had, we had to understand the flow in order to provide the convective heating at the surface. So we, along with many others around the country, were pursuing computational fluid dynamics and basically pushing the computers to do what we needed to do. Now, by the time we actually flew the Shuttle, we were able to compute heat transfer along the surface, not over the entire vehicle, but over the hottest regions, if you want, about as accurately as you could measure it in the wind tunnel. Today I think we could do even better on regions of the vehicle where we have difficulty measuring in the wind tunnel because of not as severe environment, but an important environment.

So there's been quite a revolution from vacuum tube computers, if you want, to computational fluid dynamics, which can, if it's done correctly, give you even better information than the wind tunnel. Now, a lot of people say, "Well, we don't need the wind tunnels anymore," and we don't probably need as many. But when you get an answer out of a computer and you get information out of a computer, how do you know that it's correct? How do you know there's not a bug in the system and you're computing something that looks pretty but has nothing to do with reality?

So what we did with the Shuttle was we used the computation of fluid dynamics to compute what's going on in the wind tunnel, and we measured in the wind tunnel, and when they didn't jibe, either we weren't measuring properly or we weren't computing properly, and that was a fun challenge to the Shuttle.

RUSNAK: That happen often?

RIED: Yes. Now, less with time as we learned, okay, that experience in terms of what you can and can't do. The equations are valid, the computer is valid, it just does what you tell it to do. But marrying the two together to get legitimate description of physically what's going on is a challenge and is still a challenge. That area is advancing rapidly, not just in computational fluid dynamics but the principles are being used in many, many different areas now.

So the reason for all that discussion is I started this saying I was interested in research and understanding and pursuing challenges in that area, and, boy, we really had them in the space program. I enjoyed them more being close to the application than I think I would have if I had been in a research center where 100 percent of my time was focused on the basic work. The marriage, if you want, the need for the research really makes it a challenge. It makes it a lot of fun.

A lot of research is done just because an individual is interested in understanding, and that's great. But when not only an individual is interested but there's a need for it, you need it to bring a man back from the Moon or you need it to get a reusable vehicle, that really makes it a challenge and exciting, a lot of fun.

We learned from each step, if you want, from the various projects that we've had. I'm reminded, Dotty [Dorothy B.] Lee used to tell me about Bob [Robert R.] Gilruth and PARD, the Pilotless Aircraft Research Division, and the fun they had, but that experience, in my opinion, was crucial for the ultimate success of our space program because we had people who had small projects, had responsibility, and as Guy [Joseph G.] Thibodaux says, got to make mistakes and learn from mistakes. These little projects of launching an experimental vehicle on a rocket to make some measurements of heat transfer and aerodynamics taught a lot of people lessons about how things actually get done, and that experience basically just blossomed into experience with Mercury and then with Apollo and then with Shuttle.

That element, I think, was necessary for a successful space program, particularly on our side, that is, the manned vehicle part of it. I very much support the concept of small projects for people to develop with so that we can then do big projects, because if you don't have people with experience sort of all the way across the board, it's much easier to make big mistakes instead of little mistakes.

When I was growing up, when we had "a" telephone company, the reputation of the telephone company was that when you went to work for them, you started by climbing the poles. In other words, you started from the ground and you worked your way up so that someone who became a big manager in a telephone company at least had experienced all levels of activities. I think that was a very good approach, and to a certain extent, we kind of fell into it, but that's sort of what we did with NASA. I think it's a very important aspect that many times is overlooked. We go off and study things in school, but that's not like actually having done it, if you want.

You need people who have done it at all levels in order to make decisions relative to what should be done.

I'm a little concerned right now about NASA from the standpoint of we have grown from small projects, based on NACA [National Advisory Committee for Aeronautics] and other organizations, into these large ones, and through that development phase, we had people who had experienced various levels. Well, now we seem to have all these big projects, and the people coming in don't have the experience base of the small projects and what's involved in having a broader picture of the projects. So people come in to a large organization, and they have a view of just a narrow aspect of things. Obviously, we still can do things, but I don't think it's anywhere near as efficient as if you have people who have a broader experience base. I'm not sure I made myself clear on that, but it was sort of a unique opportunity we had, a unique time.

RUSNAK: Well, I was wondering if for people coming in during perhaps the later part of Apollo era or beginning of the Shuttle development if there was the opportunity to work on these sorts of small projects.

RIED: There was some, but not as much, not as much. That's something actually I tried to push is small projects, like I mentioned the aeroassist flight experiment. We had some other projects. But I found difficulty in having the small projects. On the one hand, everything has to be approved at all levels and so whether it's a billion-dollar project or a million-dollar project, those numbers seem to go all the way up to the highest levels, and so to do a small project is almost as much trouble as doing a large one. So if you're going to do battle for resources to do this, that, and the other thing, well, why not go for the bigger one?

The smaller projects are a small group of people that have a good objective and so forth, but they don't have the visibility or the resources to go all the way. They need to have representatives all up and down the line. Well, if their representatives all up and down the line have had that experience base, fine, but when they haven't had the experience base, each project looks larger than it necessarily has to be.

Another way of doing that obviously is to have more delegated responsibility and more freedom of flexibility where the objectives of the organization are well established by the Congress and the executive branch, but the actual implementation is more delegated. Our system is so efficient now that in the budget every little detail is listed and so whether or not this particular project goes or not has visibility at the highest levels. Well, that's good in some respects, but it also makes it a bigger deal than it needs to be to do that particular objective.

The other problem that I ran into was when we were sort of trying to develop small projects, everybody wanted to work on them. Oh, that looks like a fun thing, if we're going to do that. So pretty soon you had an army of people that wanted to work on this project, which I felt like what we needed was a lot more small projects for people to work on, and then each of them could be more efficient. But doing things on a large scale is a lot more difficult than doing them on a small scale and generally a lot less efficient, so.

RUSNAK: Would you think something like Apollo fit into that mold?

RIED: Well, like I said, from the PARD and comparable experience in the rocket business, as opposed to spacecraft business, in time that all fit together very nicely. Today I don't see many small projects going on. They have kind of gotten lost in, well, why are you doing that, which is necessary for the large projects, but I just don't see many small projects. I'd like to see a lot more smaller projects, and then I think we could maintain an experience base and a development path similar to a unique time that I was privileged to experience in the growth of the space program. In some ways, we're too organized, overorganized now, and as a result, it's less efficient.

RUSNAK: When you're talking about small projects and obviously you see part of the purpose of these is to give people this experience, essentially training, or on-the-job sort of training, what other sort of justification for these kinds of projects would you see, direct applications or just kind of as a research?

RIED: Oh, each project, I mean, it should be a competitive process to get to valuable projects that are meaningful and produce results within their own right. For example, at PARD, as I understand it, we'd gone beyond the capabilities of wind tunnels in terms of our need for information in terms of speed, if you want. So each of those projects had particular objectives to get heat transfer around a particular configuration, to get aerodynamic characteristics for support of all sorts of things.

So, no, I'm not maintaining that we do small projects for fun just so that people can gain experience, but that's where the real progress is made in terms of understanding, in terms of capability development. It is going on, it's just not going on as much as I think it should go on. Part of that is because we're so well organized. We know exactly what every individual is doing to a certain point. What we need is more of a process, in my opinion, that is competitive but gives rise to challenges. I enjoy challenges. I described a few of them that I had, unique experience obviously. I really enjoy it. I felt like I contributed as we solved each of those problems and made each of those steps. I think most engineers and most people really enjoy being challenged. That's the thrill of life, in many respects.

In large organizations, it's very easy for people not to be challenged. It's just a job. I just go to work, punch my timecard or whatever. I'm exaggerating, but that's the other limit, as opposed to organizations, whether it be private, government, whatever, where the people are challenged. You can't wait to get to work. The problem is to force people to also have a family life and other activities. That's kind of where we were in the space program. We were so excited about our work, sometimes we lost perspective on more important things in life.

But in terms of organizations and productive organizations, I personally think that it's important for the individuals to each feel challenged and to take on greater challenges. That's sort of the experience that we had in the space program, because none of this had been done before. You'd solve one problem, and there's another one that either crops up or all of a sudden you're smart enough to realize that it's there.

I guess I sort of gravitated into advanced programs, as they call it, relative to trying to develop technology for future things. I think probably the most fun I ever had as an engineering job was working on a solar power satellite. Back in the energy crunch days, that was an option which when you first hear about it sounds absolutely stupid, but after you think about it, it makes some sense.

But the challenge I had or the fun that I had was doing engineering on a vehicle the size of Manhattan Island. There's no precedent for that. No published papers about, okay, this is how you do this, all brand new. Well, it'd take a long time to go into great detail, but the solar power satellite is immense. The concept is to be, for example, in geosynchronous orbit gathering the sun's energy, then transmitting that energy through microwave beams to the Earth, and then providing electrical power into the power grid, the advantage being that the sun is there all the time in geosynchronous orbit, with the exception of a few blinks at a couple times a year, and so you've got continuous energy coming into the system.

The inefficiencies associated with the conversion are in space, so you don't have even the waste heat associated with developing the energy. It makes a lot of sense from an overall standpoint of providing energy for people on the planet. The difficulty with it is you don't really make money until you do the whole thing, and to do the whole thing you need to build up an infrastructure on the ground, the transportation capability in terms of rocket systems, etc. You have to build the satellite, and like all solar energy, you make all that investment up front. It has to be a massive project, and if you make some mistakes, they're big mistakes. So the problem was, again, to have sort of prototype systems.

But anyway, the reason I say I had so much fun with that is the system design, from a structural standpoint is one of dynamics. There are no loads. You're sitting up there far away from the Earth, sort of in a balance in geosynchronous orbit. The solar pressure of light is significant in terms of loads. So it becomes, the system becomes what I call sort of a stiffness design. You have to point the system to the sun. You have to point the transmitting antenna to the Earth, and that's all you have to do. So you don't need big steel I-beams and that type of thing, but you're covering the area the size of Manhattan Island, so it's kind of like how do you make tin foil stiff, if you want.

But the fun part was when you tried to explain a design or concept, and you talk in terms of natural frequencies or whatever the system. They're outside the realm of anybody's experience in terms of constructing things. But when you sort of back-of-the-envelope it, again, envision a system the size of Manhattan Island, okay, in geosynchronous orbit and it has to maintain its shape. Well, you've got beams and so forth and whatever the configuration. Well, by analogy, those beams provide a stiffness between different points, which is kind of like the interaction between molecules in a material. You take, well, something a little more flexible than a cup, I guess. You take a material and its characteristics, its modulus, its density, it's all related to the intermolecular forces. So it turns out you can take this system the size of Manhattan Island and say, well, really the stiffness, the force between this point and that point, is due to this, and that's kind of like intermolecular force, and pretty soon this overall system has a density, you know how much material you have, and you can calculate a Young's modulus for the whole thing. Given the geometry, Young's modulus, and the density, you can calculate the natural frequency. So when you back-of-the-envelope and put it in those terms, oh, yes.

Anyway, it was fun. Because one could relate, if you want, engineering characteristics of structural features of this configuration to materials and their characteristics as a result of intermolecular forces. Again, it was a simple case. It was research in the sense of new designs and new concepts, but it was gaining an understanding. I mean, I better understood materials and material characteristics as a result of doing the design of the solar power satellite. So it's a learning experience. Like I said at the beginning, that's what I've always enjoyed is learning, gaining an understanding, and doing it because it's something that needs to be done, as opposed to just completely academic, wanting to do it. So I had a really great time.

RUSNAK: How did you end up during structure sorts of things on the solar power satellite?

RIED: Well, the time frame was such that we had the environment for the Shuttle pretty well defined, because it had to be defined before the thermal protection system could be defined and so forth. So I was kind of at a lull between having done that work and awaiting some measurements from the Shuttle first flights, which was also a lot of fun. So my boss asked me to help out on this solar power satellite. He said, "I'd like you to look out for the thermal parts."

Fine. I went to the first meeting and walked in ready to do the thermal characteristics, but there wasn't anything to do a thermal of. There wasn't really a basic configuration. Being completely novel, people really didn't know where to start with this thing. I mean, there were cartoons. One fellow said, "Control is a problem, so why don't we just blow up a balloon and that will be the configuration." Well, that had some other problems.

So before I could do the thermal design or evaluations or whatever, we had to have something. So I kind of got involved in what should the system be. It turned out that one of the most important features for the structure was to have a very low coefficient of thermal expansion. To do that required making composite materials and having a design that recognized—again, I mentioned before that the forces are zero, I mean, very small, but you have to deal with them for your system. If you had any shadowing, the temperature changes in space, on one side you're looking at the sun, which is as hot as can be, and the other side you're looking at three degrees Kelvin. So as you go from shade to exposure to the sun, the temperature changes tremendously. So the materials expand when they get hot, and they contract when they're cold.

So that turned out to be a significant feature to the design of the structure. The importance of that is illustrated, for example, as I understand it we went to talk to people working on the space telescope, the Hubble Space Telescope, and they had addressed that issue for the telescope, because of its very tight alignment characteristics. I shouldn't tell on them, but the very first time they put it together and they went to test it, and they were looking for very low coefficient of thermal expansion for the entire system, and it was like ten times higher than they expected. They looked at it, and it turned out the telescope is quite large. The joint, to simplify it, let's say it's thirty feet long for discussion, and at one end the material's made of a composite that has very low coefficient of thermal expansion over a range of significance, and that material is to be bonded against the base in a particular fashion.

Well, they had put the bond the wrong way, and so the glue between the two pieces which had a normal coefficient of thermal expansion, the thickness of the glue over that thirty feet was enough to increase the expansion an order of magnitude more than what it should have been. Once they changed the geometry, then everything worked out very well. So again, the fun that I've had, like they had, was getting into areas where you needed to really understand what's going on in order to achieve the objective.

Like I said, that's what really made it fun, and that's why the solar power satellite was the most fun for me, because there was no precedent with it. Now, we just, we only did a concept. Obviously we're not doing a solar power satellite, although I do think that it's a viable option in the future, again, using this idea that we need small projects as opposed to big ones. It doesn't make sense to argue whether or not a solar power satellite is the right thing for the country or the world to do. I mean, you need to have those discussions. You need to understand whether it's a rational concept or not, but you're not voting on whether or not to commit resources for trillions of dollars to do solar power satellites without having done some small things that give you a foundation, not only in terms of understanding, but also of evaluating just what the advantages and disadvantages of doing things like this are.

For example, Abe [Abraham] Hertzberg had proposed using laser systems for efficient propulsion. As opposed to carrying the whole energy system with you, you have a local power plant, if you want, either in space or on the ground and then a laser system which focuses on your rocket, if you want, and provides the energy to heat the gas which is being expanded from the rocket, so you can, in principle, have more energy, and you can use ion engines and so forth to take advantage of electrical energy. That's an interesting concept, which I think should be pursued for application in space, but also because the power beaming and learning about what the problems are in addition to the basic theory would help provide a foundation for an option of a solar power satellite or similar system in the future.

You learn by doing things. We've kind of gotten into a mode where we talk about billion-dollar projects and we spend a lot of time debating them as opposed to having options available, if you want, for the future and smaller things that we commit and go do and are worthwhile within themselves, and then you kind of knit them together to go whichever direction you want in the future. We did that sort of by accident, if you want, in the development of the space program to where we are, but now we've become so efficient that we're kind of losing sight of that process, if you want.

I don't mean to be so critical of things that are going on, but looking at back on your career and seeing—well, when you're an old retired guy, you look back and remember the good old days and so you're critical of what's going on today, but everything is balanced.

RUSNAK: What did you come away with from your experience on the solar power satellite?

RIED: Well, as I mentioned, I think being able to relate the physical phenomena going on with a system the size of Manhattan Island to material properties at the molecular level and how they give rise to the properties of materials that we normally associate with, I think that's what I really learned from that project.

That was part of the reason, for example, in looking at this orbital transfer vehicle, that's also something that one would need if, as you project beyond low earth orbit and we go out to do things like a lunar base or going on to Mars or solar power satellite or whatever, you need to think beyond transportation just from the Earth to lower earth orbit. Even though that's the hardest step, you also need systems out in space. So again, you kind of have to look down the future to see what options you might want to pursue, and then I would maintain that we do things that are good within themselves but also can be justified in terms of building a foundation for things that we might want to do in the future.

Now, there are some smaller projects in the unmanned space activity, so-called robotic activity, and I think those are great. When you look at a rocket equation, the smaller you can make the payload, the faster you can make it go, particularly with the miniaturization that we're doing today and what we're able to accomplish at a small scale, boy, we need to take advantage of that in our space exploration, robotic as well as human. I mean, within NASA, within the space activity, we're all working for the same fundamental objectives, and we've sort of divided things into robotic and human exploration, and that's appropriate, but I think our activities need to be meshed a little bit better.

For example, there are a lot of small projects in the robotic area that provide an experience base for people, again, an analogy with the PARD and the Mercury and Apollo and Shuttle, a little more cross-fertilization of people for getting the experience base to go on to bigger and better programs as they are needed, I think is very good. Also from an understanding and technology standpoint, I think we need a little bit more cross-fertilization.

When I look back on my career, I just fell into a wonderful time, had a lot of fun.

RUSNAK: Well, were you aware through the majority of your career of these kinds of issues that you've been telling me about or is this something that kind of came up in the latter part of your career when you got into some of these special projects?

RIED: Well, when I first came to work, I mean, I was a complete greenhorn. I didn't understand who was doing what or what was going on or anything else. No, this is something that has developed in time. I mean, the broader my experience, the more I saw as to what was going on. No, it's not something that I just walked in with or hit me one day. It's something that kind of grew with my experience, and it's from my perspective.

RUSNAK: Well, do you mind if I go back and ask you some details about some of your greenhorn days?

RIED: Not at all. Fine.

RUSNAK: Well, you had mentioned early on you were working with AVCO as a contractor. Had you had any experience in either managing contracts, and then what was the division of responsibility there between you and then what the contractors were doing?

RIED: I had no experience in managing contracts. I've never really had a contract management experience, period, other than that AVCO effort, and that was a fairly straightforward one. You could write down in a couple sentences, here's what we need. They were the experts, and they told us everything they were doing, and we either agreed or disagreed with it. If we disagreed, then it may not happen, but they were able to pretty well explain everything that they were doing.

I spent a lot of time up there with the shock tube facilities. I had just come out of college. I knew nothing about the shock tubes. I'd done a little work on high temperature gasses, molecular gas dynamics. I knew nothing about contract aspects or anything else. It was very simple in those days. We really just would write a letter and say do this work. We didn't have reams and reams of paper associated with it, or if we did, they all got filled in later.

AVCO, like a few organizations, they had—don't hold me to the numbers—but they had like on the order of 300 people, fifty of them were senior scientists, engineers, whatever. The management structure was three people. It was a committee. There were three committees. Each committee had a head. Every one of the fifty researchers or engineers basically had to defend their projects and what they were doing to their particular committee. The committees were disciplinary committees. One of them, as I recall, was entry technology. So they had technically very good experts. [Art Kantrowitz] had given them a very efficient management organization, no structure at all, capable people, and they were driven and challenged just like people working in the human space flight program to get a man on the Moon.

But the contract management was we all needed this information, they had facility and they did the tests and they talked with us in terms of how they were doing it and explained to us how they did it, and any questions or problems that we had with it, obviously we made note. It wasn't a "I go tell them what to do." They were the experts in the phenomena. They were the experts in the facilities. They had the expertise.

Now, we also had expertise at NASA, but we didn't have the facilities that they had. People at Ames and at Langley pushed their facilities to the limit to get the best information that we could. We all worked together.

One of the major reasons Apollo was so successful is of the 300 or 450,000 people, everybody knew what the objective was. If my job was sweeping the floor, I knew that if I didn't do a good job and there was some dust that got into some sensitive equipment or whatever, a rocket might fail, okay? So I did a good job at what I was doing. Well, people at AVCO were the same. They were supporting the race to the Moon, if you want.

We had a legally binding setup, whatever that was, but I didn't know anything about that. I was the engineer. There were other people that worried about the contractual aspects and the financial aspects and everything. I, to this day, know nothing about how that's done.

RUSNAK: All right.

RIED: But again, having a little leadership at the top in terms of this is the objective and having that clear to everyone makes for a good project. The same thing relative to the research, if it's clear what the objective of the organization is and NACA and its contribution to NASA, also to aeronautics, unbelievable. But common shared objective, whether it be in a research lab, whether it be in a government or private industry or in a production facility building airplanes or whatever, each person or each organization had their objectives, and they were consistent.

So it wasn't so much, at least my impression today is, we put together these major contracts. Frankly, as I said, I really do not understand that whole system. My bag has been the technical aspect of things. But let me just give an example.

On the Shuttle, the contractor did work obviously in defining the environment for every body point on the Shuttle vehicle. Okay. Well, the contract people worried about, well, how do we know that they really did that and what do we pay them for? Well, the agreement was they deliver this data book, kind of like the Congressional budget, a big pile of paper with numbers on it. So there it is. Oh, okay, now you get paid.

I said, "Well, I don't know that all those numbers are right unless I have a staff that does the same work, and we don't want to duplicate it." So to me the product is an explanation of what they've done and how they've done it. That's basically what we did on the Shuttle. Contractually, they delivered piles of paper, said, okay, here's the—today it would be a CD [compact disc]. But, okay, here's all the numbers, or here's all the whatever. But what I looked at was what's the basis for these numbers, how did they get them, what did they do, go through the basic rational. On the NASA side we would be doing basically the same thing. We had this arrogant outlook that, not that we were smarter than anybody else, but that we understood this better than the people actually doing the work. Now, that sounds irrational.

For example, on the Shuttle, they had to define the environment all over that vehicle, every nook and cranny, okay. I had the responsibility for the environment definition, but I couldn't generate all those numbers. I had to understand what do we know and what don't we know. What are the limits to our knowledge, and how do we deal with those limits? In other words, in principle, how do we do these things? I had to and the people working with me had to go through the process. How well do you know the viscosity or the conductivity or properties of a gas in the Shuttle's lower temperatures? In Apollo, for example, at 10,000 degrees, what's the basis for those? How do you get that information? How good is the conductivity or viscosity at those conditions? We had to understand the fundamentals. What is the state of the art? What data do we have? What theoretical assessment do we have?

On the NASA side, we really had to understand those things in order for the environment to be defined. Now, they had to understand them also, but they had to understand them more in the standpoint of how do we handle this particular piece of geometry and this particular facet of defining the environment? We had to understand it from a fundamental standpoint and be able to carry it through to being able to generate specific numbers.

Now, like I said, the deliverable on that was not we ran so many wind tunnel tests and it cost so much and we produced all this much paper, it was how good is that environment and have you covered everything? We couldn't afford to say, well, make the heat shield twice as big as you need and that way we'll be safe, because on the Shuttle that's taking payload off. We wouldn't be flying Shuttle if we'd done that.

So there's a fundamental difference between what we had to do as the NASA or government side of things versus what the industry was doing. Well, there was overlap, but there's a fundamental difference. Sometimes that difference is overlooked. The need for having people who truly understand what's going on sometimes is overlooked, and that's when big failures occur.

Like I said, I think that fits with what I started with here in terms of I was searching to do some research and have some understanding. Now, I haven't developed a new relativity or anything like that, but I've had a lot of fun, as did all the people working with me that I worked with. It was a fun time. It really was.

RUSNAK: I've certainly heard that from a lot of people.

RIED: Well, when old guys get together for breakfast or whatever, we all kind of sit there with a smile and say, "Boy, we had a unique opportunity. We really fell into it."

RUSNAK: It was a good time.

RIED: Yes. It was fun. We had leadership at all levels, from "within the decade" down to the lowest level supervisor who had an experience base and was working with his people. In fact, I don't remember exactly where it was, I read some analysis of management organization within the space program at NASA, and they said, "They didn't understand it." Nobody really knew who their boss was. Nobody knew where the organization fit, but everybody knew what the objective was, and everybody was trying to bring whatever they could to bear on the problem.

NASA was a very healthy organization. If there was a problem, the program manager, the administrator, whoever, would be talking directly to the guy on the floor that understood the problem. I mean, obviously the normal information flow went through channels, but as things got critical, there was a particular problem or whatever, boy, that was short-circuited right away. It wasn't I'm talking to the big boss, it's I've got information you need information type thing. It was a very healthy organization, and particularly with people like Bob Gilruth and Max [Maxime A.] Faget, people who had been there and done that, a little different, but they'd done something comparable, they understood.

RUSNAK: Can you maybe comment on some of the leadership that you saw kind of on a daily level, whether it be from a management sort of perspective or technical kind of leadership?

RIED: Well, I think it permeated the organization, both technically and from a management standpoint. You asked me before about the contract with AVCO. I knew nothing about the details of the contract. I was just focused on the work and the product and getting what we needed.

The management was casual. It was kind of like a family or whatever. People didn't care what their titles were. None of us were in it to make money. I mean, I thought I mentioned, maybe I didn't, but when I came to work for NASA, that was half the salary I was offered for a different job. Everybody that I knew could be making a lot more money, if that was their objective, doing something different. Our objective was to explore space and put people on the Moon and develop a Shuttle system, reusable system. So there was a management structure there as required. It wasn't as loose, for example—I shouldn't say loose. There was more structure than the AVCO system that I described, but it was a lot bigger organization.

But particularly in the engineering side of the house, what was important was the product, the basic information, and understanding so that the proper decisions could be made. The decision process was very open. It wasn't, okay, we've considered all your input so we'll get back to you in a week as to what we decide, the decision was made right there. Based on these inputs and our understanding, this is the best thing to do right now and this is why. If somebody, it didn't matter who, in the room said, "But you overlooked," they'd stand up and say so, and the project manager or the supervisor or whatever would say, "Oh. You're right."

Now, that doesn't say that people didn't get angry, but they got angry about whether or not the right thing was being done. They didn't get angry at each other very much. It was an ideal organization. It really was. Not that there weren't problems. I mean, certainly there were problems, but my overall recollection was, boy, everybody's head was screwed on right. You kind of all had a common objective, and there wasn't anybody out to build an empire or do other people in or back stab or whatever. At least if there was, it wasn't visible to me.

RUSNAK: Earlier you were talking about having this understanding of the environment so that you could oversee the products of the contractor, these kinds of things. How did you yourself come to that understanding as someone coming into the space program with essentially limited experience?

RIED: Well, I had a good basic education. In fact, I mentioned that I was going to graduate school while I worked for NASA. For me, that was an ideal. I would go in to a class in molecular gas dynamics, graduate class, and most of the students—we were all peers—I was actually using the stuff at work. So if the professor—or addressing books or the professor's going through something, I immediately knew what was important and what wasn't important, because I was trying to solve a problem that used that.

So it developed with time. I mean, initially I was interested in gas dynamics. I'd done a little bit in nonequilibrium before I came to NASA, and I mentioned the nonequilibrium radiation. That was exciting, but I had a foundation, and I just learned as I went.

With the environment on the Shuttle, in order to do the radiation, I had to understand the flow field, so they were married. I had to understand the fundamental aspects of convective heating and what was going on, because when you get to those high temperatures you begin to question whether or not you've got the right equations. The physics is correct, but the equations get a lot more complicated very quickly. So it was a combination of studying and the work that had to be done. So it, my knowledge, if you want, grew with time.

Again, I don't mean to harp on something, but the idea of you don't just hire an expert to come in and do something. The best expert in the world, if you're a real expert, you're generally fairly focused, and each problem is different. To bring the expertise to bear on the appropriate problem, there has to be communication in terms of what the real problem is, in addition to the required expertise. There were times when we had a massive problem, Apollo 13, Challenger. We had big problem on STS-1 with overpressure. The agency would pull in the best expertise, and you had a combination of the best expertise and the people who were familiar with the system or whatever elements of the problem were. It was kind of a race as to who figured out what was going on first. I mean, I don't mean that in a negative sense. Everybody was working together to a common objective, but both sides had to come together for understanding.

On Apollo 13, I'd never worked in cryogenics. I really never thought about it, okay, but all of a sudden we had the incident with Apollo 13. My boss, Joe [Joseph N.] Kotanchik was running around saying, "Hey, we've calculated it would take so many pounds of dynamite to blow that tank up, and all there is is millivolts going in to this system, where does the energy come from?"

That's a good question. So we started thinking about it. Now, there were people who knew what was important. I mean, the design of the oxygen tank involved a fan or circulating system because when you're in low g, well, if you light a match in this room, it heats, but you have like a candle flame, all right? Because of gravity, the hot air rises and the cold air falls, and so there's mixing, so it never gets any hotter than the flame, obviously, that's the source, but that's spread over the room in time. In space, you have essentially low gravity, not moving, you've got this cryogen, if you have an energy source, it heats locally, but there's no free convection because there's no acceleration or gravity field. So what happens? It gets hotter, and in the cryogen you form a little gas bubble, if you want, and that gas bubble just continues to grow. So with very little energy, you can create very high pressure in low gravity.

Well, the people who designed the tank knew that. They had put in a fan circulating system so that it would circulate things and you wouldn't run into a potential high pressure rise. They knew that in the design. But that information wasn't readily available to everybody. Well, I had never worked in cryogenics. My guys had not worked in cryogens, guys and gals had not worked in cryogens, but here's this where did the energy come from? Well, we figured it out in an afternoon and made some hand calculations, which were an order of magnitude off because of the difficulty working at the triple point.

But in any event, I forget the question now, but the point was that you have to drive to understand the fundamental—oh, it's communicating between. There was an expertise relative to constructing those tanks and how they were designed. A problem occurred. It didn't have anything to do with the design, per se. It had to do with the handling of the tank, as it later came out, but the question of immediate concern is what happened? How bad could that damage be, and where did the energy come from? Well, we were able to figure that out in an afternoon, not having any background in cryogens or anything else, but understanding basic fluid mechanics and heat transfer and with the information that we had. So it's not just having the basic expertise, if you want, it's also understanding the problem, and it's bringing those two together, and that is just pretty fundamental.

RUSNAK: All right. Well, actually if we can take this time to pause so we can change out our tape.

RIED: Sure.

RUSNAK: When we had last stopped, you were telling us about Apollo 13 and some of the related issues there. I didn't know if there was anything else you wanted to finish up on on that flight.

RIED: Well, we were very fortunate on that. In fact, the whole program, I didn't hear it directly, but I was told that after the first successful landing on the Moon and return, Bob Gilruth is supposed to have commented that, "Some day they'll try to do that again and they'll realize what an amazing accomplishment it was."

It was a very complicated system, but we had an approach that basically made sense. The only thing we really did, that I remember, without testing it first was the actual lunar landing. I mean, I mentioned the 501, 502 vehicles that were fired into. I mean, they were command modules. They were on an entry trajectory. They went through that environment just like eventually the men would do. We had 201, 202, which were orbital returns, that went through the entry environment before the people did. Everything we did, we tested to make sure it would work without risking a human life before we took that step. So even though Apollo happened quickly and it was risky, we had a good, rational approach in terms of how we did things. I mean, it's not just at the overall vehicle and system level, it was right down to the nuts and bolts, the same kind of basic approach. We had a tremendous advantage that resources were not really a constraint. But unfortunately we did it quickly, so we can only spend so much money. The objective was not to spend money, the objective was to put people on the Moon.

But relative to Apollo 13, well, I think, for example, I don't remember precisely how quickly things happened, but we had a team. I mean, there was an operations team that were immediately supporting the crew. There was all kinds of supporting teams in terms of what happened, what problems, what other things might happen. There was a high-level investigation team put together very quickly, also. They all worked together.

On the investigation team, Hans Mark, who had just become the director of Ames, was on that team. I think I discussed before we had made a calculation saying, hey, here's where the energy can come from and just really communicate. There were other people that were more expert at that than we were, but we happened to be there and did the analysis.

Well, this fellow in the back of the room kept asking me probing questions, like I knew what I was talking about. He came up later. Well, it turned it was Hans, and he wanted to go to the lab and do some tests. He was a center director. But that was what was important at the time, not how is the budget doing and where are the projections for this, that, or the other thing, but what happened on this vehicle?

I'm reminded of STS-1 and the overpressure. We had a nice successful launch. This is on Shuttle. It had some measurements made on the back of the vehicle that all of a sudden people realized the accelerations were a little higher than they should have been, local accelerations on the structural members, and pressures were much higher than anybody expected, like a factor of ten higher overpressure than they expected. I had not worked on overpressure. But the question was, well, what's going on here?

We almost lost that first vehicle because there wasn't an adequate understanding of the overpressure phenomenon. I think everybody had done a reasonably good job, but there really wasn't an adequate understanding. The overpressure had been characterized for missile launches in silos, which you'd think would really be a bad situation, and there had been papers published characterizing overpressure.

Shuttle came along and went through those solid rockets, and they're virtually a controlled explosion. They start so quickly that the exhaust coming out compresses the air in the vicinity. It's sort of like a piston coming out compressing the air. There had been some work done for silo launches, missiles, and the data was correlated, and that was unfortunately accepted as an understanding. It was an empirical correlation, which was the best that was available, but the situation was a little more complicated than that. With Shuttle, we saved lots of money. We used the launch facilities that had been developed for Apollo. Why build new facilities? It made financial sense. But the exhaust ducts were designed for the Saturn. We'd configured all kinds of stuff to do it for the Shuttle, had to rotate the Shuttle after we launched, etc., but we saved money in terms of not building a new trench and putting in new concrete.

Turned out that the geometry was a very important factor in the overpressure, and the geometry had not properly been accounted for in terms of the phenomenon. It was a big mystery. Where did all this pressure come from? But really, an almost back of the envelope analysis in terms of, as I mentioned, it was sort of like a piston compressing the air, is it one-dimensional, is it two-dimensional, or is it three-dimensional? It turns out the scaling for each dimension is considerably different, and all you have to do is say, okay, I've got a one-dimensional or two-dimensional or three-dimensional piston, and all of a sudden you go from being independent of the rise rate to linear with rise rate, which was the empirical correlation from the silos, or is the rise rate squared, which was the case on the Shuttle, because it was a three-dimensional compression.

Once we understood that, it made a lot of sense. Twenty-twenty hindsight was great. Fortunately, the structure was robust enough it could withstand that overpressure. So then once we understood it, well, what do we do about it? It's kind of late to go dig a new trench for the exhaust. So Max Faget came up with the idea of, well, we need a barrier, but the barrier would get blown around.

There were some tests done later to get some data, and the overpressure on the—I might not have numbers right, but in perspective—the overpressure on the base heat shield was like three psi or so above ambient. We had made some quick calculations that said pressures down below were significantly greater than that. So there were some tests done in a scale facility and then eventually on the MLP [Mobile Launch Platform]. They put in pressure gauges that went to fifty psi in the hot region, if you want.

We said, "Well, those aren't going to be high enough." Sure enough, they pegged. It got over a hundred psi down there, because those solids started very quickly and it was, indeed, a very three-dimensional flow, like I said. Anyway, there was an awful lot of pressure. So if you put a barrier between the vehicle and the overpressure, that barrier gets blown away. Well, Max came up, oh, how about water? The vehicle has to withstand water anyway, and you don't feel good if you get hosed down, but it's not like getting hit with a brick or a flying two-by-four.

So that idea, plus putting together his sewing team to make the troughs out of a lightweight material, came up with a barrier that is to my knowledge still being used, and that took care of it. But again, first it's fundamentally what's going on, and then what's going on in this particular situation due to the geometry, the high rise rate, etc. See, the scaling was from a

Tomahawk [cruise missile] size, well, a Tomahawk was a one-fifteenth scale SRM [solid rocket motor], basically, and the facility had two Tomahawks set up with a one-fifteenth scale orbiter and ET [external tank] and MLP and all that. At one-fifteenth scale, it's 225 times if you square it in terms of what's going on. So that would make pretty refined measurements and scale those up. Who would think that you'd multiply by 225 or by a factor of ten instead of a factor of three in terms of the rise rate correlation? But that's what it turned out to be.

But you have to understand fundamentally what's going on in the application and then, and Max really was the one that came up with the idea of the water troughs, how do we deal with it in a practical sense. There wasn't much time, at least we wanted to have a second launch in not too long a time.

RUSNAK: The first launch had another problem losing some of its tiles on the OMS [orbital maneuvering system] pod, which I guess would kind of fall into your area of expertise.

RIED: Oh, yes. I mentioned that we had a pretty efficient design on the thermal protection system. Normally one thinks of the windward side, where all the high heating is. There's a lot more area on the lee side, and even if you put just a little thermal protection system over that large area, it's a significant weight, so tremendous pressure to reduce that to be minimum. Max used to joke, "All we need is a coat of paint," because on the Mercury and Gemini configurations, there was a lot of weight on the thermal protection system in the separated region, which in hindsight wasn't needed but it was very hard to determine because the wind tunnel really didn't simulate the entry conditions that well. Even if it did, it was very difficult to measure in a wind tunnel these very low heating rates and correlate them. It's actually more complicated than on the front side.

So he felt very strongly and most appropriately, in fact, he told one day, he said, "Bob, I want you to burn at least a small hole someplace on the lee side." Well, as close as we came was the OMS pod, and again that was the difference between the wind tunnel condition and flight condition and the nature of the flow. That was beyond our computational fluid dynamics before we flew. We just about got there. Plus there was also a combination of, again, a lack of simulation capability. We needed the heat, if you want, the hot environment, as well as the pressure. It was a combination. Didn't actually happen at peak heating, it happened a little bit after peak heating, the scrubbing action at the higher pressure. Pressure continually increases as you come in.

The other thing, well, the lee side of the vehicle is still a challenge for any vehicle, even for a simple configuration, because on the windward side of the vehicle, one can pretty much assume an adiabatic flow, that is, the kinetic energy of the gas comes in, it's converted into thermal energy, and you basically retain that energy.

On the lee side of the vehicle, it's a big wake region, and the gas that's there is there for a long time, so it actually gets cooled. It's not an adiabatic flow situation. It's far more complicated, even for a symmetric simple configuration today, plus it's unsteady. The flow is unsteady. It can be done, but it's still a real challenge to predict heating accurately on the lee side of a vehicle or in the wake of a vehicle.

The environment, as I mentioned, is very small or low compared to the windward side. So it's a challenge of a little bit over a large area. For any real configuration, you have local hot spots where the heating can be significant for a short period of time or significant just at a particular angle of attack, a particular Mach number and all that, and you go through a complete spectrum. Getting the resolution and the understanding of that is still a challenge today, frankly. So we did damage some TPS on the OMS pod, which we then beefed up with some tiles, and that was fine.

Relative to losing tiles, if we lost enough tiles in the right region, we could, indeed, lose the vehicle. On the other hand, we also had computed if we lost a tile anywhere, we would not lose the vehicle, not because of the thermal protection system, but because we used an aluminum structure and aluminum has a tremendous conductivity, which is great. It's hard to weld. You can weld steel easier than you can aluminum, even though the aluminum is a much lower temperature because it's got just a good conductivity compared to steel.

Well, that was a very good choice for material for a thermal protection system, as a structure under the thermal protection system, because if there was a local failure, the conductivity of aluminum basically saved you. So we were not worried, actually, about a single tile coming off. We were worried if we lost more than one tile in particular regions in areas.

Also what was of concern was the flow between the tiles, the gaps, which were later stuffed significantly. Again, the flow gets a little bit more complicated as you get to a greater level of detail. It's one thing to compute the environment over a simple configuration, another step to get the environment over a complex configuration, both empirically in a wind tunnel as well as now with computational fluid dynamics. It is a third level of significance or difficulty to get the flow between the tiles and the associated heating and the associated thermal relief of that heating.

On an exterior surface, the tiles can take extremely high heating rates and reradiate the energy, and the way they get rid of the energy is to radiate it away. In between the two tiles, they just radiate back and forth to each other, so there's no heat lost, and they're very poor conductors. So a little bit of heating between the gaps, and the temperature goes real high. The temperature goes real high, and the gas stays real hot. Ordinarily the flow in the gas would be nonadiabatic, that is, a little bit of gas comes in, it's hot as can be, but it's cooled by the surfaces of the thermal protection system or the tiles. It's a complicated problem. Plus the geometry is such that the flow-through through those tiles is affected not only by the overall flow but the pressure distribution in terms of if you want sort of like a leakage. The worst thing you can do is to punch a hole through the vehicle that would allow gas that's very hot to flow through. That just is a catastrophic situation. So having the whole thing sealed is extremely important.

Stuffing the tiles was our approach to not being able to understand the flow between the tiles in coupling with the complicated geometry of the orbiter and the fact that the tiles all have to fit together. Today, with current computers, we probably could do an even better job, although each tile would be unique, and then you get into the practical problem of if you drop one tile, it's unique and you need a replacement for that whole tile, so you need complete ship sets for those things. The practical problems that they these get complicated pretty quickly.

Another thing that we learned with the Shuttle is what does it cost to do things? A congressman comes down and says, "Where's all this money gone?" Well, they show him a vehicle. He looks at the vehicle and said, "Oh, boy, that's an expensive vehicle." Well, the actual vehicle didn't cost hardly anything. The money went into all the people who are employed to do the design, development, and everything else, but you have this image of that vehicle.

One of the objectives of the Shuttle program was to reduce the high cost of launching things. Saturn was very expensive. So we wanted to make it as reusable as we could, which is a good objective. You build this expensive piece of equipment and then you throw it away. That's costing a lot of money. You've got to go buy another one. Well, the problem is that unless you've got a fleet, you've got sort of unique hardware, so each piece of hardware is maybe not custom built, but effectively custom built, and so it in itself is expensive. So if you throw them away, they're expensive, but if you don't produce them in mass production, they're also expensive. We learned that there's probably a nice optimum in that curve which is not completely reusable, but our objective was to have it as reusable as we could.

The tile systems were highly criticized because there's so many parts. It's not metal, like God intended. It's a ceramic, and that's brittle. It's a glass you can break, but it went to high temperatures. At the time of development, it was the only thing that possibly could have made the Shuttle fly, to tell you the truth. Now, we've extended the capability of tile systems, and metallic systems have also been developed, and I don't know what's best now, but at the time I think what we did was probably the only way we could have gotten there.

Your question was? I've forgotten your question now.

RUSNAK: The original question that got us on this line, I think, was just about the missing tiles on STS-1.

RIED: The tiles got a lot of flak because it was different and new. Airplanes are built out of metal, but they don't have to come back from orbit. They don't have to experience that severe environment, which, unless you've designed one or gone through it to a certain point, you don't really fully appreciate.

A lot of concepts that people have in their mind is based on their experience, and sometimes they're good and sometimes they're not good. There are a lot of people who feel like they understand high speed hypersonic flight, and they look at biplanes and say, "Well, they go slowly, and they have straight wings. Then you go a little faster and you sweep the wings, so obviously in you travel hypersonically, boy, you've got to really sweep those wings because you're really traveling fast."

Certainly for missiles that have to penetrate, they indeed are penetrators through the atmosphere. But spacecraft that you want to bring back, the job is to brake the vehicle from its orbital velocity of 17,000 miles an hour to landing speed. It's not something that you fly around and try to get from San Francisco to Paris in a short period of time. You're trying to dissipate energy. So even though the Shuttle is not as slender as, for example, a jet fighter, it's certainly a slender vehicle compared to a capsule.

But a lot of people never really understood how a capsule worked in terms of atmospheric braking. The idea is to dissipate all that energy in order to land. With the Shuttle, to make it reusable, we had to fly it at a high angle of attack, so if you want, conceptually it looks like a capsule coming in when it's being heated, and then it has to tip down to be able to land like an airplane. That's quite a challenge.

But the point being relative to your question, airplanes are made out of good, solid metal. They have to be very efficient structures, and they can withstand certain types of loads, and some types of loads they cannot withstand. By analogy, in terms of thermal protection, you have to be able to withstand high temperatures, and metals are good at a lot of things, but they can't absorb all that energy. So tiles were a good design and are a good design, but they weren't understood by a lot of people without the expertise in that area, but expertise in comparable areas. So tiles got a lot of bad press.

Plus, they were expensive to make each one, and we weren't making them for a fleet of more than four vehicles, so I mean it was mass production in terms of making tiles, but because of that aerodynamic shape of the Shuttle, there are a lot of unique tiles. So they were expensive, but they could do the job.

Today we have not only extended the material temperature capability in the tile system design, as well as better understanding the environment and how to fly vehicles and so forth, but there's been a lot of understanding in different areas that make the job a little easier. But building something with today's technology, building anything less than half-scale Shuttle would be a tremendous challenge. In fact, there have been attempts that haven't gotten too far, which we were well aware of in terms of the difficulty.

It's been a lot of fun. I really enjoyed it. I learned a lot. We just scratched the surface. We still have an awful lot to learn. I don't know what's going to happen tomorrow, none of us do, but putting a man on the Moon, putting a space station in orbit, by analogy, for the Polynesians, that's like putting their toe in the water. I mean, the solar system is big, the galaxy is immense, and the universe is much bigger than that. I have no doubt that some day we'll be putting a base on the Moon, that we'll be going to Mars, and probably beyond that. Certainly I'd like to see the robotic systems really press the frontiers.

But when, I don't know. I once read that after major significant accomplishments it takes like fifty years until you have another one. The first transatlantic cable was laid in year X. Everybody thought, wow, that's great, but it took fifty years before people really started to take advantage and use them and they laid the second one. We've done some pioneering things in the space program, but it looks like it will be a little while until we push the frontiers again, the way things are going. But I'm convinced we will.

But again, getting back to pushing for small projects, I think it's academic to argue about the big major programs until we take some steps where we learn more, put some more robotic systems on Mars and on the Moon and find out indeed if there's water on the Moon or on Mars. Well, we used to know what moons were. We had one, and we looked through even a Galileo telescope and we'd see other planets that had moons, so they had moons just like ours. It wasn't until we sent systems out and started taking even pictures of these things that we realized, gee, some of those moons are a little different. Every time we get a little information, we learn something, and there's an awful lot to learn, an infinite amount in space. What we do in the future is anybody's guess, but we have to be taking steps and opening our options, in terms of what we might want to do, and not worry so much about—well, we have to worry about dollars, obviously, but we spend a lot of time arguing about how much this cost and how much that cost.

I don't think people are challenged enough today. I really do think that in the good old days we were more challenged, and people have gotten into—well, not people but organizations have gotten people into boxes where they're not challenged. You stay within your box. You have this job to do, and you have an input, and you take that input, and you have an output, and if everybody does their thing, the whole thing works.

Well, that's fine to a point, but people don't advance, the organization doesn't advance, and things don't get accomplished beyond the routine. We're getting a little too routine about how we do business. I think we need to shake things up a little bit, have some challenges, and not worry so much about trying to get more money to do things but rather how do we produce more with resources that are available, change the system a little bit. But that's easy for an old retired guy to say.

RUSNAK: Well, I think perhaps with the new administrator that NASA has, they'll at least get a little bit of shakeup there, so we'll see what impact that has in the long run.

RIED: I certainly have no idea. Sounds like a good guy. He certainly picked a good deputy. I'm optimistic.

But there have to really be some changes made, because I think things have gotten completely out of hand. I don't think there was foresight or leadership at the top in terms of where we're going, and as a result I think the troops are confused, and as a result there are a lot of resources just going down the drain. But again, for an old retired guy, it's easy.

RUSNAK: Well, I know we're getting near lunchtime, so I don't want to keep you too much longer, but just to kind have a couple wrap-up questions from me, when you're looking at the Shuttle Program as it's getting into sort of an operational mode where they're flying with at least some frequency and essentially the same types of missions at least in terms of the reentry environment, what kind of work does that leave you as an aerothermodynamicist to do? Are you constantly evaluating data coming back from the Shuttle?

RIED: No. From the fifties on there was a lot of attempts, both in ground facilities and in flight facilities or flight testing to get basic data, better understanding. Fortuitously when we flew the Shuttle, we got probably the best data ever obtained and probably far beyond anybody's expectation due to unique circumstances of the tile systems and the instruments that were onboard. The initial flights were well instrumented. You got a fantastic amount of data, and it taught us an awful lot, not only at the basic what's going on, but at more and more detailed levels like I was describing before relative, for example, flow-through gaps, catalysis factors that affect the heating. In other words, it's not just a gas and the heat transfer, it's the surface and what it does chemically as well as obviously mechanically.

I thought we would be in a development process for vehicles that go beyond low earth orbit at this point, and that's why we were working on transfer vehicles, how does one design a reusable transfer vehicle? In other words, from an aerothermodynamic standpoint only, we've got a Shuttle. We can improve on it. The next generation system can be a much better design, because we've learned a lot. If we want to come back from the Moon or from geosynchronous orbit with an efficient system, a reusable eventually, we have to understand that environment much better than we do. So there are a lot of challenges there.

I mentioned the computational fluid dynamics. In the ascent environment on the Shuttle it was not precisely what was predicted, and it really wasn't understood. The best simulation on the ground facility could not duplicate the aerodynamic characteristics. Now, they're a small factor in ascent. Most of the factor is the rocket engine and the thrust, but in any event the aerodynamic performance was not exactly what was expected. We had a fellow that investigated—a lot of people, but one fellow in particular, and he found that the more detail he put into the geometry, the closer he got to the data, in other words, as you got to each nut and bolt, almost, in order to explain the aerodynamic drag and forces on the vehicle. So there's no limits to what you need to do to get to a more refined or improved design, a more efficient design, even at the low speeds. At the higher speeds you get into phenomena we haven't dealt with before, so there are a lot of basic questions.

Also, going into the Mars, Mars does not have air as we know it, oxygen and nitrogen. It's got a different gas. Those characteristics are significantly different, not only from transport property standpoint, also from a standpoint of the radiation characteristics. Radiation is a lot more important in the Mars atmosphere than it is in the Earth's atmosphere. Every time you go into a different atmosphere, it's a whole different ball game, and new fundamental questions come up.

But you've got to be going and doing things. If you wait and say, "Okay, well, some day we might go to Mars," all of a sudden there's not time to treat the problems adequately, and so you have to be conservative in the design, and that really costs you in the long run in terms of what you can do. Not only that's assuming that everything goes well, but in terms of the risk. The more you understand, the less risk you're taking.

But even though my reputation is an aerothermodynamicist, I don't limit myself to aerothermodynamics. We discussed the structures work I did on solar power satellite. I'm just representative of the engineers that we have at NASA and probably engineers anywhere and people anywhere. A good challenge is worth the effort. So I don't limit myself to the box of being an aerothermodynamicist. I had a lot of fun in that box, but I sure don't confine myself to that box.

I mentioned before about more of a union between the robotic activities and the human exploration. What unifies them, if nothing else, is the technology and how you do business, how do you do things, and your understanding of what you're having to deal with. We would like to go on to put a base on the Moon, learn more about the Moon. We'd like to go on to Mars. We'd like to go on to Europa and better understand that unique moon. Is there life there, whatever. There are all kinds of fantastic challenges.

I don't blame anyone for this, but we should be addressing those challenges instead of arguing about what the next billion-dollar program is going to be. We ought to have activities that enable the people who can contribute to those challenges and getting an understanding be able to do it by, if you want, delegating responsibility a little bit more. I'm hopeful that our new administrator and deputy administrator will be able to do that. With all the things going on in the country, I hope that there will be a little better perspective for the need of delegating and not thinking that the one person at the top can do everything and tell everybody else what to do. The system doesn't work that way. Whether it be space program or energy business, it doesn't work that way.

But the thrust of your question is where do we go from here or what happens to all old aerothermodynamicists? I don't know where we're going from here. Hopefully we'll get to a little bit more efficient organization where people are more challenged, and I think as a result the concern about costs can be dealt with up front instead of in the back end. So I'm very optimistic about the future. But there are some bumps in the road. We have to learn some things the hard way.

Like Guy Thibodaux said, "You've got to allow people to make mistakes if you're going to advance," and we're just not making enough mistakes now. We've got this concept that we don't make mistakes anymore. Everything is perfect. We now have computers, and they can figure everything out for us, so we don't make mistakes. We certainly need them to work with us, but if we're not making any mistakes, we're not making any progress, and so I think we've been too concerned about making mistakes and not having a system set up where you allow people to make mistakes but little ones, so that you don't make real big ones.

A lot of exciting opportunities with space station, working with the Russians, I think, and all of our international partners, I think is fantastic. I mean, to the Russian people their space program represented the epitome of what their country could do and what they could do, and truly they have been very practical in what they have done. They have had tremendous accomplishments, and particularly for their economy and their system, they work wonders. Working together with them I think is great. I think that's where the focus ought to be, working with the Russians, as well as all of our other international partners.

But there are different ways of doing that. Remembering back on Apollo, that was a very sophisticated, very complicated system, and there were tremendous integration problems with it, but they were dealt with up front. You go build a rocket, we'll go build the manned portions, and this is our interface. We didn't try to build a rocket, and they didn't try to build the spacecraft. They didn't supply this, that, and the other for the spacecraft, and we didn't supply this, that, and the other for the rocket. I'm not saying that's the best way to do things, but what's important is to up front assign responsibilities and delegate. That way, there are always going to be problems, but the integration can be a lot more straightforward.

Because we have computers? I don't know what the rationale is, all of a sudden everything has to be integrated and the same. I think space station should have been put up in about three years, period, and then we should have taken advantage of what we've learned and put up another one in another three years. It probably would have been a lot cheaper than—my analogy, if you want, is sort of the Dulles Airport. Dulles Airport was designed back as the jet commercial aircraft were coming into play, and boy, that was a big job. You had to take into consideration that jets were a lot bigger, a lot noisier, a lot faster. They needed long a runway, so they needed to be away from the cities. They needed to have a very efficient design, and it was very efficient on paper. But when I fly to Washington, I like to fly into Washington National, not into Dulles. It takes a lot more time to fly in and out of Dulles. The flight time, if you get the schedule everything is great, but then when you land, you've got to get on a bus in order to get to the terminal, and then you have to get on a bus or something in order to get to Washington.

When I try to get on an airplane—and I'm not criticizing Dulles per se, but just using it as an example—if I go to get on the plane at Dulles, I have to go through so many different gates. I mean, to get into the terminal, I have to get on a bus to go to the right terminal, then I have to get on a bus to go to the right plane. Each time I get on a bus, it's not load the rear seats first, it's whoever got the bus in whatever order, that's how they get on. So getting on and off an airplane at Dulles, it takes a lot more time and effort for me as a passenger than going to Washington National.

The point is on paper it looked great. I mean, these jet aircraft are so big that we don't want to pull them all up to the terminal. There are advantages and disadvantages in everything, but there's a tremendous advantage in learning from a little experience. Obviously, I'm an old retired guy. I'm talking about experience here, but we have tried to do everything up front on paper, work it out perfectly, where then all we have to do is see all the cookie cutters and go put it together. That is the most expensive, most risky way of doing business. It's much better to evolve and take one step at a time.

Even though we did it quickly during Apollo and the Shuttle, I think that's kind of how we worked, and we've sort of gotten away from that a little bit. Everybody knows that we're spending a lot of money in space. Well, it's not much by some comparisons, but it's a lot of money, and so we don't want to do anything until we have all the nuts and bolts in place, and that doesn't really make sense. We need to have more of an evolutionary program that has a little give and take, if we truly want to get ahead in space and explore and do the wonderful things that we could be doing.

So I think we need to look at the core of how we're doing business on some things. Again, from an old retired guy who learned form the past, learned from my mistakes in the past, and maybe try some different things. If they don't work, get rid of them. But I think we need to try some different things than what we're doing.

RUSNAK: I think some of the original concepts for the space station had the sort of approach like you were talking about, we pay for this sort of core segment now and then we add different ones as different needs arise or as different budgets come in.

RIED: Yes. Yes. An awful lot of things you can do in space. You've got the vacuum to deal with and also play with. You've got primarily a very low gravity environment, which is a unique situation. I talked a little bit before about some of the fluid dynamic problems that are different. All kinds of things to learn there. Then there's the very fundamental thing of human beings and how they function or living organisms and how they function in space. But to just put up an outpost and say, "Okay, we're going to do all this stuff," a lot of those things are in conflict.

For example, if I want to do ultra low gravity experiments, I probably would benefit easiest from a free flyer that goes and does these things. The question is what are we doing and how and what makes sense and what doesn't make sense. Trying to figure all that out in advance and go build a perfect system that will enable you to do all that is the most expensive way in the world to do it. To me it seems much cheaper to do it on a small scale, try things and learn what works and what doesn't work, and evolve from there, taking into account that safety and human life is very important.

My great-grandfather died in a fishing boat sinking off New Jersey back before there were regulations about having lifeboats and life vests for everybody onboard. The boat went down, and there was just no way people could make it. So we need to always remember that the unexpected can happen. We need to have capability of life vests, if you want, and lifeboats to handle the fundamental lifesaving capability. So there some principles that we need to abide by, kind of like we did with Apollo. You don't put a man or a person into the system until you're confident it's going to work. That's by having it work.

So I'd just like to see some attempts at doing things a little differently so that we get a little bit more efficient and challenge people more. I had a lot of fun as did my peers, because we, indeed, were challenged. It was fun all the way. People are challenged today, but I don't think quite as much at the individual level. There's a big group challenge. How do you get 400,000 people together—well, not that many today, but however many to go do this, and I think we'd be a little better off to putting it into more bite-sized chunks. I seem to be on a theme here today. [Laughter].

RUSNAK: There's nothing wrong with a thematic discussion. Your remarks about your great-grandfather reminded me of one of my questions. One of these things you worked on was an alternative escape vehicle for the space station, if I'm not mistaken.

RIED: Oh, yes. Yes. I had the pleasure of having a bunch of young people working for me in advanced programs, which is kind of novel. Normally you put the old folks in charge of advanced programs so they take advantage of the experience, but circumstances were such that I had the advantage of having some young people.

It was very simple. I said, "I think we need escape capability for the space station, and I have some ideas, but I don't have all the answers." But the challenge I gave them, I said, "Okay, picture yourself up there and something happens. There's a fire, a meteor goes through the thing, complete blackout, whatever, I mean, some catastrophe that we did not anticipate, and you've got to get out of there. Or if you don't think you're going to be on a station, your son or your daughter is up there. Now, give me a system that will enable them to live." I said, "Now, for example, my idea is no computer at all. I want like a life vest or a simple lifeboat that I can jump in, punch out, and come down and have a little GPS receiver that we could locate wherever that system is and go rescue the people."

Now, the young people said, "No computer? You can't work without a computer. You've got to have a computer to tell you where you are," and all that stuff, and I lost that battle. However, the computer, as we refined the design a little bit, the computer had to constantly update it and you had to check the batteries or the power source or whatever to make sure the computer was working, so the computer—I'm not arguing there shouldn't have been a computer, I'm just pointing out that I was pushing for a very simple system, and they came up with a simple system.

There's a problem on impact, back on Apollo. In fact, I was not involved in the structure on Apollo at all. On the other hand, one of the first tests of the Apollo impact, just dropping the command module into the water, you never know what the sea state is. The loading is pretty dramatic, and there was a congressman or somebody coming down, and we were going to have a test. What do we do with the congressman? Somebody, "Oh, we'll show him this test."

One of the structures fellows had done some computations said, "You better not do that."

They said, "What do you mean?"

He said, "Because it's going to sink."

Nobody believed the calculation, but that's what happened. We learned some things.

Anyway, we had some ideas in terms of how to absorb the energy on impact in terms of the basic design. In other words, this is not a Mercury capsule, it is not a Shuttle, it is not one of these other things, it's an escape vehicle. I think they came up a reasonable design that could have been built in-house for not that much money and replicated for not that much money. But I don't know who took the proposal forward, but what happened was, "What? JSC has a proposal for a spacecraft. Well, it's probably going to cost ten times what they think," and so the price was going up by a factor of ten, which I couldn't believe, but maybe that was good, I don't know. Then it got through Budget Office or whatever and they said, "What? Oh, that's probably ten times too low," so I was up in a billion-dollar program for a simple little thing which might not even have had a computer in it, but would have served a particular function.

I'm not saying that it was absolutely needed. I think we've got a good program. We've always got systems on board they can jump into, and there are different approaches for this. I'm not saying we have a unique answer, but I would say that each problem, to the extent that it's unique, has a unique solution. We tend to try to adapt solutions that we have, because we look at the hardware and say, "Oh, my goodness, that's expensive hardware," so we try to use that hardware for other purposes. Sometimes that makes sense. Many times I think it's erroneous, because what's expensive is making very few, period.

I mean, when you make a spacecraft, to make a second one, I don't know what the price is, but it's certainly not twice. In fact, I would guess it's more like ten percent. So what's the optimum? Our cost models, I think, need an awful lot of development. Not that there aren't a lot of people working in those areas, but I think there are some fundamental assumptions that are made that probably are not right or probably can be altered significantly.

We've gotten a little bit into too much tradition, too much this is the way things are done. It's complicated, but again, I think trying some things and see what can be done differently. There are examples. I mentioned the AVCO organization as just one having a different management structure. There are organizations associated with universities or whatever that have done what are considered miraculous things. Why? What's different? Oh, well they're smaller. Maybe. Maybe it's just the way they're doing business. Maybe they're utilizing their people instead of telling them what to do, which really, again, that's how we put people on the Moon, is everybody understood the objective, and everybody was fighting to contribute and do something that would further the objective, and it happened and on schedule.

I know nothing about cost models. I mentioned I know nothing about contract process, nothing about the legal aspects, but I think there's a lot of work that's needed in that arena, because I think there are a lot of assumptions that are made that are just flat wrong, or you can just go out of the box and change things significantly.

But your question was relative to the emergency escape. Yes, I mentioned my great-grandfather to the young people that came up with the design, and they had a lot of fun. They learned a lot, and they came up with some good ideas. Not that there aren't better ideas, but they did some good work.

RUSNAK: Well, one more question from me. I was thinking that over the course of NASA you've got this civilian agency going the space program, but you've also got on the military side they're looking at they're the ones that are doing the pioneering work in some of the ballistic reentry and then later on they're trying things like the Dyna-Soar program and other efforts at both manned space flight and unmanned. I was wondering what sort of information exchange there was between NASA and the military in terms of their programs, particularly in the areas that you were interested in.

RIED: Generally very good. If you get the working-level people communicating, the exchange is fantastic. You get a military aerothermodynamicist and a civilian aerothermodynamicist, if there is such a thing, you get them together, they can communicate very well. To the extent that that has occurred, been very good, and that exchange has been good.

From NASA's standpoint, the civilian agency was chartered with just open information, and the military, by definition, is not. I mean, you don't want the bad guys to know what you're doing or how you're doing it and everything else. So there's this basic conflict there. But putting that aside, I think communication and exchange has been generally very good.

I ran into great difficulties working on Apollo in that the military wanted to classify everything we were doing because it might have application to their stuff. So I had to be very careful there, because working for NASA, my intent was to distribute information to everybody so it could be used either in aerospace or whatever industry. So it was just a different outlook and mind-set, but the cooperation was good.

The experience base is different, and as a result you come to different conclusions, so you have to realize what the experience base is and where people are coming from.

I think we've had problems from the standpoint of trying to have one size fits all types of things. You look at these billion-dollar programs again and say, "Well, can't we use that for this other billion-dollar program" or whatever and that gives rise to—but everything is a compromise, and there comes a point where you have to make a decision. If you try to do everything, you get nothing. If you do just one thing, it's very expensive, so you make rational decisions.

I think [President Dwight D.] Eisenhower was brilliant in making the agency a civilian space agency, particularly as it played such a role in the cold war, where we were open and everybody could see what we were doing. I think that in itself was a big major contribution. Having the technology that's developed open is good.

When I was a senior in college, I would never have worked for the government. In fact, I just refused to ever work for the government. I ended up going to work for NASA, which really wasn't the government. It kind of became the government after a while, but I've learned a lot since then. I recognize that there is a role for the government certainly in R&D [research and development], and there's a role for industry, and the two are separate, and yet they need communication and cooperation and they can both benefit.

But I also learned there's private industry, there's government, and there's government contractors. They're not exactly the same. Private industry has a customer and customer base. The government contractor is trying to satisfy the rules and regulations and the system for him to get the next contract, if you want. I mean, there's competition, but it's not competition across the board where the consumer is making the judgment. It's a competition where you go through the wicket for what the selection rules are. I think there's a confusion between private-enterprise free-competitive systems and government contractors, if you want. There's a spectrum there that sometimes is overlooked.

But anyway, my point being that I do feel that our government and our developed civilization has the responsibility in concert with the universities and academia to look toward the future. I'm not saying necessarily like the way Japan does it is necessarily a good idea, but I think the government does—which I didn't recognize before I came to work for NASA that the government does have, in my mind, a responsibility to kind of push R&D of a common interest that too high risk for any individual company to invest in.

In this country, it seems like the private enterprises are kind of eliminating their R&D activities. To a certain extent, that makes some sense for them to specialize. There's a lot that can be done in universities, but again there's a whole spectrum and there's a gap between the fundamental developments and the application that NASA has kind of filled in aeronautics and space. I think there's a responsibility that the government has in many areas to provide that type of capability, if it's needed. Because it won't be done by industry, per se, private industry enterprise, per se. It won't really be done by academia. If it's in a crisis situation, it's the most expensive way. I think having an investment in the future and a process by which how that investment is allocated would make sense, which is a complete change from when I was a senior in college when I wanted absolutely nothing to do with the government. People are the same whether they're government employees or private industry employees or government contractor employees. I think everybody wants to be challenged, and I think everybody wants to contribute. We all finds way of doing it, in spite of the systems.

Well, that's gotten a little far afield from aerothermodynamics, which is supposed to be my expertise.

RUSNAK: That's okay. Like you said, you don't want to stay within that particular box. It's good that you were in an environment that allowed you to explore outside of that and didn't keep you confined to just any particular area.

RIED: I think everybody can step outside their boxes and contribute, and as they do, it's generally recognized. I think those are the fun things. If you find a solution in the box, that's fun, but if you find a better one outside the box, that's even more fun. It doesn't hurt to have leaders that say, "Within a decade."

RUSNAK: There's right. Well, if you don't mind, I'd like to have Sandra and Jennifer step outside their box running the equipment and step into their historian mode and ask some questions, if they have any.

RIED: Sure.

JOHNSON: I don't actually think I have any right now. Thank you.

RIED: Thank you.

ROSS-NAZZAL: I was just wondering, you were talking about the escape vehicle and how much different what they came up with was from what they actually used, the Soyuz. Was it just as simple?

RIED: Well, Soyuz is available and it's simple, and I have no problems with Soyuz. When we were addressing the system we came up with, which was just our idea, the biggest problem that we had was everybody wanted to satisfy their requirements with a new system. Just to exaggerate but make a point, I was worried about a fire, a meteor, something that we did not anticipate. The medical doctors were worried about things that they could anticipate, people get sick, break a leg, whatever, and so their requirement, what they wanted was something where if somebody had a heart attack on a space station, they could put them in the ambulance and the ambulance with land at the local hospital here. That's a whole different system and different set of requirements, which leads to a lot more complicated vehicle.

What we had proposed was, okay, this is the simplest system you have. That's sort of the baseline, if you want. Then you have to make a rational decision as to what sort of capability would you like to have as a function of the probability of needing it and the cost and all other factors. So the biggest problem we ran into is in terms of explaining and presenting our concept was, well, it doesn't do this other thing. It doesn't do that, or it won't land at the airport, or it won't come down at a scheduled time so that we can get to it within two minutes or whatever. It was sort of a requirements growth on the system, which is a natural process, but you need a baseline, which is kind of what we provided, a simplest system possible to do the basic job, and then it's a question of where you go from there.

Soyuz is an established system. It works. If you can jump in and push the button and get out, that would satisfy my requirement, if you want—not mine, but, I mean, the basic requirement.

We always have preconceived ideas as to how things are based on our experience. I'm certainly an example of that in terms of what I'm saying right today. But we need to examine those and identify what's the problem and what are we trying to do, what sort of capability would we like or can we afford, and not necessarily just as an entity within itself, but how does it fit with everything else that we're doing, which is an overall organization with leadership and delegated responsibility.

ROSS-NAZZAL: Thank you.

RIED: Thank you.

RUSNAK: Well, as I said before, I'm out of most of the questions that I had, but I did want to give you a chance if there's anything else you wanted to remark on, any other thoughts before we close for the afternoon.

RIED: I think I've philosophized about as much as anyone can. [Laughter]. When I was young, there was absolutely nothing I couldn't do. I wondered why all these old codgers had great reservations, and certainly at NASA and throughout the space program, industry and throughout the country as young people come along and don't realize that things can't be done, that's how things do get done. So I guess if I want to summarize with anything, it's now I'm an old codger, and I can't do anything anymore, but when I was young, there was absolutely nothing I couldn't do. So everything in life is a balance. This has been fun.

RUSNAK: I'm glad you enjoyed it. Okay.

[End of interview]