ORAL HISTORY TRANSCRIPT

ERNST STUHLINGER
INTERVIEWED BY MICHELLE KELLY
HUNTSVILLE, ALABAMA – 8 DECEMBER 1997

KELLY: The following interview of Dr. Ernst Stuhlinger was conducted on December 8, 1997, in Huntsville, Alabama.

I'm sitting here in Huntsville, Alabama, speaking with Dr. Stuhlinger. It is December 8, 1997. I'd like to thank you, Dr. Stuhlinger. We're very honored that you've agreed to talk to us about your background.

STUHLINGER: You're welcome.

KELLY: We've done a lot of research about your past and about your work with the space program. First off, I'd like to ask you when and where were you born?

STUHLINGER: I was born a long time ago, almost eighty-four years ago, in a small village in southern Germany. My father was a teacher there. I grew up in a rural setting. It was a very beautiful, little, small village, and I was more at home in the stables of the farmers and in the fields and in the woods than I would have been in a town. It was a beautiful youth for me. My love for nature and for all things in nature began probably at that time and has persisted ever since.

KELLY: Where did your interest in rocketry and the space program develop?

STUHLINGER: As a relatively normal young boy, I played with rockets when I was about fourteen or fifteen years old. They didn't amount to much, and I'm almost embarrassed when

I think today of what I did at that time. I took pipes and filled them with grued [phonetic] powder which I made myself, and they got up a few yards in the air, but not much more, and it didn't amount to much.

But then when I was about fourteen or fifteen years old, I happened to see in a journal, in a picture journal, a photograph of a person who held a rocket in his hands, and the caption said, "Professor [Hermann] Oberth believes that man can fly to the moon one of these days, with rockets." That impressed me very much, and at that time, it was long ago, I still believed everything that a professor said. That would not be true today. [Laughter] But at that time it was. I believed that man can fly to the moon with rockets, and I still believe it today.

KELLY: And did you go on to study that, then, in college?

STUHLINGER: I did study physics, which I loved very much, in Tuebingen. Tuebingen is a little university town in the south of Germany. I studied physics, and I had the good fortune of having an excellent teacher and professor. That was Professor Hans Geiger, the inventor of the Geiger counter. He was a very impressive person. He inspired us young students to do more, to learn more in physics, and to really love physics. I did, too.

I also had a great preference for biology, in particular zoology. So these two subjects, physics and zoology, besides mathematics, were my main subjects at the university. I had the idea at that time that I wanted to combine physics and biology interests and become a kind of a biophysicist. That was a field which began to develop momentum at that time. That was in the 1930s.

But then came the war, and everything was different, of course. One could not pursue one's own plans. It was a time full of anxiety, of course. It was a very unhappy time for all of us, for those who were directly involved in the war even more, of course, but also for the young people who saw that their future would be very much in question. So all these dreams of biophysics had to be pushed out and could not be pursued.

On the other hand, there was at that time a new discovery in physics; that was the splitting of the atomic nucleus in uranium, which sets off a large amount of energy. It was discovered in the fall of 1938, and immediately after that, all physics laboratories, I think, in the world, in other countries, began to study and to work in the field of the transmutation of uranium and the production of energy by this process.

My boss – that was Professor Geiger – he also worked in that field. At the same time he transferred from Tuebingen to Berlin, to the Institute of Technology in Berlin, and he took me with him as a young assistant. So I continued my work in Berlin at that time, and for a couple of years in Berlin I worked with many other colleagues in the field of transmutation or the production of energy by the uranium-splitting process. We did that work in a loosely organized program which was headed by Professor Heisenburg in Berlin, and I worked there, too.

But then came the relentless grip of the military, and I had to put on a uniform, and I was sent to the Russian front as a PFC [Private First Class]. So I spent one and a half years on the Russian front until early in April my unit was sent to Stalingrad. We didn't quite go into Stalingrad because the Russians had closed the ring around the city, but we proceeded to that ring of the Russians. We were badly beaten at that time and had to turn back and then had a long foot march, about 600 miles, back through the wintery Ukraine, to the German lines again. It was a very hard time in the way some of us made it, and I was among the lucky ones also.

After we had reached the German lines again after our retreat, I was reached by an order from Berlin which said, "You are to report to Peenemunde." Now, nobody knew were Peenemunde was at that time of those people to whom I had access, and my sergeant said, "Well, hitch a ride on one of the trains which go back from here, go to Berlin, and in Berlin somebody would probably know where Peenemunde is and what they are doing there."

So I went back – that was in April 1943 – to Berlin, and from Berlin somebody told me that Peenemunde is an island in the Baltic Sea, the island of Usedom. So I traveled by train to that little island and came to the place which we know so well today as Peenemunde. It was a very active place where the big rocket was developed under [Wernher] von Braun. We called the rocket "A-4." [Joseph] Goebbels later called it a V-2, which stands for retaliation [vengeance] weapon, but the A-4 was our designation, and up to this day, we old-timers, when we talk about it, we talk about the A-4. So that was the A-4 rocket in Peenemunde, and I learned about its existence, and I was very much impressed by the enormous task to build a precision rocket.

One should not forget that up to that time there had been no guided rocket invented. All the rockets were relatively erratic. They were Fourth of July rockets and some rockets for practical applications also, but they were unguided, they were crude, and no one knew exactly where they would end up. That project in Peenemunde set out to build a rocket that could be guided, could be controlled, and really would be a precision instrument.

Now, some people claim today that von Braun developed that rocket only in order to enable [Adolph] Hitler to win the war. Now, historically this is just not correct. Von Braun began to build rockets when he was about eighteen or nineteen. Well, as a young boy already he built rockets, and they also were crude and unguided and all that, but he began a systematic work on precision rockets when he was nineteen, in Berlin.

When he was about twenty years of age, the Army, which was at that time the Reichtswehr – that was before Hitler came to power – the Army had also a program of developing rockets for military application. They realized that von Braun and his few co-workers were working on precision rockets, and they also realized that those young people, particularly von Braun himself, were very serious about their work and that they had made already verified progress. So they offered a contract to von Braun to work for the Army and help the Army to build a rocket of defense. Now, von Braun describes that time in great detail in some of his own memoirs, and he describes how he was wavering between the decision to go with the Army and accept the support of the Army and build his rocket with Army support or whether he should continue as an amateur with a few people and no money. He decided that it would be absolutely hopeless to make any progress with a precision rocket on the basis of amateur work of about a half dozen eighteen-, nineteen-year-old young people.

So he joined the Army, got a contract in 1932. The Army supported the work. Von Braun made good progress. That impressed the Army, so the Army built a large center for rocket development in Peenemunde. It was built in 1936 and 1937. From that time on, von Braun developed his rocket in Peenemunde with the support of the Army.

Now, at that time Hitler did not want to have anything to do with rockets. He said, "No, I don't believe that thing will work." There was an Army general, von Brouwich [phonetic], who said, "Well, I don't think so. I think there is something to it, to these rockets. One of these days they will be able to fly in a controlled manner, and we should continue to support these young people." So Von Brouwich saw to it that enough money was available to build up the development activity, to hire more people, and he succeeded in keeping Peenemunde alive. By the way, all of that is described in some detail in the book here, so you can read the details in there.

KELLY: Thank you.

STUHLINGER: I wouldn't like to go too far into all of it.

KELLY: I understand. It's already documented.

STUHLINGER: We would run out of time. Now, the end of the story was that by 1945 the war came to an end, finally. Before that end, von Braun tried to fan out the people in Peenemunde to other places in Germany simply because the Allies had started to throw bombs on Peenemunde. There was a very serious attack in August '43, and other attacks followed. So the Allies knew exactly what was going on in Peenemunde and wanted to destroy the situation, which in a situation of war is normal, I would think.

Von Braun was then ordered to take his remaining people in Peenemunde and his instruments and machines and production machines and laboratories and move south. He did that. He moved as far as the southern border to Austria near Garmisch-Parten Kirchen, in that area. Again, it's a long story. It is described in the book again, and I would like to make it very short. Hitler died or killed himself in April 30, 1945. After that, there was a kind of chaos in Germany. That was to be expected. One of the points was that the guards that had watched over von Braun from the SS – you know what the SS was? It was special troops. They had guarded von Braun very carefully that he wouldn't escape from their grip, but after Hitler was dead, they disappeared. They put on civilian clothes and spread out into the population [unclear].

Von Braun was at that time in a little mountain resort place near Garmische-Parten Kirchen. He said, "Well, we cannot sit here forever, and it may be that even the Russians would capture us, but if we are to be captured, we would like to be captured by the Americans." So he sent his brother, Magnus [von Braun], to the approaching line of the Allies. The Allies were already in Garmische, and von Braun was in the mountains. There were a couple of miles in between. Von Braun told his brother Magnus, "Go down there and try to make contact with the Allies and tell them that we are up here waiting for them." Magnus spoke a little English. Well, he spoke English quite well, and the reason is a very interesting one.

KELLY: How's that?

STUHLINGER: His parents lived in Berlin. His father, Wernher von Braun's father, was at that time — he had been Secretary of Agriculture under the democratic government. He left his political position when Hitler came and became the head of a banking house in Berlin. So he was a prominent man and family, still, and they had a nanny for their children. Do you know what a nanny is?

KELLY: I sure do.

STUHLINGER: The nanny happened to be an English girl, and she brought up little Magnus and taught him English before he learned German, and so he spoke English. He went to the Allies. Again, that's a very interesting story. It has been written here in the other books also. After some time – I'll just skip over some of the details. After some time the Allies came and met von Braun and interrogated him and found out that he really was the man behind the Peenemunde work. So they said, "We want to help you and keep you under guard in captivity, more or less, but we want you to go to America as soon as possible. You should take some of your people with you so that you can continue to build rockets in America."

KELLY: May I stop and ask you one question?

STUHLINGER: Yes.

KELLY: What was the reason behind the decision to go to America as opposed to Russia? Was there a big decision there?

STUHLINGER: Miss Kelly, it was not a big decision. It was a very straightforward and immediate decision. First of all, I should mention here that for most people in Germany, particularly for people like me and my colleagues, the Americans were never real enemies during the war. We never felt like it, even though they threw bombs on us and all of that. But America was for Germans, and particularly for young Germans, always a kind of a dream country where they wanted to go one of these days when they are older. They wanted to go to America. America had such a glorious image for us in Germany, and the idea that we are enemies did not really come up, even though we were at war. We knew the war would be something transient and not something to stay for a long time. That was one reason.

The other reason was that we knew that the Russians were quite different. The Russians had come into Eastern Europe and into Germany, and there were a lot of cruelties by Russians, and we knew we would not have an enviable fate if the Russians would have captured us.

KELLY: That's a very interesting perspective.

STUHLINGER: It was not a difficult decision. It was very straightforward, absolutely. Also we had hoped at that time that if and when we can go to America, we would have a chance to continue our work on rockets, and for us the rocket was not a military system, but it was meant to go into orbit and into space. It was von Braun's dream and reason to work on rockets from the very beginning.

When von Braun was fourteen years old he said – he was a little grandiose – he said, "I would like to open the gates of heaven for mankind." [Laughter] Doesn't that sound great? And that's what were his driving force and his dream all the time.

KELLY: And he did just that.

STUHLINGER: The work for the military was for him a short interlude. I should make another explanatory remark here. For us people in the rocket project, von Braun himself and he said so many times and others, too, my colleagues and I personally also, we never thought that our rocket would be ready for deployment before the war ended. We'd never had the idea that this would be a war instrument and a weapon, and it was forced into deployment by the SS, and it was forced to be deployed as an uncompleted and immature system. It was not an effective weapon. Never was.

So it would not be correct to say that von Braun wanted to build a weapon for Hitler. It was not so. There was war, and in war, you know, individuals have no opinion. They cannot say, "I want to do that," "I don't want to do this." In the war one cannot do that. War is so different from any other condition or situation in which a country may be.

KELLY: So it was more scientific in focus.

STUHLINGER: Some of this is in here and in our paper I will give you, but it's a good question, and one could talk for a long time about it. Much nonsense is being talked about it now by young people who never have had any feeling or any idea of what it means to be at war.

Another point I would like to mention here after the following. Many of the young people, the young historians today, they believe that all the Germans were firmly behind Hitler, and that is just not the case. It is absolutely not the case. It is true that many people were jubilated when Hitler came to speak, and one sees that on the pictures today and old reports. But these are 2,000 people, and Germany is 85 million people. So that was not all the Germans, first. And second, there is a very important point: a person anywhere, in

Germany or in America or in another country, has a country and he has a political leader. It's very natural. And it is by no means so that everybody combines the two and feels the same loyalty to the country as he does to the leader. See? So many people in Germany were very much in favor of their country, loved their country and were ready to sacrifice for it, but they didn't want to do the same for Hitler. Just think of what we do today. There are many Americans who love American and are good citizens, but do not like [William] Clinton or [George] Bush or [Ronald] Reagan or somebody.

KELLY: That's right. I think that's very well put. I really do. It's a very interesting perspective.

STUHLINGER: And that was the same in Germany. And to say now, today, that all Germans were behind Hitler and were guilty of all the mess and tragedy is simply not true. But the individual has no choice when there is war. One cannot say, "Well, I'll do all for the country, but I'll do nothing for Hitler." It's not so easy. Anyway, we could talk very long about that.

Anyway, as a result of the contact between von Braun's group and the American Allies, as a result of that, there was in the vocation of von Braun and some of his people by an advanced group of Americans who came over at an early time into Germany to look for German scientists, there was this Action ALSOS. You may have heard of it. A-L-S-O-S. I cannot tell anymore what the acronym stands for, but it was a group of civilians, scientists mainly, who contacted Germans and found out what kind of scientists and engineers and technically active people were there that might be of interest for America.

We met some of them later on, for example, Dick Porter, Richard Porter. You may have heard of him. He was a GE man, a very capable engineer for GE and a high-ranking member of the General Electric staff. He came over and contacted us, and we became good friends, dear friends later on for years, for many years in this country. So that was immediately – it was a little guarded, but it was a friendly relationship, a genuine relationship.

And then there was particularly General [Holger N.] Toftoy. He was a colonel at that time. You may have heard of General Toftoy. He was a colonel at that time, and he was given the order from his superior to come over and also try to put together a group of the Peenemunde people whom one could take over, and should take over, to America for rocket work. Toftoy was a very fine person. We became very close friends later on here in this country. To his end he was always very good and nice to us Germans. He was – well, what should I say? We called him our father in America. He was about that kind of a person. He wanted to bring over 500 people to America, but his people in Washington said, "There's no way. You get a few, but not 500." Then finally they settled on 100, and then Toftoy counted them, and when he counted 127, then Toftoy said, "For heaven's sake, I was always bad in mathematics. I'm sorry." [Laughter] But 127 came over.

In the first group – there were five shipments. The first group were von Braun and a few that came over in September '45, shortly after the end of the war, and the others came later. I was in the third shipment. I came over in February '46. And the last shipment came over in summer of '46. Then we came to Texas and lived in Fort Bliss, in Army barracks, former Army barracks. At that time, we didn't have much to do. The American Government did not really see to it that we would have a plan or possibility to really go to work and build rockets or something. Our commanding officer was Major [James P.] Hamill. He said at the time, "Well, we'll put you on ice. We may need you later on." That was the official wording.

We used the time to work for ourselves. Fortunately I learned mathematics and guidance theory and things like that. I also began a study at that time which became a relatively major part of my work. It was electrically propelled spacecraft, electric propulsion systems, high propulsion systems. I don't know if you have heard of that before. Anyway, I began starting on that subject at the time, and through the years, until very recently, I've spent again and again much time on it. I still believe in it. One of these days I hope to fly to Mars electrically.

KELLY: There you go. What types of projects did you work on in the electrical propulsion system area while you were at Fort Bliss?

STUHLINGER: I did only theoretical work. Professor [Hermann] Oberth – is Professor Oberth known to you?

KELLY: Yes.

STUHLINGER: He spoke in 1923 that one could build rocket engines which work on the principle of electric fields and electric forces rather than chemical forces. Von Braun asked me one day in 1947 in Fort Bliss to look into Oberth's claims and see whether one could make a workable system out of it. That's how it began for me, and I did look into it and contributed a number of new ideas. I also wrote a book about ion propulsion. Would you like to see it? I have it here. I can show it to you.

KELLY: This is the Professor Oberth that inspired you as a child, too, isn't it?

STUHLINGER: Yes. Same person. The main work that we did in Fort Bliss was to launch the V-2 rockets which had been brought over from the production line in Germany. There were altogether about 100 V-2 rockets or A-4 rockets in parts. They were brought over here, first by railroad, then by ship to New Orleans, and then by truck from New Orleans to White Sands, and it is unbelievable today for a rocket engineer, these fine parts which were made in clean rooms in Peenemunde, and precision parts, they were unloaded and put in the center of the desert in Fort Bliss, and they were just lying there in the sand storms. [Laughter] So we had the task of putting these together and making workable rockets out of them again.

KELLY: That must have been fantastic.

STUHLINGER: Many of the rockets were – there were parts of about 100, but they were not quite complete. Some parts had to be manufactured again by General Electric to make a rocket complete. Anyway, we launched about, I think, seventy-two rockets, and in spite of the rough treatment of these rocket parts, the long time since their manufacturing and the long trip from Bittlof [phonetic], Germany, to the White Sands Proving Grounds [New Mexico], in spite of all of that, about 65 or 70 percent of the rockets worked very well.

KELLY: Wow. Even despite the fact that they traveled all the way from Germany.

STUHLINGER: Yes, had spent some time lying in the desert sand in the open.

KELLY: That's remarkable.

STUHLINGER: The rockets were used to launch scientific instruments. That was a very important and very useful application of them. The Army, of course, wanted to learn how to launch rockets and how they would function and what one has to do to launch them, how many people would be necessary, what ground equipment one would need, but there was a group of scientists who had joined together to have a program of instruments flown to high altitudes. That became a very important and very rich program, rich in new results and new knowledge of the upper atmosphere and of the space beyond the atmosphere. It was a very rich program. It's very interesting to realize that the military application of rockets, or that rocket, or our rocket in Germany, lasted from September '44 to March '45, February or March '45. That's about – well, a few months. The use of the same rocket for scientific purposes in White Sands covered seven years.

KELLY: Wow. That's spectacular in contrast.

STUHLINGER: A number of very interesting things of the higher atmosphere were discovered with these flights, including, for example, the first X-rays from the sun were found by [Herbert] Friedman from the V-2 rocket. A number of people, including [James A.] Van Allen, found out the distribution of cosmic rays in high altitudes, and the temperature gradient up in high altitudes and pressure and composition of the atmosphere and things like that were discovered at that time. It was a good program.

KELLY: What did you do with Dr. Van Allen? It was about that time that you started working with him, right?

STUHLINGER: Yes, but I should begin a little earlier. When I was studying in Tuebingen, physics, I was working in cosmic rays. That was my subject for my thesis. I read the literature, and the literature which I read in 1933 or '34, there was an article by Van Allen. I read it and liked it and used it in my own work and considered him, even though I didn't know him, as a colleague. And sure enough, when we launched our rockets in White Sands, there was Van Allen, and he put a little Geiger counter on the [unclear]. So we came to talking together, of course.

KELLY: Because you had worked with Dr. [Hans] Geiger.

STUHLINGER: I had worked with Dr. Geiger, and Van Allen had worked here in this country, with—I don't recall with whom he had worked. Anyway, we were, so to speak, colleagues. On the other hand, not much contact developed at that time. There was a ruling from some upper organization that there should be no fraternization, and it was also maybe not a strict law, but some people felt like, well, those guys, we were in war and they were our enemies and we cannot fraternize now. So we were a little bit at a distance, unfortunately.

I met some of the great people at that time, Friedman, for example, who became head of the Academy of Sciences, and Van Allen, who became world-famous, and many others. We were together and met each other, but it was always under a little distance, unfortunately. But that's war.

Then we came to Huntsville in 1950. The reason why we came to Huntsville is also described in here. It was the time when the Korean War began, '49. At that time, the Army hospital in El Paso [Texas], in whose barracks we were living, wanted the barracks back for hospital purposes in expectation of war, the casualties and wounded people from the East they would have to treat. So we had to look for different housing somewhere.

Our boss in Washington was this General Toftoy. He traveled around in the country and found eventually this place here in Huntsville. In Huntsville there was a chemical factory, a chemical war factory. In real language, it was a poison gas factory, which one doesn't like to mention it today, but that's what it was. Anyway, the factory was discontinued when the war ended, and there were many buildings empty. Also, there was a huge area because when they manufactured poison gas, they wanted to have the buildings far apart in case some leakage happens. So we had a lot of area, twenty miles this way and six or eight miles this way and empty buildings in it, and Toftoy managed to get this for his people. It's a little bit more involved and more difficult, but that's also in the book, but that's, for the time, sufficient.

Anyway, Toftoy and von Braun moved here. Toftoy was in Washington, but von Braun moved here. At that time his group had grown to about 400 people or 500 people. Besides the original Germans, there were a number of Americans who had joined us, younger people and middle-aged people. We had a large mixed group already.

We began, then, here in Huntsville. The first thing we did is we converted an old Army hospital in a building that would have offices and laboratories and such things. The building is still standing today. That's where we moved into some barracks and began to work.

At that time, actually in the fall of '49 already, it was realized that there may be a war with Korea developing. The armed forces wanted a rocket as one of the weapons for that war and turned to von Braun and said, "Please build us a rocket as quickly as you can. Build it on the basis of your old A-4, because you know how to do that, modernize it, but build it as soon as you can." von Braun got this order in the fall of '49, then early in '50 we moved here, and we began to work very intensely immediately.

In '53, 1953, the rocket was ready. It was launched and worked. It was the Redstone. The Redstone rocket was built in that way. That's how we began here in Huntsville. Again, I'll give you a little write-up about our beginning in Huntsville. And there's a chapter in here also.

Later on, the Army wanted a missile with a longer range. The Redstone had a range of about 250 miles, and they wanted 1,500 miles. So we built the Jupiter rocket. You have certainly heard of the Jupiter.

KELLY: Yes.

STUHLINGER: Have you seen the museum here?

KELLY: I haven't, unfortunately.

STUHLINGER: No? Well, all these rockets are on display there. In Houston they have a V-2 at the moment.

KELLY: That's right. I've seen it.

STUHLINGER: Have you seen it? Actually, it was our V-2, and it had to be repaired because it began to rust. So it was rejuvenated, and Houston said, "Let us have it for three months, then we'll send it back to you." And they have it now.

KELLY: I've seen it. It's very impressive.

STUHLINGER: So the Redstone is here. The Jupiter is here. You should see the museum. It's worth seeing. We natives are quite proud of the museum, and we say it's the finest and largest rocket museum not only in the country, but in the solar system, which is easy to claim. [Laughter]

KELLY: There you go. Did you anticipate when you were developing the Redstone rocket that it would be used for purposes of man's, or human, space flight?

STUHLINGER: The Redstone?

KELLY: Yes.

STUHLINGER: Well, again it is in here. The answer is yes, and it's one of the anecdotes. The Redstone flew in '53 the first time, and even before that, in about '52, von Braun and I met each other in the hallway one day, and just in passing, he said to me, "With the Redstone we can do it."

I was dumb enough. I said, "Do what?"

He said, "Launch a satellite, of course." [Laughter]

I said, "Oh, that's very good, very fine, very interesting." And then he described to me just – and he said how he would do it: put three solid stages on top of it, spin them so that they would be controlled, and then go up high and then get horizontal and shoot out the third rocket into orbit, and one can do it. He said it would not be a very elegant way to put a ten-pound or fifteen-pound satellite into orbit with such a monster like the Redstone, but it's our only way how we can do it soon. There is no other way. And he added, as far-sighted as he was, he said, "If you don't do it somehow like this. Then the Russians will do it before we do it."

Well, but then a very up-and-down history began for the first satellite. That's again in here. I wouldn't like to go into it. It takes too long. It's also in the other article I gave you about Sputnik. It's also described briefly in there and at length in here.

KELLY: Thank you. Okay.

STUHLINGER: Well, anyway, the Explorer was launched in '58. It was a very dramatic story, the whole thing, very, very suspenseful and full of hopes and frustrations, all of that, and it would be not right if I began to tell about it now. It would take until midnight also. You can read much of it in here. That was the Explorer, and then the Jupiter also sent – that's heavier and stronger – sent up satellites and then, of course, other people sent up satellites and it became not only a nationwide, but a worldwide undertaking now. And if you just imagine that we have right now at any time, I think about 2,000 – I'm not quite sure – satellites in orbit, different ones.

KELLY: It seems incredible.

STUHLINGER: Russian, English, Indian, Japanese, Chinese, European, and, of course, American. It's incredible.

KELLY: And it makes you realize how big the Earth is, too.

STUHLINGER: Yes. Oh, yes.

KELLY: To understand that none of them are running into each other or geosynchronous –

STUHLINGER: You wanted to know about Skylab and the space station?

KELLY: Yes, and perhaps shuttle. You worked on the Saturn as well.

STUHLINGER: Shuttle and Saturn.

KELLY: There are so many things that you've done.

STUHLINGER: Many of your questions are what is my work in there.

KELLY: That's right.

STUHLINGER: I should make a few general remarks here first.

KELLY: Okay.

STUHLINGER: My personal work in Peenemunde at first was a guidance system. You know, probably, roughly at least, what a guidance system is and what it must do.

KELLY: Yes.

STUHLINGER: My particular work was in accelerometers and integrators. An accelerometer measures the acceleration. It integrates the acceleration once to get the velocity and twice to get the distance. Can you follow that?

KELLY: Yes, I can.

STUHLINGER: And if you do that on a flying rocket, that instrument can tell you at any moment not only how the acceleration is, but how the velocity is and what distance it has traveled. That is a so-called inertial system. Inertial means that it is done on board, without connection to the ground, which is very important because it cannot be disturbed by radio means. That was my work in Peenemunde and in Fort Bliss to some extent also.

Now, in Huntsville, I began to change over to some extent in the following way. I have to begin at a more general viewpoint. The space flight is done primarily to explore the space, to explore the world around our Earth, which means that we engineers who build the rocket have to have a strong connection with the scientists who know how to explore, what to look after, and what to do with the results which we gain from there. This duality is a very important point if one wants to understand the whole space program: the engineers on one side and the scientists on the other side, the engineers who build the machines and the scientists who use them. The end result would be better knowledge of our Earth, which is very important, of course, to have and to use, to increase also.

My function, particularly beginning here in Huntsville, was about the following. Basically I wanted to establish contact between us engineers and the scientists on the outside. In doing that, my work would be something like that of a two-way ambassador. That means he goes out from the engineers and tells the scientists what the engineers can do, and then he comes back and tells the engineers what the scientists want. You see? And that is very important in order to establish and maintain that very necessary interaction between the scientists and the engineering, which is very important. I would say it's almost a vital necessity to do that.

If there's no reason and no purpose for the rockets to use, then why should they be built? And if the scientists don't have a vehicle to go out in space, they can estimate and speculate forever without knowing how it really looks like out there. So that was my function.

Now, there are some people that describe that position a little bit more differently and more realistically, unfortunately sometimes. They say, well, when a person like that goes to the scientists, they say, "Oh, well, he's an engineer. He knows nothing." And when he comes back from the scientists to the engineers and want to tell them something, they say, "Oh, he's one of those scientists. They are dumb however you look at them." So it's very difficult to make a real productive interplay and interaction. There's a lot of diplomacy one has to utilize here, and knowledge, too, of course. One should know what one talks about, can talk both languages, more or less.

KELLY: It seems you were uniquely suited for that with you background in both engineering and –

STUHLINGER: Well, it helped, of course. It helped. I tried just to do that. That was my main effort. In addition to that, I did a few more realistic things. I developed the idea of electric propulsion, made a number of publications and talks all over. And another one was to prepare Skylab, and that was a very good example of what I just said of this double, or two-way, ambassador. There were some people who said we should do something with our rockets to build up some interest in the outside world, of the scientists in what we are doing. And there were some scientists who said, "Well, we would love to use your rockets if we knew how to do it."

I had to get into action, and I made many visits to the scientists. I worked on their committees and met them and visited them and telephoned and so on. Here in this book there is a list of all the scientists, about fifty-six, for the Skylab, and I had established contact with all of them and told them what they can expect and how they should prepare their instruments coming back, and I told my colleagues here how to build the facilities, the stabilized platform and all kinds of things so that the scientists can use it. This is a function which is overlooked by many people who don't really see and don't believe that it is necessary, but it is necessary. It's a very important function one has to do. Well, anyway, that was my main function.

One exception was, for example, the Skylab. It began actually – I would say this, Professor Oberth died in '23. In his book he said one of the things we can do from orbit, from a satellite, is have telescopes there and look at the universe with an optical capability which would be unobtainable from Earth. Are you versed in astronomy and optics to some extent? There are three or four main factors which are very different between observing from Earth and observing in orbit. For example, we had no atmosphere to disturb the vision. The atmosphere around us does not allow us to have a resolution of images of better than about half an hour a second. For an amateur photographer it's good, but for an astronomer it's not enough. They want more.

Second, ultraviolet rays do not come through here, so we cannot observe on the Earth the UV, the ultraviolet, of emissions of stars. Another one is that the atmosphere introduces a jitter because of variations in density of the path of the light through the atmosphere. Therefore, one cannot expect pictures or images better than a certain capability, resolution, and wavelength limitation also. Anyway, Oberth pointed that out in 1923.

So, around '55 or so, some astronomer said, "Well, couldn't we do something to put a telescope on one of your machines and go up in orbit?" Then there was a proposal, and it's shown in that picture I'll show you here. Some optimists said, "Let us launch one of your Saturn capsules, command service module, and put a telescope on it." He had two telescopes.

KELLY: So that was the early design of the satellite.

STUHLINGER: It was the early design. Now, that was the beginning. When I started my work here in Huntsville, I had talked to von Braun and to others, too, and said, "This is something that we should do more of it and it looks beautiful and very promising, and we can really do something for science. When the scientists are happy with us, that gives us a lot of firm ground to extend on with our rocket work."

Then a long process of planning began. Fortunately, there were other people. Also he said, "Well, that's fine, but we should do more. That's too modest. We want to do this also and this also and this also." So a number of committees were formed with these people here, with these scientists. They had interest in the most different subjects – astronomy, but also actions of weightlessness and materials, processes, things of that kind, observing of the atmosphere.

KELLY: Some microgravity research.

STUHLINGER: Microgravity work and effects on astronauts and living beings. Anyway, we soon had a long list of things that should be done, and the system grew lots and lots and lots, and that service module was no longer sufficient.

And then here came then an idea to me that von Braun had from the very beginning. Well, I should say the very beginning, that means from about 1950 on. He said, "We should, of course, build a livable station in orbit as soon as we can. However," he said, "the best way to do it without making it too expensive and too difficult and involved, let us take an empty tank of one of the rockets which we launch up there and let us make a habitat out of the tank, the empty tank." That's a natural and simple idea and, typical of von Braun, it was one that could be made to work. So he said, "Let's do that."

Again, I must be careful not to go into detail. You're running out of time otherwise. The first idea that von Braun had, and he had to make it very cheap so that it wouldn't cost too much, he said, "Let us launch a two-stage Saturn," the big one, not the Saturn V, but the Saturn 1. The second stage had a hydrogen tank, an oxygen tank, and it was sitting on the first stage. It was used, needed, to burn the second engine for the second stage so they can reach orbit. Then the idea was that when the tank is empty after having reached orbit, one could go up there with another flight, astronauts in their garbs, in their suits, could go in and just clean it out and put things on the walls and all of that.

It was the idea of the so-called "wet workshop." Wet means because it would be filled with liquid hydrogen when it goes up, and then after the hydrogen was used up, it would be dried out, and then one could install it and establish housing units in there and instruments and all kinds of facilities. That was the wet workshop, and it had the advantage that one would not have to launch an extra container. You know, the container would be there with the tank. But one would only have to go up with the service module later on and take new parts and put them in those instruments. This was, for those who would have to build it, a nightmare. The extravehicular activities are possible, but a person in the suit and with an oxygen bottle on his back is not too agile in building up the instrument and all that. So the idea of the wet workshop was a kind of a sales – I wouldn't say a sales gimmick, but it was to make it easier to sell the idea.

On the other hand, von Braun and his co-workers who worked on that system, they had always in the back of their mind, "Well, when we are close to it, we will change it into a dry workshop." A dry workshop would be the following. It would be, again, a rocket, by this time now a Saturn V rocket, the big one, the real big one, and it had the two stages, one, II stage which will burn, and then it would have a third stage, and the third stage would be the tank of the second stage again, but without fuel. It would not be used as fuel. It would be equipped completely on the ground. It would be a complete workshop from the ground on. One could install it and build it up on the ground under comfortable conditions. It would be launched by the Saturn V, so we had enough thrust to put it up there, and then we would have it right there from the beginning to be occupied and to work in it. That was the so-called "dry workshop." Now, the dry workshop was more expensive, of course, than the wet workshop, and people in Congress, they only look at the cost. They don't look at the possibility to build or not to build it, and they don't know the problems the engineers would have had.

However, something important happened. In the meantime, our lunar project proceeded, and we made good progress. It worked. You know, we had the men on the moon and coming back, and the Congress was jubilant about the new image that the American people and country and government had for the outside world. Our boss in Washington at that time, that was George [E.] Mueller. You remember his name, I'm sure.

KELLY: Yes.

STUHLINGER: Maybe you know him personally, George Mueller. He used that moment when the Congress was so happy and so up in the seventh cloud, almost, and he said, "Gentlemen, we have come to the conclusion that we should build a dry workshop." It was accepted. [Laughter] And so we built a dry workshop, and the dry workshop was what we know today as the Skylab, and it was a beautiful success altogether.

KELLY: It sure was.

STUHLINGER: We had a mishap on the launching, as you probably know. There was this –

KELLY: Right.

STUHLINGER: And for us inside, that was a very tragic situation because it could have been prevented so easily and a few things were just not done right by some people, but it's of no use to say afterwards when something like that happened, "It's your fault," and so on.

KELLY: That's right. So how was the problem fixed? Did you work with other centers in fixing—? It was the meteorite shield, I believe, that didn't deploy correctly?

STUHLINGER: Yes. It was the following. Here is the tank itself, the Skylab inside, and then there was a meteorite shield out here.

KELLY: Okay. On the outside, around the –

STUHLINGER: ..At that time, before we had done that, von Braun told me one day we should know more about the meteorites in orbit. How dangerous are they for our space station? Do we have to expect hits and damage? How many are there, and how big are they? And he asked me to look into that with my little group of co-workers. We did, and we found out that one just doesn't know. There had been a few experiments, but they were contradicting, and we found out why they were contradicting and they were no good, so we decided we would just have to make a new measurement, and we made the new measurement by building a system which is called Pegasus. We have a Pegasus 2 now that's something different, but that Pegasus was a winged system with huge wings, space capsule, and these wings were only sensors for meteorites.

The sensor was the following. It was a thin layer of metal, then an insulating layer, and another layer of metal. When a meteorite hit here, it made a little hole here, and that hit was such a hit, produced a lot of temperature. You know, it's a very fast particle that hit, so there's a little, like an explosion for a moment, and that explosion goes through here, and that explosion makes a conducting path because of the hot ionized gases which are – for a very short moment. Now, if you have a condenser here connected to this one here, and this is charged up by a power supply here, kept on charge, but you have a condenser, and there is this short here, then the condenser discharges through for a very short moment, half a microsecond or so.

When you have an instrument in here, you can measure the time and the amount of current, of discharge, that flows over here, and then after a very short time, it is over and there is a hole but there's no further discharge. So the instrument is at rest again. So when you look at the record of the instrument, it looks like this, and all of a sudden this here, and that means a meteorite has hit. When there is a big one, then there's a big discharge, and that looks like this. When there's a small one, it looks like this here. So from the size we can estimate how big or how fast, or both, the meteorite was. And we built that thing.

It was a huge system. You can see pictures of it in some places. I think it's in here, "like a two-story building," it says. We flew it on the Saturn, and it worked beautifully. That was many years ago. I even don't remember when – in the fifties. So, forty years ago. Even today it's still the best measurement of meteorites in orbit that we have. It has not been improved, the data which we got. And we found out that there are not many meteorites.

The problem that our Skylab would be hit by a meteorite was minimal, a certain percent, maybe one dangerous hit in a thousand years or something like that. I then said to von Braun, "Let's forget about this meteorite shield. It is a strange contraption here anyway, and we don't need it, and we have proven that we don't need it, so forget it."

Now, von Braun made a decision which he later on regretted, and it was a decision which he made because he was at the time it was towards the end of the sixties already when Skylab was built, and Braun was already thinking of his time and life in Washington, not so much here. "Oh, they have built it already, so let's keep it on." It was kept on, and sure enough, when the air began to stream by during the ascent of the rocket, it tore open here and tore that housing off and tore this thing off here and also ripped off one of the solar cells and the heat protection of the Skylab, and then it was, of course, a disaster, at least a mild disaster.

KELLY: Although it was fixed.

STUHLINGER: It was fixed.

KELLY: Can you tell me a little bit about that?

STUHLINGER: It was a dramatic kind of fix. The people who did it have to be admired, certainly. There was a little hole here in the white for observations, with an airlock to it. So they here in Huntsville built a kind of umbrella, and that umbrella reached through here and then unfolded here so that it would spread out and would protect that Skylab against heat.

That was the problem, the main problem, the heat, because that meteorite shield was also a heat shield at the same time. So the umbrella was a shade for the Skylab, and that brought the temperature down again. [Phone rings. Tape recorder turned off.]

... quoted, and many times wrongly quoted. It's a question of how do we go to the moon, with Earth orbit rendezvous or lunar rendezvous or direct? And that is a story which is very clear and well known to us old-timers, but not to the new-timers, and the new-timers often say, "Well, that's where von Braun was wrong, but he was told the right way by the people in Houston, and then he had to agree." But the truth is quite different and that is worthwhile telling the right way.

KELLY: Can we get that on tape? Whenever you're ready.

STUHLINGER: Yes. You know, probably, the story of the different ways of how to go to the moon.

KELLY: That's right.

STUHLINGER: The one is to go up there into orbit and then have a rendezvous here and go on from here and go to the moon and go down directly and then come back like so.

KELLY: And that's the Earth-orbit rendezvous.

STUHLINGER: That's the Earth-orbit rendezvous. And then there's this other one, which is go from here into orbit but then a little later go out to the moon, go down and up again, and then, from here, back to Earth. That's the lunar-orbit rendezvous. And then the third one is a direct mode, what goes from here – here's the moon – direct down and up again and so on.

KELLY: And that's the direct ascent.

STUHLINGER: Yes. Direct. Now, von Braun was at first for this Earth-orbit rendezvous, and he said we will have to have two launchings of two Saturn Vs – this one, and that's the second one, Saturn V – and then we can do it this way. Von Braun had three reasons for doing that. First, he said, if we do it that way, we will develop the orbital rendezvous method for this project, and that will be a milestone for the space flight to come, for many, many other applications. For example, when we have the space station, we need a number of rendezvous operations in Earth orbit. We have to develop that, how we go there, how we transfer from one vehicle into the other one, and things like that.

KELLY: Was this prior to the time that rendezvous was perfected?

STUHLINGER: Well, there had been rendezvous with the Gemini, you may remember. So it was perfected, but there was still much to develop, how to transfer heavy loads and things like that. So, von Braun said let us do it this way, then we have this development, here, so to speak, as a side product of our lunar project. It will help us to get into the future, and we will need it. We'll need it anyway, no matter what comes. When we go to Mars later on, we'll need an Earth rendezvous.

The second reason, he said we have to go to the moon's surface on the rear side of the moon. See, that the rear side from the Earth, we will not have a direct connection by radio, and all of that must be done by computer, by programmed computer. We have no idea how it worked. We have no contact. If something mishaps, they cannot talk to us, we cannot talk to them. We don't know what happens. We only find out later on that there's no further word from the moon, so something must have failed. So let's not do that. That was the second major reason.

Third, he said when we do that, we have to use the Saturn V and we have that already, so we do not have to develop a new vehicle here. Now, the other way was this one here, and that was the proposal by this fellow. [Pauses] I'm sure that you know the name. That is a matter of old age, you know, that names don't come back.

KELLY: Is it John [C.] Houbolt? I'm not sure how to pronounce his name.

STUHLINGER: Right. And he said, "Let's do it this way." He said, if you do it this way, then we do not have to take the return vehicle down to the surface and up again, as we have to do it here. So we save this effort and propellant to go down and up again. So let's do it this way. Also, we can do it relatively easily from here with one Saturn V. We can do it this way so that it's much easier.

Von Braun said, well, that's all right, but first we do not have this opportunity of developing something which we need for later uses. And second, he said, if we do it this way, then we have to make this maneuver, which is not an easy one, and this, which is not an easy one, on the rear side of the moon. Again, in this case it would be relatively simple because we just go up and just land there and then go up again. But here we have to make maneuvers and actions and all of it out of sight, and we should avoid that.

The one man who supported this idea very much was [Dr. Robert R.] Gilruth and [Maxime A.] Faget. They said, "Well, we cannot do that." At that time it was in the mid-fifties. Then what Gilruth said was, "Tell those guys in Marshall they should develop a better rocket. They should develop the Nova, the Nova rocket, which would be a multiple of Saturn V. And they should do it. They always talk about their rocket developments. Let's show them what they can do. Let's do it, and we'll do it this way. That's simple and direct, and, of course, we need a bigger thing." It would be about five times as big as the Saturn V.

But the Houston people said, "That's no sweat for those guys in Marshall. They should do it, and then we can do it this way. Very simple."

Now, this project was lost relatively quickly because it was simply obvious that one could not build a Nova and still make the lunar landing in the decade as the President [John F. Kennedy] had promised. So this had become immaterial and was no longer pursued.

KELLY: Is that because there was too much development that needed to be done?

STUHLINGER: Right, and too expensive, and too much time particularly. Just to develop a system like that and to try to test it out would take a few years. Here we had the individual parts already. So at that time there was, for some time, an open situation, so to speak. Nobody was very firm. Von Braun said, "What we should do is study the two systems further before we made a decision." It was not so that von Braun said that was nonsense and we have to do it this way. He said, "Let's study it further."

Gilruth and Faget, after this had been discontinued, came to this one here, but Gilruth still said, "Well, I feel very uneasy because of these activities behind the moon when we rely only on the computer. We should not do that."

KELLY: This is lunar orbit rendezvous that they were in favor of.

STUHLINGER: Yes. But then, at one time there came IBM, and IBM had worked hard and had developed better computers and more reliable computers, and they convinced Gilruth that this can be done with the computers they had. And that was the actual decisive moment and point in the whole controversy. Many people don't see that and don't know that and don't mention that. They simply say von Braun was – that his mind blocked somehow and was stupid and sitting on this one. This was the ideal situation. But the point that really gave the go-ahead for this system was the development of the good computers.

Gilruth one time came to von Braun and said, "I have now confidence in the computers that we can do that, and I'm no longer hesitant to go into that operation." And in a moment von Braun accepted this and backed Gilruth up and said, "Yes, let's do it this way."

Now, von Braun still had to convince his own people. That's also described in here. And I remember that very well. It was a very dramatic time in Marshall Center. One morning he came to our Friday meeting – every Friday we had a big get-together of the staff people – and said, "Gentlemen, I have now come to the conclusion that we should go LOR."

And his people, some of this people, went up in arms and said, "What? What? You cannot do that. We have always been fighting for this one, and you yourself said that this was the best thing because of this here and because of this here."

And von Braun just listened quietly. And then he said, "Now, gentlemen, let me tell you. First of all, Gilruth has accepted this here. So it seems no sweat to do the maneuvers on the rear side of the moon. Second, when we do it this way, we have a chance to do it in this decade and to fulfill the promise which we gave to the President. If we do it this way, we will not be able to do it before the decade is out."

I should mention another point here. Let me see. Well, that was about the situation. So von Braun said, "Let us do it this way. First of all, Gilruth has accepted this. It's no longer a problem for this capability and this new development of orbital maneuvering in orbit, orbital operations. We just have to drop that for the moment. That wish and that hope cannot be done at this time." And furthermore, von Braun said, "Gentlemen, we have now spent a full year just talking about this, and we have made no progress. The situation is still the same. The ideas are the same, with the exception of the IBM, which has made a great progress here. If we do not go ahead and stop talking now and start doing, we will never do it in this decade." And then he convinced everybody of his people, and we were firmly behind him.

Von Braun went to [James E.] Webb, you know, Mr. Webb, and said, "Mr. Webb, I'm now for this one here. Let's do it this way. That's the only way how we can hope to fulfill our promise. Let's do it this way. Then we have a good chance of beating the Russians and of doing it and being the first ones to land on the moon." And Webb still over a year to think it over and talk it over, and some people in Washington, for example, even – I think it was Dale [D.] Myers, some of the high-powered people, were still for this one, wanted to back von Braun, and von Braun said, "No, don't back me. I'm no longer for this one. I have found out that is the right one."

So von Braun went to Gilruth and said, "Bob, I'm all for it. Let's do it this way." And so it was that the air was cleared again and the go-ahead was possible, the green light was there, and things developed. But then there were people who said, "Well, von Braun was so stupid he relied upon this one here and he just didn't want to see the light of the day, but we convinced him," which was just not right.

KELLY: Used it for their own purposes. Now, what about the Earth-orbit rendezvous made it so difficult that it would have taken too long to develop, until the end of the decade?

STUHLINGER: Miss Kelly, if we had done it from the beginning and not spent a full year in talking about it, we could have done it, but not at the time. When the decision finally had to be made, it was too late then.

KELLY: How come?

STUHLINGER: It was two Saturns, and they would have to rendezvous up there, and that was something that must be developed, you know, the guidance systems and how do they get together close enough so that pieces and tank fillings and so on can be transferred. It must be developed. It would have taken time, and it was just too late at that time. So, that's about it.

KELLY: It's a very interesting perspective, and I'm glad that –

STUHLINGER: It's also in here.

KELLY: Great. It's in the book as well. Who were the IBM computer people who made the major computer advances?

STUHLINGER: One of them was a fellow who had then his company here, his own company. He left that company, but the company's still here. It is – it's old age. I'm so sorry.

KELLY: That's okay.

STUHLINGER: But he was the brain behind that computer work.

KELLY: Well, if you think of it some other time, let us know.

STUHLINGER: Yes. Let me write down what I should –

KELLY: I'd like to ask you about your work in the ALSEP [Apollo Lunar Surface Experiments Package] on the lunar surface.

STUHLINGER: I was not too deeply involved in the direct work for the Saturn-Apollo program, but at the beginning, when we started thinking of lunar missions, I was involved in several investigations. One of them was, for example, the question of how the lunar surface looks like, whether it is a deep layer of loose dust or whether it is solid or whether it's something in between. That has importance with respect to the vehicle we may eventually use on the moon, what kind of tires should it have, what wheels, how should it move forward, should it be built like a car for Earth or should it be like something else going through deep dust.

What we did in my laboratory was to compare a number of potential samples of lunar surface and to try out the bearing capability of several arrangements of dust and rock, things like that. Also we developed at that time a drill that could drill into rock and find out how the rocks look like in their center and also what the composition of rocks at greater depth may be. I was involved in such things, but not in the development of the Saturn, the big Saturn rocket itself. That was up to the specialists for rocket engines and rocket vehicles, spacecraft.

KELLY: And how did you test your experiment package? Were you able to test it on the lunar surface prior to its use?

STUHLINGER: Well, we, of course, tested very carefully all the individual sensors and radio links, control systems. All of the functions had to be subjected to – including low temperatures. That was done very carefully and with success. It worked quite well. Even after the astronauts had left the ALSEP worked, continued to work and to transmit data to Earth.

KELLY: And today?

STUHLINGER: Not today, no.

KELLY: But years after the lunar landings.

STUHLINGER: Some time afterwards.

KELLY: That's great. It sounded like it worked very well. Now, how were you able to determine the composition of the lunar surface prior to the astronauts arriving?

STUHLINGER: That could not be really determined. One could find out from landings of spacecraft that had been there before, unmanned systems. One could find out roughly what the surface could be, so we were quite confident that it was relatively solid. You may recall that the LM, which was eventually developed for the Saturn-Apollo, the LM, Lunar Excursion Module, had four legs, and each leg had a pad off of a metal plate, and we expected and hoped that these pads would be big enough to keep it from sinking into the ground, into dust ground. This hope was fully verified and fulfilled later on. The landing vehicles stood very firmly on the ground, so there was no problem.

KELLY: It would be very interesting to ask you about some of the letters I'm sure you received on some people who had concerns about the lunar surface. Did you receive any such letters of people who thought it was just an ocean of dust and that people would be sinking if they landed on the moon?

STUHLINGER: There were one famous prediction for that from Thomas Gold. Thomas Gold is a well-known geologist and astronomer, also, and a person who has been very active in space programs before, and he thought that there would be a deep layer of dust on the moon and there would even be a danger that the astronauts may sink in and never be seen again because they would drown in the sea of dust. But that was not the case.

KELLY: And were you able to test that along with the unmanned spacecraft that landed on the moon?

STUHLINGER: By the time we sent the Apollo up there, I believe that nobody was afraid of the astronauts meeting a sea of dust up there.

KELLY: Did you collaborate or work with or cooperate with any of the other centers on this project, and, if so, how did you do that?

STUHLINGER: There was certainly a lot of cooperation between centers directed from headquarters, from [NASA's] Washington headquarters. Houston, of course, was the most important element of the lunar program, together with the Marshall Center. The Marshall Center and the Houston Center, their responsibility was divided very clearly into two parts. The Marshall Center had to build and develop and operate the big Saturn V rocket that would go up from the Earth into Earth orbit and then leave the Earth orbit and send the capsule, the landing capsule service and command module and the lunar lander on its way to the moon, but as soon as that third stage of the Saturn had fulfilled that task of pushing it out quickly, it would cease to participate in the program. The capsule would go on by itself, and from then on it was purely Houston's responsibility.

It's very interesting to recall that the guidance system – of course, the guidance system was a very complex system for the entire vehicle – the guidance system for Saturn from the Earth into orbit and into the first beginning of the lunar transfer, that was Marshall's responsibility. The guidance system from then on, that means when the capsule had its high velocity to reach the moon, from this moment on to the moon and back to Earth, it was the responsibility of the MIT [Massachusetts Institute of Technology] that was the laboratory of Dr. [Charles Stark "Doc"] Draper, the Draper laboratory, and that was under Houston's responsibility, our responsibility.

This partition of the guidance problem worked very well. The two guidances had to be, of course, adapted to each other at the moment of transfer from the Saturn vehicle to the lunar transfer spacecraft, but even that connecting point between the two guidances worked very well, and our guidance systems worked very excellent.

KELLY: So is it analogous, then, from the launch being, I guess, under the supervision of Kennedy and then that being transferred to Mission Control in Houston after the vehicle was launched?

STUHLINGER: Yes.

KELLY: That's very interesting. I'd like to skip forward a little bit and past Skylab, because I know we already talked about it, and it's well documented, as you've shown us in your book, but I had watched an interview of you with Dr. Tarter [phonetic] that was done in 1984, and at that time you mentioned that you thought that there would be a few space stations orbiting the Earth –

STUHLINGER: Yes.

KELLY: – and perhaps two or three with even participation from both U.S. and Russia. Did you ever think at that time that there would be cooperation among all of those nations, including the U.S., to develop an international station?

STUHLINGER: I believe at that time only very few people, if at all, if there had been any at all, would have thought that there would be a real joint effort between Russia and this country to build one space station. When it finally was decided that it should be a joint operation and a joint venture, I believe that everybody in NASA kept a little bit his breath and said, "Well, we really hope that it goes well."

There are several reasons why we were hoping that things would turn out all right. The least reason was doubting the technical capability of the Russians. We did not doubt them. We were always impressed by the technical and scientific capability of the Russian colleagues for their own projects. Remember that they sent an unmanned probe to the moon many years ago, which picked up samples and brought them back to Earth. That's an enormous accomplishment, and those of us who were involved in space programs themselves, they had a lot of respect for that accomplishment, even though there were no people involved, but just to do it all by computer and by automated instruments, that that's really a great accomplishment.

We saw sometimes hardware that the Russians had built. We were always impressed by their capability as engineers and as technical people. They did beautiful work. I must admit that some of it was more – how should I say? More vigorous and more powerful than we would have done it. We would have done it lighter and not with brute force, as they did in some cases, but they did it, and they had a lot of very good successes. Not all of them were successful, of course, but many of them were, and we had a lot of respect for them.

So when we talked about a potential cooperation between Russia and America, we had sometimes our doubts, but we did not have doubts because of the technical capability of the Russians. The difficult things that come to mind with people involved in space programs are about the following. One is the fact that engineering practices are different in the two countries, even the measuring system. The Russians have metric. We are not metric here in this country, but that would be probably something that could be overcome.

Another point is that the management structure of a project like this in Russia and in America is different. How to build them together, it would be something possible, but it would take extra work and extra effort and extra time and money, probably.

And a third point which was heavily on our minds when we began to doubt a successful cooperation was the difference in the government structures of the two countries. For us it was very hard to see and to anticipate and predict how well the Russian Government, which was at that time still in a dictatorship, you remember, how well one could rely upon that government to live up to the promises and to go through development of that kind, even if some budget increases have to be applied once in a while. How would that be done by an independent government as they had in Russia as compared to our government, where one could always talk and negotiate? That was another point.

So we were not always out of concern how a cooperation like that would work. I believe even up to the present moment we are not quite without concern. There are still indications that there may be rough points. Another point is the following. If there is a problem, for example, of how to service a problem or how to do a certain part of a project, if the Russians are of one opinion and the American of the other opinion, who will decide? See, if Houston and Marshall disagreed on something, then there was always headquarters, Mr. Webb, to decide. And both of us, Houston and we, would believe Mr. Webb, and we felt being under his command, we were cooperative enough to accept what he said. Now, between Russia and America, who will be that man? Would he be a Russian or an American or something else?

KELLY: Right.

STUHLINGER: These are problems which cannot be left out of consideration entirely. It's problematic. It would, of course, be wonderful if a big project like, for example, going to

Mars, if that could be done jointly, but whether it could really be done smoothly is another question. We in our project, we have seen a number of rough spots in the cooperation between centers, not only between Houston and Marshall, but between other centers, and in this country the problems could eventually be solved because there was an authority in Washington. And the least point was that Washington controlled the money, you know, they could be always not giving the next budget installment if you don't cooperate. Now, between Russia and America, how would that be handled and how would that be done?

So it is a dream. The cooperation is a dream, a beautiful dream, and it would be wonderful. Whether it could be made to work, probably that must be shown. It may be too costly in money and time to try to do that. I don't know.

KELLY: Supposedly time will tell. You also mentioned Mars. Can you tell me a little bit about your thoughts on space travel to Mars?

STUHLINGER: In that direction I am biased. I'm very much for a manned trip to Mars. In fact, I wrote my first paper in which I described a mission to Mars with electric propulsion, by the way, in 1953 or '54, and I've believed in it ever since. I wrote a number of papers about going to Mars and how to do it, and I strongly believe in it.

I think we should go to Mars primarily to explore. We should find out whether we find traces of life there. I believe we will, but that must be proven. It's a belief; it's not knowledge. I think that nature is acting in a way that it does not develop life on one planet and nowhere else. I don't believe that. I believe there are forms of life, but we have to go there and find them, and I believe that one should not rely on robots only to find that. There should be people. A human mind is still so much superior to any robot when it comes to finding something new and to explore. It will take some time, a number of years before we can do that. It may happen sooner than we anticipate now. It may happen later. I don't know.

KELLY: Do you think that might work in the purview of the American space program, or do you think it should be a joint effort between nations, or do you think there are enough resources to –

STUHLINGER: If I could make that decision, I would simply say let America do it and let America invite other countries to contribute instruments, for example. Think of a number of our spacecraft which are in orbit now. Many of them have instruments built in England or in Germany or in Russia, even, or in Italy, and they are integrated on our spacecraft. And that's a good way to do it. That's how I would do a Mars mission.

KELLY: So you're primarily thinking of the shuttle spacecraft that has the different telescopes and various instruments on it that are being used.

STUHLINGER: Just on a Mars mission, going to Mars. Going to Mars. Now, the shuttle, of course, has also a number of instruments built by other nations, by other countries, and that's a method which works. I think it's a good one.

KELLY: I'd like to go back and ask you a little bit about the shuttle. I know you worked primarily with the Hubble Space Telescope. I don't want to take too much or your time. We could probably talk for weeks about it. Would you like to stop?

STUHLINGER: Oh, more than two and a half hours. [Laughter]

KELLY: Would you mention something about your work on the Hubble Space Telescope? And we can wrap things up very shortly.

STUHLINGER: The first mention of telescopes in orbit was made by Oberth, as I mentioned before. The idea of having telescopes on a spacecraft never were lost out of sight by those who planned for spacecraft. We mentioned the Skylab earlier, and we had five solar telescopes on Skylab. There were some other telescopes on other spacecraft, but then some astronomers soon came to express the idea that in order to really utilize the excellent situation in orbit for observing, one would should build a telescope with a diameter of three meters, and that would give a resolution that would really allow us to see the fine structure of all kinds of astronomical objects, particularly galaxies and nebulae and possibly the early stages of star formation, things like that. There were a few plans for such a telescope, and some of us here in Marshall made also plans for that, tried to find out how a three-meter telescope could be put on a satellite and how it could be launched, and in that effort I was deeply involved myself.

One of my activities at that time was also a part of that two-way ambassador situation which I described earlier. I went to a number of astronomers in the country and tried to interest them in a project of that kind. There was one astronomer, well known, that was Lyman Spitzer [Jr.] in Princeton. He was from the beginning of the satellite era, he was a very staunch supporter of a telescope in space. He not only opted for it, but he really made a campaign for a big telescope in orbit. He even went to the effort of proving in his own laboratory that the guidance system could be built to the accuracy demanded for such a telescope, and that's unusual for an astronomer who looks through a telescope to go into his basement and build up machines and gyroscopes and air bearings and things like that which can prove that a certain stability and accuracy of control can be obtained of a mechanical system. And he did. He showed that the angle accuracy, which is just almost out of our understanding how accurate that thing would be, I think about a hair's width at a mile distance, something like that – unbelievable – and he proved that it can be done, and he was a very strong supporter for the telescope, but not everybody was among the astronomers.

Some of them were – I met with – I did travel for thirteen astronomers in the country and talked to them about a big telescope. Some of them were all for it. For example, I don't know whether you know these names, Roger Angel [phonetic]. He is in Arizona. He is the one who makes these spin cast mirrors now. He was on my committee and an early supporter. And Arthur Code [phonetic] from Wisconsin. He was one of them. And then Dr. [Charles Robert] O'Dell. Do you know of him or his name? Bob O'Dell, an astronomer from New York, his observatory. They were strongly for it.

There were others who were not so strongly for it. For example, there was Dr. [Jesse] Greenstein. He was the old master of astronomy at that time, a very highly respected astronomer. I went to him also and it was a complete failure. He said, "Young man, how much does that cost, what you have in mind here?"

I said—well, at that time I was optimistic. I said, 180 million dollars. Do you know how much it cost at the end? About 2 billion dollars or so with repair work.

"And you see? That's what I mean. For that money we could build six P______."

And I said, "Yes, Professor Greenstein, that is certainly correct. However, all six of them together could not look that deeply into the universe as ours could do, all together could not look that deep into the ultraviolet, all together could not have this fine resolution and see the fine details of astronomy structures."

And he said, "We don't need that. We need more P______to look at, and that's all we need." And he virtually threw me out of his office. So I was disappointed, but that was it.

But then Spitzer was very much for it, and at one time that was very, very well preserved in my memory. We had a meeting here in Huntsville of all the astronomers, and my wife and I had invited them for the evening here to our house. We were out here on the lawn, that was in summertime, and I said to Dr. Spitzer, "Lyman, I think we should have an astronomer as the head of the whole effort. That would give it more credibility than if an engineer from the Marshall Center tried to be the head of it," and wouldn't he like to be that head.

He said, "No, I wouldn't like to do that, but I have a good proposal for you."

I said, "Who would be him?"

He said, "That young man over there," I stood next by. That was O'Dell, Bob O'Dell. And then the two of us went to O'Dell and said, "Bob, would you be interested in taking on the leadership of that project?"

And he said, "Well, let me think of it." And he thought of it, and he said yes, and he became the leader and director and head of the project for the first years, and that was very excellent, because he was an acknowledged astronomer. Astronomers and scientists respected him. They didn't respect engineers like us here, but they respected him, and that gave a big boost to the whole project.

But then it was a little – the estimates, that we were quite modest in our estimates for the cost. It would have become too expensive. So the money people in headquarters, they wanted to reduce the three meters, since that is too heavy and too expensive. Finally we came down to 2.4 meters, and 2.4 meters was the accepted diameter. That's what it has now.

Do you follow about what the Hubble is providing, the pictures? It's really mind-boggling, what you can see. Somebody said one new discovery per week. Can you imagine? It's a boost in astronomic knowledge which is unprecedented over the centuries, almost.

KELLY: Absolutely. You probably gathered more data from the beginning of the Hubble Space Scope until now than you did from the beginning of time until the launch.

STUHLINGER: Right. It's a beautiful instrument and very, very interesting.

KELLY: Absolutely. Well, I don't want to keep you much longer, but I would like to ask you just one more question, among many others, and I'd like to ask you what you think your most significant contributions were to the space program. What was most important to you?

STUHLINGER: Well, that's hard to tell. Also, you know, a man doesn't want very much to talk about his own accomplishments. I think, too, one is that I had the opportunity or the privilege, or the good luck of bringing scientists and rocket engineers together a number of times, a number of points in the program. I don't believe that many would acknowledge that now. You know how people are. "They know this was my idea. I didn't have to listen to those guys." That's how I'd respond. I wouldn't pay too much attention to that. And the other one is electric propulsion.

KELLY: You did a lot of significant work in that.

STUHLINGER: That has, unfortunately, not yet led to a good project. There's now one under way after many years of effort by JPL [Jet Propulsion Laboratory] they want to go to one of the asteroids with an ion propulsion system. I hope they will accomplish it and get through with it. The Russians are more active there. They use electric systems to control their satellites in orbit. You know, many of their satellites, particularly high-orbit satellites, stationary satellites, must be controlled carefully with small thrusts every once in a while to keep them in the right orbit, and electric systems are very useful for that, and the Russians do that. The Air Force does it, too, here, it has also done it, but NASA is somehow reluctant. The reason why they are so reluctant is probably one reason which is expressed by a common expression used in NASA; that is, the "NIH factor." You know what this is?

KELLY: No. What is it?

STUHLINGER: NIH means Not Invented Here. [Laughter] You know what I mean?

KELLY: Exactly.

STUHLINGER: You know, space people, among other things, are humans, with all their frailties and weaknesses.

KELLY: Absolutely, and sometimes students forget that.

STUHLINGER: Yes.

KELLY: Well, I want to thank you very much. We're very honored that you've been able to talk to us.

STUHLINGER: You're welcome.

[End of Interview]