ORAL HISTORY TRANSCRIPT

R. BRYAN ERB
INTERVIEWED BY CAROL BUTLER
HOUSTON, TX – 14 OCTOBER 1999

BUTLER: Today is October 14, 1999. This oral history with R. Bryan Erb is being conducted for the Johnson Space Center Oral History Project, in the offices of the Signal Corporation in Houston, Texas. Carol Butler is the interviewer and is assisted by Kevin Rusnak and Sandra Johnson.

Thank you so much for joining us today.

ERB: My great pleasure. Thank you.

BUTLER: You have had quite an extensive and interesting career in aviation and aerospace. To begin with, maybe we could talk about how you got interested in the field in the first place and how that led to you becoming involved with AVRO [A. V. Roe Aircraft Ltd.].

ERB: Well, it occurs to me that I got interested in space before I got interested in aviation. I think every kid growing up in the thirties, forties, looked at airplanes and thought, gee, that was pretty neat. But I recall a time many years ago, I think I was in the fifth grade, at Earl Grey Elementary School [Earl Grey was an early Governor General of Canada] in Calgary in western Canada, when we were visited by a person who was billed as an explorer. This guy came and talked to our fifth grade class, and he had a big black bag of things that he was showing people, and one of the things that he had was a shrunken human head he had picked up in Borneo or someplace like that. That was what intrigued my classmates, but what captured my imagination was that he said, "One day man will go to the moon."

I went home and told my parents that. I guess I was about eleven years old at the time. And my mother, of course, sort of pooh-poohed this. It just wasn't absolutely something one should think about. But at any rate, I sort of hung onto that notion.

Many years later I got into the aviation area by being interested in flying, and did some private flying in the early fifties, but came back eventually to space through my connection with AVRO.

BUTLER: That's quite an interesting way of getting involved in it. That's great. You went into college and you worked in going toward the aviation area, obviously because at the time space wasn't going. How did the opportunity then arise for you to get into working with AVRO?

ERB: Well, I was taking civil engineering as my undergraduate pattern, but I was always more interested in the fluid mechanics and fluid flow side of things than the structures and the bridge-building and that sort of stuff. I don't know whether there was anything sort of subliminal, that that was an avenue to aeronautics. As I mentioned, I had been doing some flying lessons and doing some flying.

So the British Government at this time—this would be starting in, I think, 1951, were offering scholarships to Canadian students for two-year study in the United Kingdom, and I was interviewed by this group [the scholarship selection team] when they were crossing Canada in the fall of 1951, just before I graduated. I guess they liked my credentials. At any rate, I was given one of these Athlone fellowships for two years' study in aeronautics at a place called at that time College of Aeronautics in Cranfield in the United Kingdom.

So I spent two years there, and while it was really sort of airplane-focused at the time, there was one very pivotal professor who taught high-speed hypersonics and high-speed flow, and he was very much interested in the British Interplanetary Society and in the whole notion of space travel.

So I did my thesis on a heat transfer problem, which I thought, well, if we're really going to travel away from the Earth and travel to the other planets, we have to be able to reenter the Earth's atmosphere, so this would be a good thing to learn about. This fellow was named Terry Nonwheiler, and I worked under Terry for a year on my thesis project.

At about the same time, it was an exciting time in aviation because these were about the times that people were beginning to go supersonic. [General Charles E.] Chuck Yeager, of "Right Stuff" fame, was breaking the sound barrier in the [Bell] X-1 and so on. But space was only sort of still a dim notion, but the British Interplanetary Society was pretty active in promoting the whole idea of space travel and space activities.

Also during the time I was at Cranfield we had a visit from Arthur C. Clarke, who came to the college and spoke. I found this absolutely fascinating. I'd never been much of a science fiction buff, and Clarke was not as famous then as he later became, but at any rate, it was very stimulating. I read one of his books that had just been published at that time called *The Red Sands of Mars*, and that again sort of got me a little more interested in the space side of things.

When I finished at Cranfield, I went back to Canada, and after about a year or so I was looking for the only employment in Canada in aeronautics at the time, was with AVRO. That was *the* major aerospace company, and they were working on a really neat project, the Arrow Mach 2 fighter. The question was not whether I would join AVRO, but when. My fiancee at that time had one more year to go at the University of Alberta, so I went back to the University of Alberta and took a second master's degree in fluid mechanics while Dona finished up her degree program.

Then in '55, 1955, we were married and moved down to Toronto, and I went to work for AVRO. So for the next three years, I was essentially heavily involved in the aerodynamics area, and my job on the AVRO Arrow was aero-thermodynamics and [heat] transfer analysis, trying to figure out how hot the various parts of the airplane would get and what materials would need to be used to withstand those temperatures. So this was, as it turned out, great training and good discipline for what later became the opportunity at NASA.

It's a bit of a long story as to how we came to join NASA, and I say "we" because there were about thirty or so of us Canadians that joined NASA in the 1959 time frame. The Arrow aircraft was really a world-beater at the time. However, it was too big a project, I think, for the Canadian economy and the Canadian defense establishment to tackle on their own. The groundwork to find a world market had not been done. Nowadays, you know, you would work production sharing arrangements and you would make sure you had a large market before you'd start off in that sort of a production activity. But the company thought, well, if we build a good airplane, the world will come. You know, if you build it, they will come and beat a pathway to your door and buy it, and all will be well. I think we sort of stuck with that naive notion for quite a long time.

At any rate, the project had been started under the Liberal government in the early fifties, and in '58 the Conservatives gained power, and at about the same time the Canadian defense establishment decided that this was a pretty expensive project that was going to totally consume their budget, and so the upshot of all that was that the Arrow project was canceled. It was canceled in an atrociously abrupt fashion. One Friday morning we're all sitting at our desks working away, and a notice comes out on the loudspeaker that the contract has been canceled, all employment is terminated, and so something over 13,000 people were out of work just like that, on the streets.

I think we sort of entertained the notion that this was irrational, would get turned around, you know, and all would be well. Sort of the denial phase of a tragedy. But it didn't happen. After a week or two it began to sink in that, gee, we'd better start doing something.

I had two young children at the time. In fact, our daughter was only three months old when this hit. So finding a way to put bread on the table was very important, and under those circumstances you start pounding the pavement and deciding, okay, now what are we going to do.

Well, to backtrack just a little, in about '57 the Canadian military, who had been sort of following and monitoring the Arrow project, began to find in their analyses some questions about what the company was asserting the performance of the aircraft would be, and they raised, I think, some twenty-one points at which they disputed the company's estimate of performance.

The company had taken the data from the old NACA [National Advisory Committee for Aeronautics] reports. At that time, as you may be aware, the NACA, the predecessor of NASA, was sort of, in the Western world, at least, *the* source of information on aerodynamics and aircraft performance and all that sort of stuff. NACA had the best wind tunnels in the world, the best staff. Anything to do with airplanes, the data came out in what were then called NACA reports.

The company position—and I should say I was not personally involved in this, so this is something that my superiors were involved with—but the company position was that the NACA data was correct, but it had not been interpreted correctly, and our Air Force people had taken the NACA interpretation and not looked hard enough at the data.

Well, the upshot of that was that a group of AVRO engineers went down to Langley [Research Center, Hampton, Virginia] to discuss this with NACA, and I think this was in around the 1957 time frame. During the course of these discussions, the AVRO engineers convinced the NACA folks that indeed they had misinterpreted some of the data and that the company was right on virtually all, I think twenty out of the twenty-one, points.

This was apparently quite impressive to the NACA folks, so two years later, when the company folded and all of a sudden 13,000 people are on the streets, at least probably, oh, 400 or 500 engineers, NASA was interested—it was by then NASA—was interested in trying to tap into some of these folks.

So the chronology of this was that the contract was canceled February 20, 1959, and within about three weeks a team from Space Task Group at Langley came up to interview a group of us. They suggested what their requirements were, and something like 150 names were proposed by the company as people that would meet these initial requirements that NASA had stated. I think they interviewed most of that number and extended offers to about fifty of us, and thirty to thirty-five of us ended up going down to join Space Task Group at Langley.

I think at that time the Space Task Group had consisted of people from the old NACA and a few engineers from industry, but it was a fairly busy time in the industry in the United States. There weren't a lot of people floating around available for employment. So this was a pool of talent that NASA found and was very eager to tap into.

So in 1959, we packed up our family and our kids and possessions and headed down to Langley, and I joined the Space Task Group in May of 1959. Somehow all of these things just sort of seemed to come back together. Here I was indeed working on putting man in space. In my wildest dreams, I could never have imagined a scenario playing out to that effect, so it was a great time.

BUTLER: You were following up after that explorer had talked to you in fifth grade and said, "All right, here we go."

When you made the move, obviously you were going from Canada to the United States, big change in not just the nations, but also in the environment and the area. Was that any thought, or did you have any concerns about that move?

ERB: Well, you know, one always sort of hesitates to leave your homeland. You're going away from things that are comfortable. But both my wife's family and mine had had a lot of trans-border activity. My dad was born in New Hampshire. My wife's mother was born in Idaho. So the families had moved back and forth. I've never been a particularly political person, and I'm not sure but what parochial nationalism hasn't been one of the major problems in the world. So I was not desperately worried from the national point of view.

What was really driving was the job here, was an opportunity to do something absolutely unique and be involved right from the beginning. So it wasn't something I thought very long about.

BUTLER: Quite an opportunity.

ERB: Quite an opportunity.

BUTLER: When you did come down—no, actually, I'm sorry. Going back a step, the Arrow—there was a rollout of the Arrow on the same day that Sputnik was launched.

ERB: A memorable time. [Laughter]

BUTLER: Certainly memorable from your standpoint as an AVRO employee. Was Sputnik a factor, a discussion factor at all?

ERB: It was of great interest to me because, of course, I had been being interested in space in general, been following activities on a couple of fronts. About the same time, the X-15 was now flying, and [A.] Scott Crossfield was going to the edge of space at 50 kilometers [altitude]. In 1956 and '57, you may recall there was a worldwide scientific activity called the International Geophysical Year [IGY], and *National Geographic* described in one of their articles, I think in the fall of '56, how the United States was going to put a satellite into orbit. Of course, that was the Vanguard satellite, and we all know what happened to Vanguard. But at any rate, this was not widely known, I think, amongst the public at large. The notion of artificial satellites had just not penetrated the common psyche, but it was there. It was in publications like *Geographic*, and I was intrigued with this.

So Sputnik was not a surprise. In fact, I can remember—it's one of those things like the assassination of [John F.] Kennedy, you remember exactly what position you were in and where you were at the time you first heard it. I heard it on the evening news after I got home, after the Arrow rollout. I remember my reaction was, "Gee, we've done it." To me, this was a world accomplishment of engineering. The fact that it was Russian was more or less immaterial.

At any rate, the era of space travel was on its way. I think the impact of Sputnik was probably significant in the development of the Arrow project over the next couple of years in this regard, for a variety of reasons that I won't take too long to relate, and probably a historian would better go into than I. The U.S. Government at the time promoted, and the Canadian Government accepted, that manned aircraft were obsolete, and that we were in the era of ballistic missiles, guided missiles and space travel, and airplanes were no longer to be reckoned with, so therefore Canada should abandon the Arrow project and buy Bomarc missiles from Boeing, I guess it was.

It turned out that within a year or two, Canada was back in the airplane market, trying to find airplanes to buy, and indeed has been doing so for the last forty years. That's neither here nor there.

So the impact of Sputnik, I think, on the mind-set of people to sort of hint that, yes, here was something else coming along that was different from the traditional world of aircraft and fighters and interceptors and jet planes and so on, I think this was effectively part of the eventual demise of the Arrow. Wrong, but that was the way it was.

BUTLER: Certainly mind-set and the way people perceive things is a big factor on what happens in industry. Absolutely.

When you did come to the Space Task Group, how was the reception and the job environment? Was it what you had expected? Did you just jump into work and away you went?

ERB: I don't think any of us really knew quite what to expect. I certainly didn't. But the reception was wonderful. We were greeted warmly and it was an absolutely marvelous group of people to work with. Of course, everybody in the Space Task Group was tremendously goal oriented. We had one job in mind, and that was to get a man in space within two years. It was just a tremendous challenge, one of those things that you want to tackle because it's just (barely) possible. At least you hope it's barely possible.

I think the people in Space Task Group were very grateful to have an infusion of new talent and new help, and what was astonishing was how much responsibility we were given right off the bat. I mean, we were just sort of thrown into the job and it was a time at which everybody was just doing whatever needed to be done. There was no bureaucracy or hierarchy or any of the impediments that grew up later. Maybe it was a little too undisciplined, but at any rate, you were tackling the technical problems and they were formidable at the time, and you just sort of went and did what you had to do. Somehow the resources materialized to help out whatever you needed, and it was a wonderful and very exciting time.

So the reception was just great. I think there were fewer than—you'd have to check the date of it—I think there were fewer than 200 people in Space Task Group at the time we came down. It was also exciting in that about the same time the original Mercury astronauts were hired, so I think it was in the same month that we joined Space Task Group, the announcements were made of the hiring of the Mercury astronauts. So everything just seemed to be coming together in this crash project to get a man into space. A great time.

BUTLER: Building it all from scratch.

You became involved, and here your graduate work came in pretty handy, in the heat transfer and working with the Mercury heat shield. What were some of the first things when you came in? What had already been done on it? Then what were some of the problems you encountered? How did you make it happen?

ERB: Well, a lot had been done. Of course, the agency was formed in, I think, December of 1958 and absorbed the old NACA and the Redstone arsenal at Huntsville [Alabama] and some other elements from other agencies. There was really a very good team that was put together to start working on the man-in-space project. One of the giants to whom I look up was Bob [Robert R.] Gilruth, and the way he pulled things together back in that time frame was really amazing, and he had a wonderful team of people, largely from Langley, who had been doing really pioneering work in hypersonics. And another giant of the era is Max [Maxime A.] Faget, and bless his heart, he's still around. I hope you've interviewed him.

BUTLER: Yes, we have.

ERB: But when I joined the Space Task Group, there were sort of two approaches that were being considered for the heat shield. One was very straightforward, what's called a heat sink, basically a big blob of material, in this case beryllium. You just let it warm up and hope that there's enough heat capacity there to absorb the total heat pulse and not allow too much heat to soak through to the cabin and affect the astronaut.

The other approach was called the ablative heat shield. Ablation is a term involving cutting or sacrificing. It's a Latin surgical term. The ablative approach is one that depends on the material changing form, in this case pyrolizing, giving off gases. The gas sort of thickens the shock layer and allows the heat transfer rate to be reduced and build up a char, which is also helpful in insulation.

The ablative approach was relatively new. It had been used on ballistic missiles, but one of the major things that we faced was that the entry for Project Mercury was very different from the entry for a ballistic missile. If you think about the way these things work, you need to keep the accelerations low enough for a human to survive. So a manned entry—nowadays we'd say a human space flight entry—but a manned entry [required a shallow entry angle to maintain]…the low gravity levels.

That led to a long heat pulse. The heat built up over several minutes and then tapered off, whereas a ballistic missile just penetrates at a pretty steep angle, and the heat pulse is a matter of 60 to 90 seconds, very rapid. So the whole dynamics of the situation is different.

So one of the things that I was charged with doing was trying to figure out how the ablative heat shield would perform, what is it really going to do under this prolonged entry. That was something that I worked on for several months.

In the spring of '60, I think it was, there was still a real question as to how this was going to work, and so a flight test was developed called Big Joe. The Big Joe Project involved taking a boilerplate capsule, it was the right shape for a Mercury capsule, but it wasn't like the flight article. The idea was to put it on an Atlas booster, fly it up, and then point it back down and drive it down into the atmosphere as fast as you could get it going to get a good heat pulse. That we did in—I can't remember the actual flight date for Big Joe, but at any rate, late '60 or early '61, I think.

That was the first test of the ablative heat shield, and it performed just about like we thought it would, but it also gave me an opportunity to calibrate my mathematical model, because I had developed a computer program that would predict how thick the char layer would be, what the temperatures would be, and how much heat would leak through the back of the heat shield and into the cabin. So Big Joe was an important flight test in that regard.

It was also interesting in another regard. In the summer of '59, shortly after I joined Space Task Group, I was one of the new recruits, and I was about the youngest, incidentally, of the people that came down from Canada, so I was sort of low man on the totem pole. One of the jobs that I got was to go and do the night watch in the Unitary Tunnel over at Langley.

A colleague of mine, John Llewelyn, and I spent many hours on the night shift in the Unitary Tunnel doing wind tunnel tests of the Mercury capsule. I think we were operating in the Mach 4 or 5 range and looking at how the heating rates varied over the front of the capsule and along the afterbody.

One of the things that we noticed was that the heating rate was very high on the afterbody, in some cases almost as high as on the front face. This just didn't compute. It was against sort of the common wisdom and the theory. We discussed that with our superiors, and nobody wanted to sort of react to that. It was just sort of blown away as, "Well, you've got this capsule supported on this big stinger. It's probably an artifact of the test situation, so we won't worry about it."

What was interesting was that when Big Joe was flown, the actual afterbody buckled and was seriously damaged, and it was very clear that the heating rates were a lot higher than we had anticipated and designed for. So the result that we had noticed in the Unitary Tunnel was indeed vindicated. So we want back and analyzed it again and, in fact, redesigned the afterbody and at that point went to a different material and put on the cylindrical portion of the afterbody a quarter-inch-thick beryllium what were called shingles, to increase the [heat] capacity on the afterbody.

So Big Joe was a very critical test, one of the pivotal data points for both the afterbody heating and the heat shield itself. The heat shield performed so well that that basically set the decision to use the ablative shield for the Mercury capsule.

One of the problems with the heat sink was if you go through the entry heating, now you have this great blob of beryllium sitting on the bottom of the capsule, it's very hot. Supposing you land on the ground in an area of sagebrush or something. Are you going to start a fire? Can you drop the heat shield, get rid of it before you land? That adds mechanical complexity. So the beryllium heat shield had a lot of problems, although it was simple to analyze from a thermal point of view.

At any rate, the upshot of all that was the Big Joe tests showed that the ablative approach was going to work and we ended up going with that for the rest of the Mercury flights, and it performed well, no problem at all.

BUTLER: Luckily for all involved it did. It's good that so much was gained out of just one test, with everything going so right that you could gain that information.

ERB: Of course, now, the next real test to the heat shield was when you first come back from orbit. That we did in September of '61 with, I think it was called Mercury-Atlas 4. You may recall there were two orbital flights before Glenn's [John H. Glenn Jr.] flight. The Mercury-Atlas 4 was with a whatever they call it, a robot in some of the documentation, but it was really a box that produced the same sort of metabolic load that a human produces so that you could test the cabin cooling system. That went one orbit and reentered.

A few months later there was a two-orbit flight with a chimpanzee, and then in February, as we all know, Glenn's flight for three orbits. By this time—I'll get into it later, but by this time merely returning from orbit was beginning to be sort of not that big a challenge, and we would look at the results of each heat shield as it came back, and I would go down to the Cape after each flight and write up the post-flight report on the heat shield. But the performance was perfectly nominal. Once we had flown Mercury-Atlas 4, had done an orbital entry and the heat shield was performing the way we thought it would, I sort of lost interest in that class of entry problems.

BUTLER: At that point you moved into the AVT team with Owen Maynard, the Advanced Vehicles Team.

ERB: Right.

BUTLER: And you were working on Apollo-related work. In between that time, actually, when Alan [B.] Shepard [Jr.] launched, President [John F.] Kennedy had made the challenge to go to the moon by the end of the decade. When that challenge was made, what did you think of it? What did you think of the chances of making the deadline? I'm sure you were excited about the chance to work on the moon project.

ERB: Right. Well, to backtrack just a little, you're right, in the spring of 1960, NASA was beginning to be concerned about how do we manage the Mercury flights. They knew that we needed to maintain routine, pretty much as constant as possible communication with the capsule as it went around, so the then called worldwide range was set up with stations at various points around the world.

A call went out sometime in very early 1960 for people who would like to become flight controllers. This was sort of the genesis of what we now have for missions operations. Anyway, that sounded sort of interesting, and I entertained fantasies of going off to Zanzibar [Tanzania, Africa] and other romantic places and being a flight controller, so I threw my hat in the ring and decided that would be fun. It turns out, I'm really not an operations type. I've learned that over the years. But it sounded good at the time, and I was accepted as one of the flight controller trainees, and went through, I think one or two classes as to what this was all going to involve, when they came along with the notion, well, we'd better begin thinking about what comes after Mercury, and formed this Advanced Vehicle Team that you mentioned.

I think, as a matter of fact, that people like Faget and Gilruth had probably been thinking about that for a long time, but by the spring of 1960 things were going reasonably well in Mercury, and they could now start to put a little bit of effort toward that. So they assigned, I think it was eight of us, including Owen [E.] Maynard and Al [Alan B.] Kehlet and Bob [Robert G.] Chilton, I think three or four others, and myself, and Bob [Robert O.] Piland was the leader of that team. I was the thermal person.

We started, as you suggested, thinking about, okay, what do we do after Mercury? This was going to become the foundation for Apollo. This sort of challenge, because it was very clear we were going to have to do something with going to the moon, and this sort of challenge made Mercury sort of pale into insignificance, and that had been challenging enough in the summer of 1959, but all of a sudden we were talking about a whole different regime to operate in.

I was concerned, of course, about the heating aspects of return from the moon. As you're, I'm sure, aware, when you come back from Earth orbit, you're coming at a speed of about 25,000 feet per second. When you come back from the moon, you're coming at 36,000 feet per second. Now, that doesn't sound like a big difference, but it makes a huge difference.

There are two kinds of heating that you need to consider—convective heating, which is just heat transferred from a hot shock layer by conduct[ion] through the atmosphere to the vehicle, and that sort of varies as the square of the velocity. So as you put the velocity up, the amount of [convective] heating goes up as the square of the velocity. What is even worse, though, if you come back into the atmosphere at a high enough speed, you build up a very hot shock layer, and the temperature of the air in the shock layer is so high that you can radiate a substantial amount of heat right directly to the heat shield, and this radiative heating was not something that we really had to contend with for orbital returns. It just isn't a parameter. It's not hot enough to cause a problem. But coming in at lunar return speed, it's quite a different situation.

So getting a grip on this radiative heating was one of the major challenges that we struggled with through 1960 and '61, and we had a little bit of data from shock tunnels. We had a little bit of data from the kind of tests where you fire a small pellet down an instrumented range and see how the shock wave builds up. But skimpy data. And it was really exciting when we managed to get all the data that existed on to one sheet of what we call three-cycle-log log paper. The scatter in the data was terrible. Over the course of another year it began to get better, but we were doing a lot of pioneering in the sense of really having to try to bring some judgment to play as to how serious this was going to be and what we should try to design for.

So during '60 and '61, while Shepard's flights were going on, we were looking hard at the design for the reentry vehicle, the other aspects of the lunar mission, and, for me, the radiative heating and how you protect against it.

I think I'm one of these people who's pretty optimistic. I think if you're going to be in the engineering game, you have to sort of say, "Well, sure, we'll find a way." I was absolutely convinced, at least on an article of faith that I'd held since grade five, that man would go to the moon, so, you know, it was just a question of how, how and when. It wasn't a question of would we. So I was quite convinced that it was possible, and whether or not you can do it in the time available and with the resources you have, that's another matter.

But at any rate, I think the technical team down at the level I was working at was convinced this was a doable thing to tackle. I understand from much later conversation that when Kennedy said this was what he wanted to do, he wanted to make this challenge, that Bob Gilruth allowed that, "Well, yeah, probably got a chance of doing it." Now, Gilruth was very absolutely solid and a somewhat conservative engineer, and I expect he, in his heart of hearts, was confident we could do it, but he didn't want to over-promise. At any rate, that was what the political system and our political masters wanted to do, and I think in a way seeing this come out as a national commitment, stated at the highest levels, and enunciated so clearly and with such very crisp and defined goal, those of us on the team were just absolutely thrilled that, "Okay, now we've got the charter. Let's go get it." It was great.

BUTLER: Certainly an exciting—I don't think you'd have many that would say, "Oh, no, I don't want to do that."

ERB: We just didn't have that kind of people in the program. There were people that were cautious, of course, and wanted to make sure that we did things in a sensible way, but we were very success oriented. The question was, how do you do it and what do you have to do to get there.

BUTLER: How do you make it happen?

ERB: How do you make it happen?

BUTLER: You mentioned the radiative heating and how the reentry from the moon was so different than reentry from Earth orbit. Looking at how you find this information in the first place, because you haven't—you know the Earth orbit because you've sent vessels up there, but at the time, had you had anything return from the moon to know any—

ERB: No, there were no entry tests of anything like lunar return speed until much, much later, so during the '63, '64, '65 period, we were doing the detailed analysis on the thermal protection system, figuring out what the heating rates were, looking at different kinds of entries, and trying to bracket how bad it could be.

I might mention that for Apollo there were a couple of extreme cases, and we had to sort of design the heat shield for both of those cases. If you consider yourself at the moon and you're looking back at the Earth, you have to come in and hit at just the right angle. There's a narrow range of angles at which you can enter the atmosphere, called an entry corridor. If you are too low, you will dig into the atmosphere too deeply. The accelerations will be too high, and your crew and your vehicle won't survive. In fact, it looks like that's exactly what happened to the Mars climate vehicle that was lost a few weeks ago. If you hit at too shallow an angle, you will skip and take a homeless exit toward Venus, and that will be the end of it. So you've got to hit just right in that right corridor.

If you hit at the bottom edge of the corridor, you get a very high heating rate, but a short pulse. If you hit at the top of the corridor, you sort of drag in over a 5,000-mile entry and you have a long heat pulse, but a lower peak heating. We basically had to design the heat shield to accommodate this range of conditions.

Now, as it turned out, our colleagues in the guidance world always managed to hit right dead on in the center of the corridor and everything was very nominal. So we never really exercised the heat shield for any of the [extreme] design conditions. I can remember on some occasions being given a lot of grief for the fact that my heat shield was two and a quarter inches thick at the leading edge and it only charred about two-tenths of an inch, and why had I overdesigned it so grossly? I would have to go through this argument that we had to design it for conditions that we never experienced.

But the testing of the Apollo heat shield was one of the more interesting things that I've wrestled with for a number of years, because while you have facilities on the Earth—arc jets and other test facilities—you can never simultaneously get all the right conditions. You can test to the…[right] pressure, you can test to the right temperature, you can test at the right heating rate, but you can't do these simultaneously.

So one of the major things that I look back on as really a unique part of my activity was to build the mathematical model that allowed test data from various facilities and places to be sort of put together analytically so that you could make a projection that, yes, this is really going to work and it's going to work the way you say it is, but you can never do a full-scale test. I think that's probably unique to the heat protection system on Apollo. It was the only critical system that you couldn't subject to the real final entry conditions on the ground or any way until you actually flew it. So it was a good challenge.

We did fly in—I want to say about '66, on a Saturn 1B, a test that was designed, almost like the old Big Joe test, to fly up and drive it back down to get higher and higher speeds and test out the heat shield. The only problem was, we just didn't have enough capability to build the velocity up, so we were able to get a little higher than orbital speed, about 28,000 feet per second, but still not enough to give any appreciable amount of radiative heating.

So we were really relying on our math models, and I felt pretty confident that we were going to do all right. As a matter of fact, my then supervisor, in Structures and the Mechanics Division, an absolutely delightful fellow named Joe [Joseph N.] Kotanchik—and I'm sure you've heard that name from time to time.

BUTLER: Yes.

ERB: Bless his departed soul. Joe had a way of framing questions to really focus your thinking. He said to me one time, said, "Bryan, when we come back from the moon for the first time, and the crew has come out of lunar orbit, and you know that in three days your heat shield is going to be tested at full lunar return speed, are you going to sleep well those nights?"

I thought a minute and I said, "Yes." And I did.

So for me, of course, the Apollo 8 was the real test case. That was the first time we really brought in the command module at full lunar return speed.

BUTLER: Crew aboard and all.

ERB: Crew and all, and everything worked just as we had projected, so after that I was—well, I was pretty confident before, but after that, you know, it's okay. We know we can do it, the heat shield is going to perform perfectly well. So that was the real acid test for the Apollo heat shield.

BUTLER: It's great that you were able to have that confidence going into it, that you knew that it would work, that you had been able to do enough planning and modeling and testing.

ERB: Of course, there was a tremendous amount of work went into this, as you can imagine, not only at NASA, but our prime contractor, North American, the heat shield contractor, which was AVCO [Corporation] in Everett, Massachusetts. We did wind tunnel tests all over the place, whatever facility would help give us additional data.

There were some hairy moments. One that I recall was that Langley had been running some tests on their own, and they had tested the Apollo heat shield material and found it, in their view, wanting. They said it was going to deteriorate much too rapidly. A test to fly a small sample of the material about a foot in diameter, [and the shape was a]…replica of the…[front face of the command module], was devised to launch on a Scout vehicle.

I don't know if you're familiar with the Scout, but it's a three-stage rocket that was launched in those days from Wallops Island. The idea again, drive this up with a couple of stages and back down with one to get a reasonable heating pulse. This test was of great concern to the Langley people and they said, "The material is going to fail. It's not going to survive this test."

And just as the launch was ready to go, they raised the flag again and the decision came down to Max Faget. Max tracked me down. I was in a meeting somewhere and he had me hauled out of the meeting and talked to me on the phone. He said, "Bryan, should we fly the Scout mission? Langley says it's going to fail."

I said, "No, they're wrong. They did their testing at the wrong pressure. When you fly the actual mission, it's going to work just fine."

And Max, bless his heart, said, "Okay, I believe you," and he backed me up and said, "Fly it," and it did, and everything was fine. So it sort of showed to me again that you've just got to be very careful not to latch on to one data point in one facility, because you can't replicate [the conditions], so you've just got to be confident in your model and go with the model that best fits all the data. So that was a further vindication.

BUTLER: That's good that Max Faget obviously knew that you were a specialist in your job, you knew what you were doing, and you had the facts to go with it.

ERB: I think this was pretty characteristic of Space Task Group. We had an awfully dedicated group of people, but the management was absolutely solid, and they were good engineers in their own right. They knew when you were blowing smoke and when you had something that was substantive. If they felt you were on solid ground, they would back you all the way. It was a great environment to work in.

BUTLER: Talking of the management, and we've talked about Max Faget a little bit, luckily we have had the chance to talk with him on this project, you also mentioned Bob Gilruth a couple of times. Unfortunately, we can't talk with him. Did you have a chance to work very closely with him or was it more where he was just in charge of the whole program?

ERB: More the latter. He was, of course, the director of Project Mercury when I joined the program. He was one of the people who came up to Toronto to interview us when we joined the Space Task Group. I was, of course, in meetings from time to time with Gilruth, and I was always impressed at his balance and his way of trading risk and opportunity. He had such a wealth of experience going back, as I'm sure you're aware, to the early days of stratospheric ballooning with [Jean] Piccard. This sort of historical connection was always intriguing to me, that we had this sort of continuity from stratospheric ballooning in the thirties to space flight in the fifties.

But I never really did have the opportunity to work with him in a technical basis, but I think he was, let me say, really the intellectual driver of the Space Task Group in the sense of what it should be doing. He was the grand strategist. I have just great regard for Gilruth.

BUTLER: It seems like he had a lot of very good things that he was able to contribute to the program.

ERB: And I think he never got, in my view, the sort of public acknowledgement that he really deserved. He was a very quiet and unassuming person. He never put himself forward. So if you sort of compare—and I don't mean to detract from Wernher von Braun, but von Braun was sort of the public figure of the age, and people sort of thought he was NASA. NASA Headquarters in Washington, the administrator of NASA, people like Gilruth, none of those had the press or the publicity that von Braun did, and von Braun was sort of the spokesman. He was a very articulate and powerful spokesman. Like I say, I don't want to in any way diminish his contribution, but I think Gilruth was quite a peer, at least of von Braun's, but almost invisible to the American public, and I think that's a shame.

BUTLER: It is. It is. Well, hopefully we can, through projects like this, we can help.

ERB: I think as you read the histories that have been written so far, like [*Apollo:*] *Race to the Moon* [by Charles Murray and Catherine Bly Cox] and the book by [Andrew] Chaikin [*A Man on the Moon: The Voyages of the Apollo Astronauts*], you know, the Gilruth role and the role of the engineers comes through, and that's good to see.

BUTLER: Very good to see. You've talked a couple of times about developing the model for testing the Apollo heat shield and evaluating and combining the various analyses. How much did you work with computers at that time?

ERB: That's a good question. I got thinking about that last night. My first exposure to computers was before I left AVRO. At that time, '57, '58, we had just started into the use of digital computers and had an IBM 704, which was sort of the big machine of the time. Hence, it's amazing to think that a major aerospace company or aircraft company like AVRO and, indeed, Space Task Group ran their whole programs on an IBM 704. My laptop has vastly more power now than the 704 had in those days.

I was working very little with it at AVRO. I really got into computers after I joined Space Task Group. My first exposure—and I'm really dating myself on this, but I guess that's the name of the game—was with a computer that was put out by Bendix [Corporation]. It was called the Bendix G15. It was a big gray box about twice the size of a refrigerator. The data transfer medium was a punched paper tape. You've probably never seen a punched paper tape for a computer, but that was the way the bits were put into the machine. You programmed it in what would be called assembly level language. That is to say you would get this number from this buffer and you put it here, and you get this number from this buffer and you put it there, and you add the two of them together, so the instruction set was gruesomely complicated. But I worked with that for quite a number of months.

Then took a course in FORTRAN programming for use on the IBM and wrote most of my ablation performance programs and that sort of thing, in FORTRAN, and would come up with the projections as to what the heat shield was going to do under certain circumstances. So '59, '60 was my first introduction to computers, and it was another exciting thing to learn.

BUTLER: Absolutely. I'm sure at the time you would never have imagined how computers would grow so rapidly and change so quickly.

ERB: That's true. It was, of course, many years before we really got into the personal computer era, but my wife, who [was] a computer systems analyst [engineer] for the Mitre Corporation, of course, I think had a much better vision of how these things were going to develop. When the very first personal computers began to come along in the late seventies, we got into that quite early on. So we have been heavily into that for quite a long while now.

BUTLER: It's always interesting to look at how in the early days of the space program you were working with these large computers that did only have, in comparison to today, a limited ability, and slide rules also, and just paper, but we're going to the moon.

ERB: Absolutely. Yes. When one thinks about some of these things and thinks what will history say about what these people did, I look at the Mercury capsule from time to time and say people 100 years from now will wonder what sort of barbarians were we, that we would put a human in that vehicle and send them off on a rocket into space. But, you know, that was what we could do, and it had worked well and we learned from it a great deal.

BUTLER: As you were working throughout the program, Mercury and Apollo, were you aware, or even NASA as a whole Space Task Group, of what was going on with the Russian program? Did that have a lot of impact on you, or any at all?

ERB: I'm sure the senior management were very well plugged into what the Russians were doing to the extent that the intelligence community had information on this. At my level I saw only what I read in the papers, for the most part. Of course, we followed what the Russians did publicly in flights like [Yuri] Gagarin's, which was, incidentally, on my thirtieth birthday, so it's one of those days I remember.

We were generally well impressed with the Russian program. It was clear they had a solid program, no doubt about it. They put a lot of emphasis on it and they flew well and flew safely and had a good program.

There was a considerable anxiety over whether or not we would really beat them to the moon. In the '67 time frame, and I remember one of my Headquarters colleagues at the time, Omar Salmassi, who later went to work with TRW, he was saying that they were convinced that the Russians were going to try a moon shot in '67, on the fiftieth anniversary of the Bolshevik Revolution. That was the sort of notion that had a certain ring of truth to it. It would be the kind of thing that you'd like to do if you were in their shoes. As we learned later, they never even got close, but it was a goad to our activity to sort of keep us stimulated, that this really is a race. We've got a very competent adversary out there who's working hard and has resources, too, and we want to make sure we do it.

To digress a second, though, I think the challenges that we faced on Mercury in a way laid the groundwork that made Apollo successful and worked against the Russians succeeding in this sense. Because of the miniaturization of nuclear warheads, the United States was able to build an intercontinental ballistic missile with a much smaller throw weight than the Russians. And for ICBM purposes, this is fine. You don't want your missile to be any bigger than necessary. The Atlas, as you may recall, had a capacity of about a ton. Turned out it was increased for Mercury purposes to about a ton and a half.

But at any rate, that was the target that we had to work with, and we had nominally a ton to a ton and a half in which to build a man-carrying space vehicle. The Russians had substantially more throw weight, three or four times as much, and so they did not have to push the technology as hard to miniaturize it, to make it compact, in order to do the orbital flights. The data point that I recall is that the whole environmental control system for Mercury had to weigh in at under 80 pounds, which is not very much.

So that when we came to doing Apollo a few years later, we had pushed our technology to the point where we could do some of these things in a more compact basis and within the confines of a single Saturn V, do a lunar orbit rendezvous trip to the moon. The Russians had never achieved that degree of compactness, so their lunar vehicle was absolutely enormous, zillions of engines, multiple stages, and many, many problems, and they never succeeded.

So I think from the standpoint of having enough challenge to really push you, not so much that you get overwhelmed—I guess sort of as a historian you would appreciate [Arnold] Toynbee's point of view. You want just the right challenge. You don't want so much that you drive civilization under, but you want enough to stimulate it to achieve great things. The limits on the ballistic missile capacity stimulated the Mercury design and reverberated all through the later designs.

BUTLER: Very interesting. [Brief interruption.]

During the time that the program was going on, Mercury had been going and everything was based in Virginia, then as Apollo began to get up to speed, it was realized that there were a lot more considerations that were going to have to be taken under. The decision was made to move the center down to Houston [Texas]. What did you think of that decision and that move at the time?

ERB: We were so interested in the job that it really didn't matter. It was interesting, though, to go back just a little further, when I signed on at Langley the very first day, in May of '59, I was told, "You'll be here for two years. After that, you're going to Beltsville, Maryland, because we, Space Task Group, are part of the [Goddard] Space Center." Bob Gilruth, aside from being director of Project Mercury, was an assistant director for human space flight, or manned space flight, I guess, at the time, of the Goddard Space Flight Center. So the only practical difference that made was that when we did get a little bit of time off, we explored to the south, thinking that we would be going to the Washington [D.C.] area in two years and living there.

As it turned out, the manned space flight program was much too big to be contained within one arm of Goddard, and I think this realization began to sink in to people in the 1960, early '61 time frame. By about middle of '61, the announcement came that we were going to become an autonomous center and become the Manned Spacecraft Center, and that we would move. It wasn't stated at the time, of course, where we would move.

Then they went through this notion of a site selection activity, and people trooped around the country looking for appropriate places. I think if anybody had been plugged into the political side of the scene—and I was very apolitical at the time—you would have very quickly said that Lyndon [B.] Johnson, as head of the Space Council and Vice President, he comes from Texas, we will end up in Texas. It should have been axiomatic, but it wasn't.

And they went through this site selection charade and looked at places that I think boiled down to providing photo opportunities for local congressmen, but there was no serious intent, I don't think, of going anywhere else than Houston.

So at any rate, it really didn't matter all that much to me. I enjoyed Virginia, a nice area, but once you've sort of left home, it doesn't make all that much difference, you know, where you're going to go. If the job is interesting, your family's with you, fine. You do what is required. So we didn't have any qualms about moving to Texas, even though we'd just built a house in Virginia not long before. This decision on moving dragged on and on, and finally we sort of said, "Well, we've got to get out of rented accommodation and get something more attuned to our needs," so we ended up with a house in Virginia for four years that we didn't need and had to rent, and eventually sold.

But the move itself was just another step along the way, and quite exciting. We've enjoyed Texas and found it very congenial, have been very happy here for going on forty years now.

BUTLER: That's great. Great that it worked out. As you said, you had a very good motivating factor and quite an interesting job.

ERB: Indeed. So I sort of feel I've been making a giant circle of the continent, from Ontario, to Langley, to Houston.

BUTLER: That's right.

ERB: Stopped for forty years.

BUTLER: Sounds like you—well, you may still make it out to the West Coast. You still have plenty of time.

As you were working with Mercury, then you worked into the Advanced Vehicles Group, and then Apollo, did you have any connection at all with Gemini?

ERB: Not really. Gemini was inserted into the program for reasons, I'll say, other than engineering, and I think, looking at it in retrospect, it was a brilliant move. In fact, I think one of our Canadian colleagues, Jim [James A.] Chamberlin, was quite instrumental in promoting the Gemini Project, and I think he and, I'm sure, others realized that there needed to be a bridge between a single person in a very limited spacecraft for a period of a day, and three people in a very complicated spacecraft for a lunar mission for two weeks.

Somehow you needed to develop a lot of operational understanding. You had to learn to do rendezvous and docking. You had to operate multiple spacecraft, as we did with the command module and the lunar module, and that you needed a vehicle to develop this. I think this was the genesis of the Gemini Program. From an engineering point of view, it was just sort of a slightly scaled-up overgrown Mercury, no particular engineering challenges. I think I made one trip to St. Louis [Missouri] to look at the Gemini shield, which was pretty standard state of the art. So I really had virtually nothing to do with it, but it was a good, solid program and an important part of the preparation for Apollo, giving the flight experience and the operational development opportunities.

BUTLER: A good learning opportunity.

ERB: Very important part of the learning process.

BUTLER: While you were working with Apollo, you had a couple of different positions in the thermal analysis section in the thermo-structures branch. Is there any specific responsibility beyond what you've told us about, work on the Apollo heat shield, anything that was unique to any of those positions?

ERB: Well, a part that I haven't talked much about is the thermal balance side of things, which was another interesting challenge. If you consider a spacecraft in deep space, the side facing the sun is going to be exposed to full solar illumination. The side facing deep space is going to just get colder and colder and colder as the heat radiates away. So how do you maintain a balance on the spacecraft?

So part of my responsibilities during that period was to look at the thermal balance of the whole Apollo spacecraft system as it went to the moon and back. Now you get into a totally different aspect of thermal analysis, then the reentry heating. You have to think about how does heat get generated or stored or radiated to or from a vehicle, and this is sort of another very interesting analytical area. Especially when you think about some of these spacecraft that we were dealing with, the lunar module being the best example, you have all these various surfaces at different angles. As the spacecraft moves, different parts are shadowed and different parts are illuminated. You get radiative transfer back and forth from one part to another.

It was really an interesting analytical challenge to model the Apollo command service module and the lunar module during the trip out to the moon. The key, in fact, to making it work was you had to say now we have a time-dependent vehicle. You don't want things to gradually heat up or you could get into a damaging situation, so you design the system to use your fuel as a heat reservoir and let it cool down very slowly. You like it to either maintain or cool slightly. As long as you've got this big mass of fuel, you've got a good heat reservoir.

But there were some things that we just couldn't do, and the command module, for instance, would get to a temperature of 250 on one side and minus 250 on the other side. So I came up with the notion that the way to solve this problem is to use what I call the barbecue mode. You orient the long axis of the spacecraft perpendicular to the direction to the sun, and you just roll it slowly. This allows you to equilibrate the heat and keep the spacecraft at a nice cozy temperature.

I can recall when we first proposed this, the crew were not very happy. Astronauts tend to sort of blame the engineers, you know. "You're forcing us to do something operationally because you didn't do the right design." But you can't fight the laws of physics. If you've got the sun on one side and deep space on the other side, you'd better find a way of equilibrating the heat flux. So that was sort of a totally different aspect of the thermal balance, and it was an interesting activity and, again, highly dependent on analytical models.

Now, there we could do a little better job of testing it on the ground before we flew than we could with the heat shield, and I'm sure you're familiar with the large vacuum chamber over on site, the chamber A in Building 32. That was part of the facilities in the division that I was with, Structures and Mechanics. The large vacuum chamber was designed to take the entire Apollo Command Service Module and Lunar Module stack, put it on a turntable, replicate the solar illumination via a large bank of carbon arc lamps, pump it down close to the vacuum of space, and then just slowly rotate the spacecraft, just like we planned to do in space, and do all this with the crew inside.

So we were able to go through a two-week simulation of the lunar mission with the entire Apollo Command Service Module stack under really a very good simulation of the right conditions, and that allowed us to tune our models and adjust the parameters so that by the time we did the mission we were very confident that we were not going to have thermal problems on the mission. So that was a fun time, too.

BUTLER: Certainly some very interesting problems and things to take into account that you have to catch all of the unique little areas that might be to make it all successful.

ERB: And even though you think you've thought about them all, there's always something that will come up and surprise you.

BUTLER: Always something.

You mentioned earlier Apollo 8 and how that was a very large test for the heat shield and that you were very confident of that. When they actually did get to the moon, even though it wasn't specifically—of course, everything you had done contributed to that, but do you remember where you were and watching it, what you were thinking when it landed?

ERB: Yes, very clearly. After Apollo 8, I had just come back from a year at the Sloan School, and I was really looking for sort of a new challenge. I knew I was going to stay with Structures and Mechanics through the Apollo 8 mission and work that mission from the backup room, but I was looking for something new, and what came up was an opportunity to join the Lunar Receiving Laboratory [LRL], which was a wild activity in anybody's career.

My first exposure to this was in the fall of '68 when the laboratory was going through its readiness review, and I was designated from my division to represent the division on the Readiness Review Board. So I went over and made all the usual criticisms of what needed to be done, and as it turned out, a few weeks later I joined the staff of the laboratory, and I had to turn around and cure all these problems that I had identified.

But we scrambled to get the lab ready for the receipt of the lunar rocks, and for the landing itself, we decided we would have a touchdown party at my house and invite some of our colleagues. I splurged and bought a color television, the first color television we'd ever had. Of course, I was interested in, from the lab point of view, getting the rocks back. I can remember after the guys got out on the surface, I said to nobody in particular—and they were sort of having a great time cavorting around and jumping and leaping and just playing—and I said to somebody, "Quit jumping around and pick up some damn rocks!" [Laughter]

Indeed, the rocks came back and we had a successful series of tests in the laboratory. That whole activity, though, was a very interesting one. Around the 1967 time frame, NASA was basically pooh-poohing the notion of having to do anything to protect against potential problems from the return of lunar material, but there are mechanisms that could conceivably exist that would cause pathogenic or toxic material to exist on the moon. The Department of Health and the Department of Interior and the Department of Agriculture collectively are charged with the responsibility for protecting life, limb, property, brook trout, and crops in the United States, said, "You're not going to bring back material from another planet without taking proper precautions."

So they forced NASA to form what was called an Interagency Committee on Back Contamination, contamination back from another planet. NASA sort of resisted this for a while, but eventually decided that we'd better do something. So the response to all that was the Lunar Receiving Laboratory.

So we had to do two things. We had to quarantine the crew and anybody who came into contact with them for some period of time, and—and this was the really challenging part—we had to expose representative Earth life forms to the lunar material, to see whether there was any adverse effect. Just the question, what is a representative sample of Earth life forms, you know, that in itself—how many species are there on Earth? How do you select ones that collectively form a representative sample? We ended up with somewhat over 100.

The whole mammalian species is represented by the mouse, so then we had pine trees and oysters, and, you know, you name it, there were an amazing array of life forms that we had to acquire and allow to grow and develop and maintain a colony under conditions of containment so that all the atmosphere and all the wastes and all the effluents and so on from the inside of the laboratory within the so-called biological barrier was all contained and couldn’t leak to the outside.

This was really a sort of neat engineering challenge. There was a lot of basic background work that had been done by the military in dealing with so-called germ warfare, so some of the techniques that were developed, notably at Fort Detrick in Maryland, where the Army had their main biowarfare facility, a lot of those techniques, how you work within cabinets, keep the cabinet a slightly lower pressure than the surrounding rooms so that any leakage of air would be inside, and you pass that air through filters, and any material that comes out is burned, whatever, your variety of means for moving stuff in and out.

But this was a very neat challenge, from an engineering point of view, to try to get this laboratory built and developed, get all these life forms growing and be ready to accommodate the return of the crew and the lunar samples. But it was a very intense time during the first half of 1969. I joined the lab as a deputy manager in, I think, January of '69, and I don't think I ever worked quite so hard in my life as that next six months, getting ready for the lunar landing.

It was almost funny, I got to thinking last night, back in about '67, NASA had gone through one of these routine budget exercises, and everybody in the center had been asked to suggest ways in which we could save money, and knowing nothing about it, I suggested, well, one of the ways would be to defer the activation of the Lunar Receiving Laboratory. We're not going to have to worry about samples for two years. Just put it off a while. [Laughter] Fortunately, my recommendation had not been taken, but I was the one that was scrambling at the end to get the lab ready.

BUTLER: That's interesting.

ERB: On a personal note, that was the first of several assignments where I seemed to be put into a situation where I was the interpreter and the buffer between a manager who was clearly brilliant and useful, but had certain problems in other areas. Our lab manager at that time was an irascible scientist named Persa [R.] Bell, P. R. Bell. It's a whole story in itself, but at any rate, Bell had come from Oak Ridge and he had made a name for himself in learning to handle powdered material under vacuum conditions, and that was what we thought we were going to be doing in the Lunar Receiving Laboratory. But Bell, bless his heart, was almost blind. He had damaged his eyesight looking into reactors in the early days. He had glasses about half an inch thick and a little magnifying glass up on the top, one lens that he swung down.

The result of this was that he could read, sort of this motion [Erb demonstrates paper close to eye, moving head back and forth], but he was totally unaware of people's reactions to him. He lived in a one-way world. His people skills, let us say, were just not the greatest. So I was the interpreter and the buffer between P. R. and the rest of the world, so that was a good learning experience, too.

The lab worked. It worked well. We had some problems with the breaches in the biological barrier, and people would get contaminated in the labs, and then we'd dump them into quarantine as well. We had projected that this sort of thing could happen and that we would have lab technicians exposed and have to add them to the crew that was being quarantined. I believe it was a reasonably rigorous quarantine and an effective demonstration that there were no effects of the lunar material that showed up quickly.

You never know whether something might show up in thirty years. There are viruses and things that will show up long after the fact, but the theory was that if you can go through a quarantine for three weeks, which was the time set, without adverse effect, then you're obviously not dealing with something that is rapidly reacting and dangerous, so you would have time to prepare a remedial action. It was a good trade, I think, between a hazard, which was not very likely, but a risk of perhaps life on Earth, which was immense. So it was one of those fundamentally indeterminate things, and you just had to make a judgment call.

BUTLER: Yes, that's certainly something that we wouldn't want to just assume that everything would be okay and then, "Oops, sorry. We didn't mean to contaminate."

ERB: You know, you fantasize about some of these scenarios, too. I thought supposing we do find something really deadly. What is the action? And it went through our minds that, well, you might, in fact, have to sacrifice everybody in the laboratory and bulldoze it under 100 feet of dirt. This sort of thing goes through your mind, if you really did have something that was seriously pathogenic. But fortunately, at least for the areas we visited, there was never anything of any serious hazard whatsoever.

BUTLER: At least so far. We've hit the thirty-year mark.

ERB: It'll be interesting to go through this again as we tackle Mars samples return, because in three or four years we'll be coming back with samples from Mars, and we'll have to think through all the same decisions, but now with, I think, a much greater likelihood of life forms from Mars. So the quarantine issue has already been discussed again.

BUTLER: Should be interesting to see what comes of that. We'll go ahead and pause here briefly to change out the tape.

ERB: Okay. [Recorder turned off.]

BUTLER: We were talking about your work at the Lunar Receiving Laboratory and the challenges that you had to work with, some that you had found yourself in the job to make it all work and to deal with the back contamination issue. Did you also have many considerations for the scientific issues, like what would be done with the rocks after they were brought back? Or was that a later consideration?

ERB: No, that was very much a simultaneous consideration, because one of the things that we were trying to do was develop a process for doing the preliminary examination of the rocks in a timely enough way so that we could get one mission out of the way before we would get another mission in on top of it. So in the judgment of the senior management of the center and some of us in the laboratory, that required, let me say, some rigor and some procedures and a fairly good step-by-step process to be adopted.

As it turned out, this was a point of major conflict, because the scientific mind-set was not much given to adhering to procedures. They would say, "Well, what do you mean, procedures? I'd just put my hands in the gloves in the compartment and I'd do my geological thing, or whatever my thing is." But we knew we had a lot of aspects of the work that we really needed to have somewhat disciplined so the rocks would be handled, cut, chipped, numbered, whatever needed to be done in a structured way.

So this caused a lot of heated argument between the—and it boiled down to engineers versus scientists. The engineers wanted things structured and rigid and proceduralized, and the scientists wanted to just sort of free float. It caused a particular lot of conflict between P. R. Bell and myself.

We eventually, I think, came to a good accommodation, though, and we ended up with procedures that allowed us to track things properly, catalog things properly, but I don't believe in any way disrupt the scientific integrity of what needed to be done by the investigators.

Then another side of that, of course, was the curatorial side. How do you decide which pieces of what rocks go to what investigators for detailed analysis? Because, of course, the laboratory was just doing, as I've mentioned, the two functions of quarantining the crew and making sure that the lunar material was not going to be harmful to life on earth, but it was also doing this preliminary examination. The goal of the preliminary examination was basically to get a first look at what the scientific return was from the mission and provide appropriate samples to specialist investigators who would then get these samples in their labs and do extended investigations over long periods of time.

So, dividing up the samples, getting the right bits and pieces of rock to the right investigators, that was another major part of the lab's function. That was another point of—what shall I say—considerable conflict. Everybody thought they should have the biggest piece of the best rock, and the curatorial mind-set—and it's a good thing—the curatorial mind-set is, give away as little as possible so that you have some left for later investigators years or decades hence. Eventually, though, you strike a balance, and it was a good experience.

We ended up with, I guess, overall thousands of investigators all over the world examining the rocks in various detail, and we've had a Lunar Science Conference pretty much every year since 1969, and new material is coming out, new understandings year after year after year.

BUTLER: Absolutely. There's a lot still to learn, I'm sure.

ERB: And there will be a lot more as we visit new sites on the moon, because the Apollo sampling was relatively limited. We visited six sites, and all were near the Equator. So you imagine how much are you going to find out about the Earth by sampling in six places near the Equator, and there would be a lot you wouldn't know.

BUTLER: Quite a lot. Though the moon's not as big as the Earth, it's still a very big place and a lot of different things going on. Hopefully it won't be too much longer before we get some of those other samples.

ERB: I'd like to see us get back to the moon.

BUTLER: Absolutely. When the first samples came back from Apollo 11, you mentioned watching while they were bouncing around on the moon and you wanting them to pick up some rocks. When they came back, were you there when the boxes were first opened?

ERB: Yes. Another function that the lab performed, by the way, was to prepare the sample return boxes, which we very carefully cleaned and prepared, the metal boxes and the special bags and the tools, so that when the crew acquired the samples on the lunar surface, they were not taking forward contamination out to the moon, because we wanted to be very sure, of course, that the samples, when we did get the box back and opened it up, that they were pristine and not affected in any way with contaminants from the Earth.

So, yes, it was an exciting time to open the box the first time. The rocks were all dusty, though. One of the lunar investigators described the process that involved the transport as sort of a shake-and-bake. You had a certain amount of dust and the rocks themselves, and in the trip back and splashing back into the ocean and all the disturbances of the transport, you ended up with dust all over everything. So, of course, one of the first problems was to sort of clean the rocks and get a look at the real rocks themselves.

But not being a geologist, of course, I never really particularly appreciated, I'm sure, the nuances of the different kind of rocks, and yet I did many years ago take one geology course and I tried to learn a little more about it during the time I was in the lab, at least enough to be able to talk to lab visitors and sort of say what are the rocks like, what are their analogs on Earth, and that sort of thing. So I didn't have a great deal to do specifically with the science other than to try, again, to be sort of an interpreter to the lay public.

BUTLER: As you were working with the Lunar Receiving Lab and the rocks and so forth, did you think back to that explorer that had come to your classroom and said some day people would be going to the moon?

ERB: Interesting question. I can't say that I did. I probably did at the time.

BUTLER: Exciting enough that it was all happening.

ERB: It was all happening.

BUTLER: You've gone through quite a change from working with the heat shield and the thermal considerations, and you've mentioned that you had gone to the Sloan School and had a brief hiatus there. Then you came back and wanted to get into something different, you said. What drove your interest in going to a new area?

ERB: I think I have always liked to tackle new challenges. I guess maybe I have a short attention span or something, I don't know. But I've been fortunate through my career to have new opportunities every three, four, five years to do something different.

When I went to the Sloan School, I was very much interested in management processes and sort of the management theory and so on. When I came back, I wanted to try to apply that at the center. As it turned out, I don't think NASA in general—maybe it's a feature in government—has been all that taken with management theory, and I don't think

NASA really knew how to use career development training of the sort that the Sloan School offered.

In fact, to digress a moment, it was interesting that of the forty-five members in our class, roughly two-thirds were from industry and one-third from government. When the Sloan year was coming to an end, without exception the people from industry either received visits, in the most case, visits, or, in some cases, phone calls from their senior management, and they would be told, "You're nearly through your year at Sloan School. Here's what we have planned for you. Here's what you're going to do next. Here's the next step of your career."

Equally without exception, everybody from government received no contact whatsoever from their home establishments. We had to initiate the communication. I wrote Max Faget and said, in effect, "Yoo-hoo! I'm coming back. I would like to do something new and different. What do you have for me?" And the reaction I got was, "Oh, yeah, right. You're coming back. Well, we'll find something." Very, very casual. That should have been a message to me, but at any rate.

I think I had done my thing on Apollo. I was interested in seeing the final missions, but I was really looking for moving out of the engineering area into something different. So when the opportunity came to join the Space and Life Sciences and move into the lab, that sounded good to me, and I guess I thought at the time, "Okay, I can now bring some of this newly acquired management theory and understanding to bear on this particular task."

So I've seldom stayed at any one thing for more than three or four or five years, and the LRL was another example. I was there, I guess two and a half, three years before moving into the remote sensing area, which was yet another issue.

BUTLER: Sort of similar. You were dealing with rocks in the lab, and remote sensing is looking back at the Earth or the other planets. And, of course, all of them were applying still some engineering in some respects.

How did the opportunity arise to go into the remote sensing area?

ERB: Well, if you recall, in '68 there was a lot of unrest in the country, civil disturbances, the Watts riots, the Martin Luther King [Jr.] assassination. I think I went through a period in that time frame that I really began to question the merit of the lunar program and space-faring. I was, I think, searching for something that was perhaps of more [direct] benefit to humans and life on Earth. When the work of the lab began to wind down and we were no longer worried about the quarantine, I began to ask myself, "Okay, what can we do in the lab using these facilities, chemical, analytical facilities, and other things to perhaps support things like remote sensing," which NASA was doing. That really never came to anything. The facilities were too unique to geoscience and lunar materials to make them generally applicable. But at any rate, we explored that a little bit.

Then the opportunity came to move into the remote sensing area and run what was called the applications office, which was basically how could you apply remote sensing to practical problems, in this case in the Houston area, problems in forestry, water quality, range management, and so on. That seemed to just sort of mesh with this sort of—what shall I say—increasing social consciousness that I was developing. So that sounded like a good thing, so I made that move in, I guess it must have been early '72.

BUTLER: In between or, I guess, while you were still with the Lunar Receiving Lab, Apollo 13 happened. Though you had been out of the direct engineering involvement, there were some considerations on Apollo 13 such as had the heat shield been damaged by the explosion, and getting them into the roll after the accident and so forth. Did you have any involvement on that?

ERB: Not really. I was watching it with great interest, although, as a matter of fact, during that time I by that time had been promoted to be manager of the Lunar Receiving Laboratory. Bell had left. It was the one time that I had a badge that allowed me into Mission Control Center at high-activity times. So as soon as I heard about the accident, I zipped over to the viewing room to watch what was going on. I didn't really think that I could contribute anything in the heat shield side of things. My successors in the Structures and Mechanics [division] were perfectly well on top of the problem, but I was, of course, desperately interested in it from the standpoint of what was going on to the mission.

And also, of course, we realized that this was going to mean that we didn't have a sample return issue, that we would not be, in fact, activating the lab during that particular mission. So it made it quite a different mission from 11 and 12.

BUTLER: I'm sure you weren't alone in following the mission. I'm sure quite a few people—

ERB: Absolutely amazing time. I've known Glynn [S.] Lunney for a long time, and I was really impressed with the way he pulled the crew together there in the Mission Control and was doing the diagnosis. I have a great regard for flight controllers. They're another unique breed, and they earn their money under times like that.

BUTLER: Yes, some hard times.

Working in the remote sensing, in the Earth observation, what roles did you get into there? What were some of your duties and tasks and projects?

ERB: In the Earth Observations Division, I was at first running the Applications Office, as I mentioned, and then later I was chief of the division. This was an exciting time to get into the remote sensing area. Up to this point it had been largely dependent on aircraft observations, but in 1972, if I've got my chronology straight, NASA launched the first—what is now called LANDSAT, but at that time they were called the Earth Resources Technology Satellite, or ERTS. Terrible acronym.

At any rate, one of the very first things I did was we put in proposals to NASA Headquarters to do investigations in the Houston Area Test Site, which was an eighteen-county area that we had defined in the region, to use the data from this Earth satellite to assist various parameters on the ground. So I was principal investigator for a series of investigations in range management, agriculture, water management, urban land use, and so on. So again it gave me an opportunity to learn about a whole bunch of new things.

It was also interesting in that this was one of the hot areas in the center, in that every visitor who came to Johnson Space Center [formerly Manned Spacecraft Center] during those years wanted to know what we were doing in remote sensing. So we had innumerable visitors to deal with, and I tended to be the spokesman to talk about the remote sensing program and what we were doing. Sometimes we were looking for collaborative programs with people from other agencies. Sometimes there were people from like China and other countries. I had the opportunity, at the request of George [M.] Low, to brief Anwar Sadat when he was visiting the center, for instance. It was good from the standpoint of not only learning new technologies and new scientific disciplines, but also getting a much more international outlook.

In the '73, '74, '75 time frame, we decided that we really needed a major project to focus the technology development, and our division chief at that time, Bob [Robert B.] MacDonald, who, bless his heart, was another one of these irascible people who had an awful lot to bring to the program, but some problems with dealing with people, and I was the buffer and the interpreter again.

He conceptualized the need for a program that would really stretch remote sensing, again the challenge notion. He said, "What we really need to do is something very challenging, like inventory wheat around the world." Okay. So at any rate, we conceived, and I was one of the major architects, of what we call the Large Area Crop Inventory project, or Experiment, LACIE, and we ended up focusing in on a few of the major wheat-growing countries—United States, Canada, Russia, Australia, Argentina, and, to a much lesser extent, Europe. So my horizon at that time blossomed to international and I negotiated agreements with Australians for sharing data and so on.

So it was an interesting time from a number of points of view—technology, international engagement, and trying to use space means for human needs. We developed some very good technology. As it turned out, the United State Department of Agriculture [who] was our main customer, were—well, let me say sort of set in traditional ways, and they never really adapted the technology to the extent that they could have. Years later they did cycle around, but it was a little bit frustrating that we were doing all this good work and our putative user out there was not really that interested in picking it up. They did assign people here to the center, and those people, of course, were very keen on it, but large bureaucratic establishments change only [slowly]—

BUTLER: There's many different applications. You mentioned the one with agriculture and looking at the wheat and that project. Of course, there's applications for meteorology, for other land use and so forth. Were others slow to adapt to that use as well, like the Department of Agriculture, or did some people realize the potential right away, or did you notice much any—

ERB: Not that I was particularly involved with it, but from discussions with meteorologists, I know that the meteorological community was very slow to adopt or to see the merit of imagery from satellites. And we're so used to this now, I mean, we have got to the point where we look at the imagery from a satellite, everybody can make some interpretation, you know which way that hurricane is going, you know which way that front is moving, but in the early days I understand the meteorological community just really didn't think that would be all that much use, and only slowly came around to the notion of [utilizing] this sort of data from space.

So I think totally new ways of doing things just take a while to penetrate. Eventually the merit of a new data source sort of penetrates, and people pick it up and embrace it and then very soon can't do without it. I think that's probably been true in most areas of space data applications. Yes, we worked a lot of different applications and worked issues with Houston Lighting and Power as to how the plume of effluent cooling water from a powerplant gets spread out through the bay and what does it do to the fish and this sort of thing. So it was really intriguing from the standpoint of having to learn a lot of new things and meeting a lot of neat people in the process.

BUTLER: Very interesting area, and it seems to be an area that, as you said, now we take our weather reports for granted and flip it on in the morning before going to work, and at night you're always having that weather report looking at the satellite picture. But again there are these other uses, like you said, the Houston Power and the agriculture. But it seems like a lot of the general average person or average public aren't as aware of the fact that this is coming from space technology, from satellites and down to Earth. Did you see any of that, or have you seen that?

ERB: Yes, you see it now, I think, in terms of a new technology, once it's adopted, sort of drifts into the background and becomes transparent. Like you never think, when you make a telephone call to Europe, that you're probably going via satellite now. It's just sort of there. I think people who navigate their boat around Galveston Bay or the Caribbean or wherever, with their hand-held GPS [Global Positioning System] receiver, they're probably focused on the fact that they've got this little gizmo that does things that are effectively magic, and the fact that it is simultaneously querying three or four satellites that are zipping around, invisible above them, never occurs to them.

But you know the technology has come along in some marvelous ways, and one I'm really excited about is the so-called precision agriculture, where a farmer can optimize the yield from his fields by knowing where the weedy areas are, where the low-lying areas are, tracking these from year to year, applying fertilizer only in the cases where it's going to be most effective, applying insecticide where it's going to be most effective. A lot of this data comes either from satellite observations of the field or from GPS data that he acquires as he drives his tractor around, so that he knows what is where. These are things that again I'm sure the farmer probably doesn't even have to think about it; it's just a new sort of data source.

A hundred years ago, people got used to the idea of a newspaper appearing on their doorstep every morning with news of the world that came in by teletype from places that were inaccessible before. Now people click on the weather or they do something like locate themselves with their sailboat using space technology and never even think about it, which, in a way, is good, but if you're trying to engender interest in the space program, you somehow have to find ways of keeping this in front of people's mind, that this is what enables all these really neat things to take place. It's not an easy task.

BUTLER: That comes back to the balances we were talking about. How do you find the right one and accomplish what you want to accomplish? It should be interesting to see what happens in the next few years as technology continues to expand and grow.

ERB: Indeed it will.

BUTLER: While you were working with the remote sensing and Earth observation, were you involved much in any of the activities going on on the Skylab missions?

ERB: Yes, I was also a principal investigator on Skylab for the so-called EREP, Earth Resources Experiment Package. The Skylab had a number of sensors, including large-format cameras, radiometers, and we got some excellent data from the Skylab. It was different from the suite of data that came out of the ERTS, in that Skylab had a fairly limited ground track, and some of these sensors depended on the return of actual photographic film, so that it acquired data only over limited regions of the Earth, whereas ERTS got data for everything. But the data quality was much better.

So we looked at some of the same issues as we had with ERTS, but doing it with Skylab and got wonderful imagery from the large-format cameras. It was great to be able to, if you were giving a talk in some town, to be able to come up with one of these marvelous images of the city that you were speaking in, taken from the Skylab. So, yes, it augmented the data that came from the satellite, but it was sort of a relatively short-lived kind of thing in the unique data set in itself.

Of course, in later years, and it was just starting when I left the earth resources area, we began to get some data from the Shuttle, which had its own suite of instruments on certain flights. Of course, the crew was always taking pictures out the window, which was always fun to tell them what to look for and what to try to image. So Shuttle has turned out to be, again, an immensely useful tool for earth resources applications development.

BUTLER: During this time were you involved mainly with the—as the data came back and looking at the uses and interpreting it, or were you also involved with developing any of the equipment used to collect that data?

ERB: I was never personally involved with the hardware side of collecting the data. We sort of kept track of what was going on so that we knew the characteristics of the instruments, but for the most part, that was done elsewhere. The ERTS and then LANDSAT series of instruments were managed out of Goddard, and we had a close working relationship with Goddard to get the data back from them.

So the Earth observations work that went on here at Johnson was essentially interpretation, extraction of the information from the data, and, to some extent, trying to use that data in predictive models. Our crop inventory was a good example. We used the raw satellite data to figure out where wheat was growing, what areas of wheat were planted in what areas, but then the other data you need if you're going to—or the other information you need if you're going to project production is what is going to be the yield. How many bushels per acre are you going to get from a given region of the country? That you can't observe directly. So we ended up coming at that from essentially a meteorological model that says given certain temperatures and rainfall, here is the kind of yield you can expect. So you then combine that yield with the area from the satellite data and put together a projection of production. It worked quite reasonably well.

In the later phases of the remote sensing activity here, we began to look at broader questions such as one project was called global habitability. How habitable is the Earth going to be 10, 50, 100, 1,000 years from now? We developed some good notions, but never implemented that.

I'd make an observation that the effort that went into the remote sensing activity, gathering the data, the satellites, the data processing, and all that was immense. There were serious gaps in terms of taking that data and applying it to the actual physical situation on the ground. We just don't have the models to be able to make in all cases a good correlation between what you see on the satellite and what is going to result in terms of the growth of a forest or the growth of a crop. So there's a great deal of work that needs yet to be done.

BUTLER: And will continue to be various areas, to continue.

You eventually decided to move on from NASA and move into other opportunities. Was there any driving consideration or were you just ready for the next challenge?

ERB: Probably a little of both, but there was certainly a driving consideration. The remote sensing activity at Johnson was effectively terminated in about 1984, when NASA reorganized, and the Office of Applications, under the new reorganization, they had to look after the care and feeding of Goddard and JPL [Jet Propulsion Laboratory, Pasadena, California], but not Johnson. So there was no sort of, let me say, sustaining interest from Headquarters in a remote sensing activity at JSC, and Gerry [Gerald D.] Griffin, who was the [Johnson Space] Center director at that time, was faced with the problem of what does he do with a division that is consuming or encumbering 100 civil servants and takes 3 or 4 million dollars a year to support, if nobody at Headquarters is interested in that.

So we ended up in the last few years sort of fighting a losing battle, and Griffin made the decision—and I would have done exactly the same thing had I been in his shoes; I think it was the right decision—he said, "This is not something that JSC can be involved in." At the same time, space station was starting up, the Shuttle was in full flight and still requiring a lot of resources, so Griffin essentially abolished the division, and that gave me the rather strong motivation to consider an early retirement.

For a while I thought about going to work in the space station area, but I had by that time nearly twenty-six years with NASA. I was fifty-three or four, somewhere in there. I began to do the math, and it looked as though perhaps there would be other opportunities. So I took the plunge and did take an early retirement.

As it turned out, during the last couple of years that we were active in earth resources, we were doing a lot of work in extracting information from the data gets you off into the area of so-called artificial intelligence. How do you efficiently pull out just the information you need from this great stream of data?

At about the same time, when space station was starting up, [Senator Edwin Jacob] Jake Garn [Utah], of later fame as a Senate astronaut, was looking for a vehicle to promote U.S. competitiveness, and he had latched in on automation and robotics and artificial intelligence as key areas that if the United States was going to be increasingly competitive, it needed to be pushed. He was looking for a vehicle to push that, and the one that he really selected was space station. He said, "If we promote the use of automation and robotics on space station, that will drive the technology." Again we get back to this notion of providing a challenge to stimulate the technology. So that what can we do in the way of automation and robotics that will make space station better, but at the same time drive U.S. industry?

So NASA put together a committee called the Advanced Technology Advisory Committee, ATAC, and I think that was structured in the late fall of '84, because it was just before I retired. Aaron Cohen was the chair of that committee, and representatives on the committee were people at the very senior management level for each of the NASA centers. However, as is usual with that sort of thing, the work is done by the staff. You can appreciate that, I'm sure.

The key person who picked up the task of putting some flesh on the bones of this notion was a close friend of mine and colleague at the time, Jon Erickson. The lot fell to Jon and a lower-level committee to pull together all the ideas in automation and robotics, show how they could apply to space station, and show how they could be applied to make U.S. industry more competitive.

During the few months before I retired, and for several months after in a consulting mode, I was supporting John as, let me say, the chief ghostwriter for ATAC. So the reports that came out under Aaron Cohen's signature for how automation and robotics could be used on space station were largely things that I had created for that committee. Then for the next two or three years, we went through an update every six months in a report to Congress to show how we had met or fulfilled these goals. I was involved in the first two or three of those updates after I had retired from NASA.

So this sort of led rather neatly into my next career, to move into the work with the Canadian Space Agency [CSA], who, of course, were going to be heavily involved in robotics on space station. So it's amazing how these doors seem to open, but this was another sort of segue from remote sensing to artificial intelligence, to automation and robotics, and on into robotics on space station, with a new employer and a totally different situation.

BUTLER: New change in focus.

In some of my research I ran across an interesting story about how it did come across for you to be able to go to work for the Canadian Space Agency and how that the group there had actually offered you a job opportunity many years back, so it's like you were accepting it, but many years later.

ERB: Your research is very good. I'm impressed. To go way back, in March, I guess now, of 1959, after the demise of the Arrow, one of the things that I did was to submit an application to the National Research Council [NRC] of Canada for a job as an aerodynamicist, and I was very pleased a few weeks later to find that, yes, they were interested in me and they offered me a job as an aerodynamicist.

However, that came about a week after I had accepted the job with NASA. So I wrote the National Research Council and sort of said, "Well, thank you very much. I appreciate that, but I've been offered this job with NASA and it really sounds neat, and it's only supposed to be for two years, so let me get back with you in two years."

To my great surprise, two years later I got a letter from the National Research Council saying, "Two years is nearly up. What's your situation? Would you like to come and take up the position?"

Well, by that time I was up to my ears in Apollo, and the world was just much too exciting to think about a change from that. So I wrote again and said, "Well, thank you. I'm impressed that you're keeping in touch. I appreciate it, but I'm now on to this new project and it looks like I'm going to go a while, so thanks but no thanks."

And then you may have heard this part of the story. Back in 1986, after I had accepted the job with then the National Research Council, to be the liaison here in Houston, I had gone up to Toronto to meet my new boss, another one of my heroes, Karl Deutsch, and Karl and I spent a couple of days at SPAR [Aerospace Limited], who was then our contractor for the space station robotics. We were flying back to Ottawa together, where I was going to be introduced to my new colleagues in Ottawa, and I said to Karl, "Let me tell you a story, Karl." And I went through this with Karl. So I said, "You see, so I'm really now taking up a position with NRC that was offered twenty-seven years ago. Just a little late." [Laughter] So things do tend to come back sometimes full cycle.

BUTLER: All the way around.

ERB: All the way around.

BUTLER: It's good to see they were still interested.

ERB: I was delighted. And just a historic accident that I had maintained my Canadian citizenship during that period of time, because they wanted me to represent Canada here at Johnson. They wanted a Canadian citizen. Through a series of historical quirks, I had not become a naturalized U.S. citizen, so I was still qualified for their short list of candidates.

BUTLER: Interesting how fate happens or twists and it all ends up working out.

ERB: It certainly seems to. I've been richly blessed in terms of this kind of opportunity.

BUTLER: Very good.

About your opportunity at the Canadian Space Agency. When you first came in, or at the time it was the NRC, what were your initial tasks and responsibilities and the state of the space station at the time, and how did that evolve?

ERB: I joined the CSA, or the National Research Council, in March of '86, and set up the liaison office here at Johnson. NASA had been beating on the partners to get liaison officers in place. The Japanese were obliging, and they had somebody here in short order, the Europeans almost as obliging, and Canada was going through this process of trying to recruit the right person. So we were a few months behind our other international colleagues in getting a person located here.

Basically my job was to represent Canada both at a technical and a programmatic level to the NASA program. In the several months before I had joined the agency, my boss, Karl Deutsch, and his prime contractor representative from SPAR had been coming down to JSC just about every week, so I think they were absolutely delighted to have somebody on site so that they did not have to truck down to these SSCBs [Space Station Control Board] that went on, and could leave the job to me. It was a couple of years, I think, before I ever saw Karl down here again. At any rate, that was okay by me. But it worked out very well.

I had been doing a little bit of work on space station while I was consulting with Eagle Engineering and with Lockheed, so I sort of had a pretty good feel for how space station was developing. I had all the contacts in the organization, pretty much knew everybody, so I could work in a way that was somewhat different from my European and Japanese colleagues. I could engage people in hallway conversations and work informally and sort of keep plugged into the program. In a way, that was helpful in another regard because Canada's part of the space station, as you know, is to provide certain robotic systems which are really part of the sort of core infrastructure of the space station, whereas the Europeans and the Japanese are bringing laboratories. So they could take a more hands-off formal approach and work at a distance, and they set up offices off site, and I always kept mine in Building 2, [or later in] Building 4S.

So at any rate, my job was basically to make sure that people were talking to each other when they needed to, that NASA knew Canada's views on various topics, and then when we would identify problems, we'd sort of get them worked at the right technical level and try to get them solved before they blew up into big management issues. I think overall it worked quite well. I gradually increased staff, got some local contract support, and through that contract support hired three of my former NASA colleagues who had also retired. So I think we were able to bring to the Canadian program really solid technical skills and contacts and means of communicating with the NASA program that worked out very well for both parties. We've had the most cordial possible relationship with the Space Station Program over the years.

BUTLER: That's very good.

ERB: Of course, as you know, it's been a long and sometimes bumpy road as we've gone through various redesigns on the station, and within a few months of taking on the job I spent six weeks at Langley as we went through the first of many redesigns to shrink the station, try to stay within budget. It's been a constant challenge with each of the partners in turn having financial problems.

I would say the stabilizing influence of international involvement has been borne out many times. There have been times that we were very helpful to NASA and helped them get their budget because of the international commitments, and, correspondingly, times when Canada and the other partners have had problems with their budget, and NASA was able to help us out and wave the international commitment flag and keep the program on track.

I'm generally encouraged now that we've got a program that's going to go and is going to work well.

BUTLER: It's under way.

ERB: It's under way.

BUTLER: Based on your work with the international cooperation on the space station, do you see this as a future for large space projects or going back to the moon or Mars, that it would be an international venture?

ERB: Very definitely. I think even before we get to that, I don't know whether your research has dredged up what my current project is, but over the last several years I've become again concerned with how space can apply to human needs, and have become very much interested in the notion that's been around for a long time, of space-based solar power. I started doing my own exploratory studies back in '91 and have continued and grown in that area of my involvement, to the point now where I spend most of my time on that, and I have a successor doing the liaison task, which is very nice.

But if you think about something as massive as capturing solar power in space and transmitting it to the Earth, which I think is an idea whose time has come again, as I'm sure you know, NASA has put a lot of effort into that, in studies back in the late seventies, and at that time for a variety of reasons it was not pursued. But the idea is sound, and I think we will see it implemented within the next couple of decades. It is very much an international activity.

Of course, a satellite is intrinsically an international device. It goes over every country in due course. If you think about what I call the enterprise model, then I can see a kind of international structure that would involve the developed, the industrialized countries of the world building and launching and operating the space segment, and the developing countries, which is where the energy is needed, building the receiving sites and buying, in effect, energy from the space segment, capturing it at their local receiving sites, and using it for their own nondevelopmental purposes. So it is 100 percent an international activity.

As a matter of fact, just this past July, I participated in UNISPACE III, which is a United Nations conference on the peaceful uses of outer space, in Vienna, and we had a workshop on space solar power. Part of what we were proposing was adopted in the Vienna Declaration of the United Nations, that nations should explore this [space-baased solar power] and do the following, and so on and so on. I think we will again see this coming about over the next coming years.

BUTLER: Certainly resources and power resources are a big concern and have been for a while, and certainly something we're never going to be able to do without.

What are some of the—you mentioned having the satellites and the receiving stations. Is there basic studies on how many satellites approximately would be needed?

ERB: There's a lot of work going on at the moment, mostly in NASA, as a matter of fact, on what the system architecture should be. In fact, it's really very gratifying to see NASA putting some serious money into this for the first time in many, many years.

I will take a little credit for having brought that about. Beginning in 1992 through the American Institute of Aeronautics and Astronautics [AIAA], a group of us started a series of workshops on international cooperation, and we've held conferences about every two years. The second one of these was in Hawaii, and we had a working group on space solar power, at my instigation. I and a Japanese colleague from the University of Kobe co-chaired this working group.

We had excellent support, including one person from NASA, some from the academic world, and some from other countries. We basically said, "Hey, this is a real problem, and if NASA doesn't do anything about it, the other countries of the world are not going to consider that it's a serious issue or a serious solution to the problem. Basically NASA should get involved."

There were individuals within NASA, in particular Ivan Bekey at what was then Code X [at NASA] Headquarters, and he was pushing from his end. But this external stimulus from a professional society, from the AIAA, that said, "Here's really something NASA should do," coming in as sort of a formal recommendation, helped the forces within NASA to say, "Hey, let's put a little bit of money into this."

So NASA, in 1995, started what they called the Fresh Look Study, and took, for the first time in fifteen years, a fresh look at space-based solar power. That has grown to the point where now this year NASA's spending something like 12 million dollars on space-based solar power, and I think this is going to be the genesis of a new and significant activity that will be, I hope, sustained this time. But very much an international activity, very much, I think, an excellent example of what space can do for humanity.

At the moment, one of the data points I use in my talks is that there are 2 billion people in the world who have no access to commercial energy. That doesn't even mean electricity; that means commercial energy. Not even kerosene, let alone electricity. The path that we're going on now—and I don't mean to get on my soapbox, but I guess I am—the path we're going on now, the default path, that will be 4 billion people without commercial energy in another couple of decades.

So I think we have a desperate energy problem, and that interconnects with, I think, an equally critical climate problem. So finding new ways of getting carbon-free energy, to me, is absolutely one of the most critical challenges of the next century. Feeding people is one, and providing energy, which drives almost everything we do, is another.

So I'm hoping—I won't probably live to see it come to fruition, but I'm hoping that what we do now will perhaps stimulate our successors and we'll see, I believe, an application of space in providing power that will dwarf even its importance in communications. There is a much greater percentage of the world's gross national product, or gross world product, spent on energy than on communications, and I think space power can become a major part of that. So it's been an exciting new twist to my long and checkered career.

BUTLER: Certainly a new twist, but yet it still all ties together, one to the next, and a very important new twist, as you say. It should be interesting to see what does happen with it.

Looking back over your career with NASA, and we've talked—or actually not just with NASA, but in the space program, we talked a little bit about Max Faget and Bob

Gilruth. Were there any other individuals through your career that made a big impact on you or helping the space program as a whole that you think were critical?

ERB: Yes, Gilruth and Faget, clearly. Joe Kotanchik was, I think, one of the pivotal mentors. He was very high on persistence. Joe had a saying that "persistence and perseverance made a bishop out of his reverence." Joe's notion was, no matter how smart you are or anything else, you've got to be persistent. I've learned from that, and I think my agency perhaps considers me too persistent on this issue of space solar power, but I've made some headway. Joe was also a person of fierce integrity and balance, one of those key people.

In the academic area, going back a long number of years, there were people at the University of Alberta that were pivotal—George Ford and a fellow named Tom Blench. At Cranfield, at the university there, my thesis advisor, Terry Nonwheiler, and one of the giants of the aerodynamics field of this period, A. D. Young was department chairman. More recently, my again mentor and boss at the Canadian Space Agency, Karl Deutsch [now President of the International Space University], has been one of these people that just is a sort of landmark in your life, that you respect and learn from, people that make a difference. So I've been blessed with good mentors along the way, very significant.

BUTLER: That's good. It makes it more interesting and easier and enjoyable environment, as well as with the technical challenges and managerial challenges.

Looking back also, what would you consider to be your greatest challenge?

ERB: Well, I'd like to think it has yet to come, so I'm not sure. I think the analytical melding of all the data that gave us the confidence in the Apollo heat shield performance, that is sort of one thing I look back on and say that was, in engineering terms, not in scientific terms, but in engineering terms, that was about as close to pioneering as I've ever gotten. It was probing into some really new areas. There wasn't a lot of precedent to go by. We had to do something, so you did what you could. Then very thankful when it works. So that, I think, is one that stands out.

BUTLER: It certainly was a big challenge and certainly was met successfully. As you said, you probably do have more coming. [Laughter]

ERB: We'll see.

BUTLER: And in comparison, or maybe the same thing, what would you think of as your greatest accomplishment?

ERB: Well, in a purely professional sense, certainly the Apollo era and the successful return from the moon, that was a landmark.

I also feel good about the large-area crop inventory experiment that we did in the earth resources area. That was a well-conceived experiment, it was well executed, even though the technology was not adopted by our users to the extent we would have liked. It was still a good professional piece of work. I felt good about that.

Under my current circumstances, I have finally got a small space power program going in Canada, and I have persevered and I feel good about that. Now, whether it comes to fruition in any significant way remains to be seen, but I think I will be proud of that as another one of those things that you look back on.

BUTLER: You certainly have many things that you can say that you accomplished and did well. I'm sure that your work on the solar power will come through eventually.

ERB: We hope so.

BUTLER: A lot of good reasons for it.

You mentioned earlier that it was in fifth grade when the explorer came and talked about going to the moon, you thought that would be really interesting. Would you ever have thought, or even possibly conceived, where all the different stages your career would take you?

ERB: Absolutely impossible to predict. I think you just sort of have to trust in divine providence that you will get the guidance, that the doors will open, and sort of try to do the best you can in whatever circumstances you are, learn as much as you possibly can, try to keep your vision broad, try to look ahead, but you just can't predict. You just never know. Things are just much too stochastic for that to work. But somehow it seems to.

BUTLER: Certainly does. Before we close, I'd like to ask Kevin and Sandra if they have any questions.

RUSNAK: I did have a few. I'll kind of work my way backwards chronologically as I've been thinking about this. First of all, I guess I find it a little ironic that you had in the eighties worked on a competitiveness council for America, and one of the areas you mentioned was in robotics, and later on you worked at the Canadian Space Agency using Canadian robotic technology on the space station. Did your work from the former program have any application later on?

ERB: Not really in the specific sense of technology. I think the work that I was doing in the '84, '85 time frame, working on the ATAC reports and so on, I was really learning about robotics and artificial intelligence, and in general I'm a fairly quick take. I can go into some new field and pick up at least the essence of it. But the Canadian program in robotics was really well established, and I didn't have much of any technical involvement with it.

As you know, I'm sure, the manipulator arm for the Shuttle was also developed by the same group of folks in Canada, both on the government side and on the industry side with SPAR. The current Canadian robotic systems for space station are sort of direct technological descendants of that technology that was put together in the late seventies for Shuttle. So I don't think I brought anything of a technical nature to the Canadian program.

What I was really offering to Canada was management contacts and an awareness of the situation here, plus enough familiarity with the area of robotics to be able to make sure that people were talking to each other that needed to. But in no way would I pretend to represent myself as a robotics engineer or anything. I can talk the talk, but I would not sit down and try to do hard-core analysis in that area.

RUSNAK: Moving backwards a little bit, remote sensing was an important part of your career, but I was wondering how was it that what was then the Manned Spacecraft Center ended up with a remote sensing office that was essentially using unmanned satellites or data?

ERB: Well, during the time of the late sixties, early seventies, I want to say a lot of the NASA centers, maybe all of the NASA centers, were trying to find new niches to work in. Clearly Apollo was winding down. What do you do next? And everybody was dabbling at everything. There was very little discipline exercised from the Headquarters level to say, "This center does this, that center does that." So everybody was sort of exploring.

JSC had the advantage of having an air field close by, airplanes, and some individuals, including my dear now departed friend Leo [F.] Childs, started an aircraft remote sensing program. So in the early seventies, '71, '72, in there, NASA-JSC was beginning to fly aircraft to sort of see what you could do to gather supporting data that would eventually allow you to interpret the satellite data. So there was always a blend, I think, of the need to get aircraft data and ground data in order to interpret satellite data.

So the real core of the competence of remote sensing at NASA started off at JSC, and almost all the competence in extracting information from the data was originally developed here. Goddard was involved from the standpoint of building the sensors and launching and flying [the ERTS], but never got heavily into the data extraction side of things. So, you're right, it was not in line with the human space flight charter of the center, but more, let me say, I think, another area of opportunity that the center wanted to try to explore and bring along as a new business line. As it turned out, it as quite successful for a decade, but then for various reasons fell by the wayside.

RUSNAK: You mentioned Goddard earlier. You discussed how JPL and Goddard received a lot of that work. After JSC's office was disbanded, how were the intercenter relationships in this particular field?

ERB: Oh, I'd say very cordial and effective. A few people, a few, one or two from our program here went to Goddard after the demise of the program here. The kinds of activity we had going on with the Goddard people during the remote sensing era involved mostly data transfer, and that was always a very productive interface, and we worked well together. We never had all that much to do with JPL, because they were really interested in the planetary stuff, gathering images from other planets.

Although I might digress. That brings something to mind, one of those just little vignettes that's a landmark in your career. In 1961, I was named as the JSC representative to a committee looking at what would be the significance of the Surveyor missions to NASA's programs in general, and Apollo in particular. Surveyor, you may recall, was the first lunar soft lander. So this committee met two or three times to sort of talk about what instrumentation Surveyor had, what it was going to yield in terms of information.

One of the major concerns at that time that had been broached by some people was the lunar surface was yards and yards and yards of dust, and that any spacecraft that landed on it was going to sink out of sight. Some of us who had looked at some of the lunar photographs, and being dumb civil engineers, didn't know any better, said, "No, no problem. It's not a trouble."

But at any rate, the other question was, well, how are things going to heat up and cool down. So there was a little bit of thermal instrumentation on Surveyor, and we were anxious to sort of squeeze as much information as we could out of that, so we had made predictions if Surveyor lands in a given location, is more or less horizontal, then the temperature at this particular sensor should do the following.

And I remember one of my relatively few trips to JPL, I was in the control room at JPL the night Surveyor landed on the moon, and sitting next to Harold [C.] Urey, who was a Nobel laureate of great note, who was also on this committee, so we were watching the real-time data coming back from Surveyor. That was sort of a neat thing to do. But our involvement with JPL was pretty peripheral, because they were doing their thing on planets and we were doing the moon and the Earth.

BUTLER: It must have also been interesting to see Apollo 12 land near one of those Surveyors.

ERB: Yes. And to see the camera when it came back, the Surveyor camera.

RUSNAK: You mentioned also earlier that your involvement with remote sensing came out of your developing social consciousness, I think you called it. In what was really, as you described it also, a goal-oriented agency, did you find that was a common occurrence, that a lot of people were really developing that? A lot of people haven't mentioned that.

ERB: No, I think it was rather unusual, perhaps. I think there were certainly a few kindred spirits who were concerned about those issues, but my belief is that most people who are hard-core techies, they've got their technical thing and they may not perhaps think as much as they should about what are the social or political or economic ramifications of technology. It's something we should think more about. But I couldn't say that I found that in any way common amongst my colleagues.

RUSNAK: Okay. When you and the rest of the AVRO group first came down to the Space Task Group, you were bringing a certain, I guess, corporate culture or way of doing things coming from industry and working in what would be a production facility. How did that compare with what was going on at Langley and the sort of attitudes, and how did those things mesh?

ERB: Very good question. I think, in effect, you might not expect it, but I think they meshed pretty well. Most of the people at Space Task Group in '59 were researchers from the Langley Research Center, awfully good people, people who had been really doing great pioneering work but, nonetheless, dealing with small one-of-a-kind models, flying them on rockets out of Wallops Island, that sort of thing. Very little exposure to manufacturing processes, to rigorous routine operations.

And we'd been doing things on the Arrow that were quite unique at the time, like, for instance, telemetry on the aircraft coming back to a control center, so as you watch in real time what the aircraft was doing. Now, this is common practice now, but at that time it was novel. So we were doing things with a production aircraft that were quite new. The industrial discipline, I think, was particularly important.

That's one thing I might mention that I think we did bring to the program that was very important in its success. When we first arrived at Langley, there was a letter contract with McDonnell-Douglas [Corporation]. Anybody who had a new brain wave as to something that should be done differently would either pick up the phone or go out to McDonnell-Douglas and tell the contractor, "Do this, do that, do the other." There was just absolutely no discipline or configuration management.

Jim Chamberlin, who was the nominal leader of our little group, said, "Hey, this isn't going to work." And he instituted what he called a Capsule Configuration Board, a CCB. Or Capsule Control Board, I guess it was. Later became Configuration Control Board. He basically forced all instructions to McDonnell-Douglas to go through—well, at that time it was McDonnell [Aircraft Corporation], I guess—to go through that board, and brought a much needed discipline to the program. I think that might not have happened as soon as it did, or might not have happened at all, just given the sort of normal style of researchers.

I would say the Langley folks respected this as something needed and accepted it. There were no problems. So my reaction was that we had a very productive melding of styles and backgrounds with these two groups.

RUSNAK: A few people we've talked to have said something very similar, that each group had its own strengths that blended very well to get the job done in this case.

I have one final question, going back even earlier. Clearly the development of the concept of a ballistic capsule was important. Previous ideas for reentry vehicles were slim, very aerodynamic. What knowledge did you have of that at the time of the introduction of ballistic capsule, and what are your thoughts, as someone who was trained in aerothermodynamics?

ERB: In a way, Mercury was a bit of a surprise to me, because I hadn't really thought much about ballistic capsules. When I did my thesis work at Cranfield, the thinking was very much streamlined shapes. You had sort of airplane-like things. And all the supersonic work up to that time had been with the X Series. Here again you're dealing with airplanes with nice thin wings, sharp noses, and all that sort of stuff.

I had not thought about the problems enough to realize that you really need, if you're going to—until I got to grips with Mercury—you really need, if you're going to keep the accelerations low and dissipate all that energy coming back from orbit, you really need a blunt shape that will produce high drag and allow you to keep G levels down. Also by having a blunt shape and spreading the heat over a larger area, you can make the heat shield problem tractable. So when you start thinking about it a little bit, it quickly becomes very apparent that that's the way to go.

I think the researchers at Langley, particularly Faget and Piland, they were probably the ones that really realized this, that, hey, that's the only way to do it. And as we've seen, it was twenty years later before something as sophisticated as the Shuttle could do an entry, and even then during the entry phase Shuttle is tipped up with its belly to the wind and is a big blunt vehicle. So the same principle engages, even though it can later go over and fly like an airplane. So, yes, by the time I joined Space Task Group, the Mercury configuration was pretty largely set. That had all been agreed to, and wind tunnel testing, as I mentioned, was under way, and it was really blowing and going.

RUSNAK: Thank you. That's all I have.

BUTLER: Sandra?

[Addressing Mr. Erb] Are there any areas that we neglected to cover, that you would like to have anything that you'd like to expand on or any last thoughts?

ERB: I'm sure there will be, but can't think of anything right at the moment.

BUTLER: We can always add them into the transcript later. I want to thank you very much for joining us today and sharing your history with us.

ERB: It's been a real pleasure. Thank you so much.

[End of Interview]