Bumper 8

50th Anniversary of the First Launch on Cape Canaveral

Mr. Konrad Dannenberg's Oral History

Kennedy Space Center

Held on July 25, 2000

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Dynacs Engineering, Engineering Development Contract

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All redlines from all participants have been incorporated in this transcription as of March 15, 2001.

Roger Launuis: Ok well, Good morning! Could I ask you to just state your name and a little bit of background about yourself. Your birthplace, your education, things of that nature just for the record in terms of general information.

Konrad Dannenberg: I'm Konrad Dannenberg. I was born August 1912 in what used to be eastern Germany for a number of years. Of course when I was born there, it was still a part of the general German Republic at that time. It was Weissenfels, a relatively small city just south of Leipzig. At an early age, my parents moved to Hannover and I was really brought up in grade school and to the technical university in Hannover. In the year 1928, I think it was, a rocket pioneer, Max Valier came to our town and talked about a trip to Mars. And he also talked about the need that we have to develop rockets. Some work in that field was really going on at the time. A number of people used initially solid propellant rockets. Other early pioneers like Tsiolkovsky, Goddard, and Oberth published books which said for high performance missions we should really switch to liquids. Valier also finally switched to liquids and demonstrated them by using them on an automobile to demonstrate that you can really generate thrust. Because in those days, in the late 20's, there were many people, even professors in universities who did not believe that you can really generate thrust if you have no atmosphere, if you have nothing to push against. And of course, they completely misunderstood or did not know Isaac Newton's Law of Motion. If you know that and accept it, then of course it's perfectly clear that you can generate thrust. But it had to be demonstrated in those days. Fortunately probably even in the same year, on a relatively long railroad track close to Hannover in Burgwedel, Fritz Von Opel who had initially worked with Valier but the two had split at that time demonstrated with solid propellant rockets on a railroad car that you can obtain relatively high velocities. And of course, Fritz Von Opel was an automobile engineer, he wanted to set speed records and I even think, at same later tests he did that. He also eventually attached rockets to automobiles, to airplanes, to ice sleds, because we have a nice long area where you can demonstrate your testing and both

of them eventually switched over to liquid propellants. And that really interested me and some of my friends, Püellenberg was the head of our group at that time, on the end he was just my age, I think he was even a year younger than I was. But he was more active in the whole area. We built our own rockets. We again started with solid propellants because they are relatively easy to handle. They were not illegal, so you could buy small systems and you could remanufacture them into larger units. We did that using curtain rods as outer shell, so we built originally our curtain rod engines or motors. And eventually again after reading the book of Oberth at that time we of course switched to liquid propellant systems. Due to that fact, eventually Peenemünde became interested in me. Püellenberg, who I mentioned earlier, had been hired already by Captain Dornberger, of the German Army. So he was in Peenemünde before I was. Members of the group in Berlin, under Oberth initially and later on under Rudolf Nebel and Wernher von Braun. They perfected some pretty good early demonstrations of amateur liquid propelled rockets. The German army became interested in their work because the German army was not allowed to build heavy artillery. And they wanted to replace heavy artillery with more efficient rockets. Of course, they had been using solid propellant rockets all along but they wanted better performance, they wanted to have better ranges, and particularly the military chief at the time, Captain Dornberger, wanted to have a pin point target capability which you can not get with solids. And that was basically the reason for switching to liquid propellants. The army initially invited Wernher von Braun to join the group and Wernher eventually got some other people from the original Verein fúr Raumschiffahrt, the society for space travel.

Launius: The VFR?

Dannenberg: I don't even know if it is even generally too well known, when Wernher started, Kummersdorf at the artillery test range. It was really an arsenal, of course. The reason for that

is the Army wanted to develop rockets to replace the heavy artillery which they could not build and could not test. There was a very great mishap, three people were killed. Wernher was not in charge of the team at that time. The thought was for solid propellant rockets we mix our propellants already in the factory. Then we just have to have a proper mixture which would be ignited and the propellants burn properly and we get our performance. They wanted to do the same thing with liquid propellants because they thought it was easier if we only have one tank, one control system, one shut-off valve; so they pre-mixed it the propellants. I think they used hydrogen peroxide and probably gasoline or some aniline type fuel and put it in the tank. And of course, today we all know what would happen if you go ahead to ignite your pre-mixed propellant mixture. So they blew up not only the rocket engine but the entire test stand and as I said earlier three people were killed. The top man was Dr. Wahmke.

Launuis: What year did that take place?

Dannenberg: That must have been around '32 I would say. So in the very early 30's. - and I think this accident was one of the reasons that Wernher von Braun later on was always very, very careful - he always insisted on the one side that we test all the rockets before we launch them because that's the only way to find out what went wrong, what is the problem you still have to solve. Once you launch your rocket it normally gets so badly damaged that you can not find the cause for the mishap. And he also always stressed safety and security and doing the testing from a relatively safe bunker-type area. And I think this accident was one of the problems that he always considered. I myself never went to Kummersdorf. I joined the von Braun team later on only after they had started already to work in the mid 30's around '36 in Peenemünde. I got to Peenemünde in 1940 because I was initially in the German army. But, I was fortunately in a horse drawn company. I was never a good rider and I was thrown from my horse and the commander finally agreed that I was not really qualified as a technical engineer to

be in a horse drawn company. So he let me go and that way I got out of the army. I was put out of the army really by the VDO. An automobile tachometer manufacturer that I had joined before the war, I wanted to learn a little bit more about measuring capabilities and of course, building automobile instruments you have to get into the measuring systems and that's one of the reasons why I joined it. They also of course, had made a pretty good salary offer. They got me out of the army. Once I was out of the army then Peenemünde could draft me for civilian purposes. So when I came to Peenemünde I was not a soldier like many future employees, I was a civilian, and I was employed as a civilian employee. Fortunately, after our early experiences in Hannover, I had learned that in those days it was still a big problem to ignite a rocket engine. We had many mishaps, most engines blew up at the time of ignition. The ones who survived that normally burned through because we did not have adequate cooling systems. So for that reason, I decided to study diesel fuel injection. Of course, the university did not teach any rocket subjects. No space flight subjects at all. So the injection of diesel fuel into a high pressure engine, a high pressure combustion chamber, was very close to the problems you have with rockets. And for that reason, I became a specialist in that area. I had studied a number of NACA reports. I'm sure you know that the NACA was before NASA. With that special fuel injection knowledge, my boss at Peenemünde at that time, Dr. Walter Thiel, was interested and I was hired. And of course my first job at Peenemünde was to really look into the injection system of the 1.4 ton smaller unit. Thiel and some people who had worked with him had made the basic design of the V2 engine, the way it was finally used during the actual military deployment. It consisted of 18 individual elements and each one of these elements had a thrust of 1.4 tons. In order to improve the performance of the entire system the idea was to save money and do it faster by developing this 1.4 ton unit. Which is one unit of the 18 units to make up the entire combustion chamber. That was my first job in Peenemünde. We also worked on smaller units. We had a one ton system which was supposed to be used for jet assist of airplanes. Our system never made it because we used two propellants and burned

them. Some people figured that was too dangerous. The Walter Corporation in Kiel had developed a hydrogen peroxide system that was eventually used instead of our combustion system. But I was working on that engine and we learned a lot from these two developments. And before we finally switched to the 25.4 ton system for the A4, as we called it, we developed also a 4.2 ton system where only three of these combustion elements were combined. We found, fortunately, that once you have additional mixing of your combustion jet from your individual element you get a much better performance. So, we gained quite a bit in Is_p. In those days, we measured exhausted velocity (V_e) and you probably know the exhaust velocity (V_e) is connected with the Is_pr by the factor g. Since g is 9.81 meters/seconds^2, you just use for simplicity a factor of 10 and make a quick conversion of Is_p into exhaust velocity. In Peenemünde we used exhaust velocity, we did not use Is_P. Although the term Is_P was known, it was not in common use. Our exhaust velocity and the Is_P could be improved by almost 10% by going from the 1.4 ton unit to the 4.2 ton unit. We even got a slight additional improvement when we finally mixed 18 individual jets in the final A4 engine. That's about my career. My activity before I finally got deeply involved in the development of the A4 engine. As I said, the design had been made before. But the problem was the design did not work. There were many problems, like ignition, like cooling of the rocket system, which had to be solved. That was my task as, we called it, a versuchs engineer, test engineer. I did not conduct the extra test runs that was done by another group of people. I was responsible to evaluate the testing and then to tell the designers what had to be changed to improve the engine and make it finally a relatively safe engine.

Launuis: Stan, do you have questions about the engine?

Stanley Starr: When you mentioned a moment ago about the additional mixing, when you had multiple injectors, were you talking about at the top of the combustion chamber as the jets came together?

Dannenberg: Right

Starr: That space in the combustion chamber?

Dannenberg: Right

Starr: So by having multiple injectors close to each other, it actually improved the combustion, the smoothness of the burning

Dannenberg: It apparently improved the mixing process and of course, you want to eject gas molecules through your throat area. You want to have complete combustion there. If you don't have complete combustion, your fuel has not been completely burned, there are still droplets in there. And that's a loss, because a droplet does not give you the large gaseous volume which you need in your rocket engine. That, we had not expected that this would happen but it did happen and it was fortunate because if we would had only the exhaust velocity of the individual component, we could not have made the range of the vehicle that was promised to the Army. We would have been about 10% short. And 10% in Is_p is quite a bit of a loss.

Starr: Why is it that water was mixed with the alcohol?

Dannenberg: Well, as I mentioned very briefly earlier already, we had still tremendous problems with burn-throughs and of course water addition keeps your combustion temperature

quite a bit lower. And the performance of your exhaust velocity, as we called it, does not really go down very much. It's a relatively flat curve and you are fairly close to the most desirable stochiometric point. And so the loss is not all that great but your combustion temperature goes down. Again, a relatively small amount but that helped. And in order to solve the final cooling problem, we even had to trickle in at the outer wall of the combustion chamber additional fuel and the very small over-pressure. That injected cooling fuel did not come in high pressure. We had a system that the pressure could be throttled it came from the original turbo pump pressure, the full high pressure, but we throttled it into the individual chamber went out into the thrust chamber with a very small over-pressure so that it stayed at the wall of the rocket chamber. It did not burn. Again, that was a loss. We lost in exhaust velocity but we had to take that loss because otherwise the combustion chamber would have burned through and we would not have made our duration.

Starr: Was there also a benefit from the peak capacity and heat transfer of the water while it was going up through the walls of the chamber?

Dannenberg: That probably helped. Water is a better insulator in a way than ethyl alcohol and as we pointed out, already, the fuel was ethyl alcohol. That was also the reason ethyl alcohol burns a little bit cooler than gasoline. We also could have used gasoline. But gasoline was anyway not too available during the war in Germany. Germany has no oil wells, so it was tough to make gasoline. While ethyl alcohol could be made very easily. In this country you normally use corn, in Germany we used potatoes. You can also make potato schnapps, potato ethyl alcohol, and that really was the main product.

Starr: In modern rocket engines if there's any combustion non-uniformity, it takes constant as it goes out the nozzle. You'll see streaks of changes in color because it's not turbulent flow, it's

more like laminar flow coming out the nozzle and if it is turbulent, it's a source of inefficiency. Do you recall if the flow out the nozzle of the V2 was turbulent or did it have that streak structure?

Dannenberg: Well, we had essentially streaks if we had burn through at someplace and metal parts, burned metal parts came out of the nozzle. During a good run, where we had no problem of that kind, I don't really recall that I saw streaks. And again, what we mentioned earlier, the mixing probably helped to have a relatively even distribution. We had, of course, some extra fuel on the side and sometimes you saw that burning after it came out of the nozzle. Because, of course, it was relatively hot. There was oxygen still in the surrounding area, so some of it burned. But normally you could not see it because it vaporized right away and it was due to relatively high temperature practically invisible. Also the A4 jet was almost invisible. You had to look real hard to see it. It looked almost like a hydrogen exhaust jet, which you can not see at all.

Starr: As I understand it, before the war ended, you had developed an A4 engine that had an injector plate, rather than the multiple injectors. Could you comment on the problems that were overcome in making that development?

Dannenberg: In the beginning, many manufacturing people thought that it never would be possible to put this very complicated system with 18 individual injection heads into production. Particularly, if you had not skilled welders and skilled check out people who do all the necessary things and for that reason there was a very heavy push on to develop this simpler. We called it a mixing nozzle, where you introduce your fuel, through radial channels, then some other holes come from the top and introduce your liquid oxygen, and of course your radial channels had to have other holes in order to let the fuel enter the combustion chamber to mix the two

propellants. I think we never completely solved the problem since we had tremendous vibration problems when we used this system. We used many different ways, of, again letting the two jets hit each other, mix each other, or come out parallel and hope that they mix later on in the combustion chamber. We tried many different systems. We also tried one system, for example, where the LOX came in the middle and the two fuel jets end up or impacted with the lox jet. But all these systems generated tremendous vibrations during the combustion process. It was so bad that much of the hardware broke, it could not take the vibrations at all. All our nuts and bolts of all the feed lines, of course came loose, and some of them also broke. We never managed to get completely perfect operation of that system. Since Hitler had at the time already decided that he wanted to deploy the A4, as we called it, as a V2 eventually we got the order from top Army levels "to freeze" the design. So we were not allowed any more to work on this new design and to hopefully solve the problem. For that reason, in many earlier schematics of the V2, this new engine is shown already because many people particularly my boss, Walter Thiel, had hope we could introduce it and we could make it work. But it could never be introduced so the real system that was eventually used should be the one with the 18 individual injection units that should be shown. That was the only one that was used for military purposes later. Now since you started that question, maybe I should go a little bit ahead in time. When later on the war ended and the group came over here. One of our three leaders, we had three leaders. One was Papa Riedel, who was leader of the design of the early engine. Number two was Klaus Riedel, who was a real good mechanic. He was already one of the amateurs working with von Braun Oberth in the German group. He was involved in an effort, I don't even know if you're aware of it, after Oberth had to go back to Romania. He was not paid anymore as a consultant and had no money anymore. Some other people convinced the city of Magdeburg to build a manned rocket. That would have been the first manned flight of a rocket. The city of Magdeburg gave Rudol, Nebel and Kurt Heinisch and Riedel forty thousand marks which was quite a bit of money at that time. They expected that they would develop and build a

much larger rocket than Oberth worked on at the time. It would have been almost 10 times as big, I think. They finally, managed to develop an intermediate rocket, it was still not a manned rocket. That was the time when Hitler took over in Germany. During all the problems and all the difficulties, of course, finally the city of Magdeburg refused to make further payments and the activities fizzled out. But a fellow, Frank Rietz, who was here recently in the States, wrote the book on that. Even the people in Magdeburg, the present city government, didn't know about it anymore but they finally found five big folders and all the details are described and I have the book that he wrote. I can show it to you later, you might also be interested to get it. That is, I think it is only available in German. The title is "Magdeburg Pilot Rocket" (in German), the Magdeburg pilot rocket. But I have the book and I may even have it with me. So that was a quite interesting side line. It also showed all the efforts the amateurs put into the thing. Rocketry was really generally very interesting. We had many amateur rocket societies. After the Army got involved they prohibited all these activities, at least in Germany and Austria. Other countries may have continued although Europe was pretty soon taken over. I think there was still something going on in this country.

Launius: There was.

Dannenberg: On the other hand during the war there was not an awful lot of effort put into rocketry. Goddard continued his work and until finally the Navy hired him and the poor fellow had to work again on solid propellants.

Launius: Right. Did you have other questions on the rocket.

Starr: I was thinking that maybe we'd ask about how it was to work with Dr. Walter Thiel.

Dannenberg: Let me even continue what I said. I think I got a little bit off on the Magdeburg project. In this country, Riedel III, the third leader, he was actually my boss at the end. He was really the propulsion man after Walter Thiel had been killed during the bomb raid. So he took over his job and was responsible for all research and development, even in the electronic area, not only in the propulsion area. Riedel III, of course, knew about all our efforts. He was the boss, he even had the idea for some of these efforts and then we came over with the von Braun team. He was a part of the rocket team, but he never got along too well with Wernher. He split off relatively early and was hired by the Rocketdyne Division of North American. It was not Rocketdyne at that time, it was just a small group working on rocket propulsion. He joined the group and he of course introduced all the ideas we had in Peenemünde for improvements of the A4.

Launius: Was he responsible for the Navajo?

Dannenberg: He was responsible for Navajo propulsion.

Launius: Ahha!

Dannenberg: And the Navajo had basically an improved V2 engine. Now for some reason or another, probably through a lot of hard work, we could make the mixing nozzle work. You probably know the Redstone has a mixing nozzle, the idea we had already in Peenemünde. Also, all the other changes we had in mind, Riedel III, really introduced. That's why the Redstone engine is really a grown-up version of the V2 because our chief engineer had all the ideas that we had already earlier and he could now finally put them into reality.

Starr: The Navajo, was a single engine, you can look at it as a single engine with three nozzles. Which were smaller than the A4 nozzle and I was wondering if that was because of the combustion instability? Combustion instability becomes more of a problem, the bigger the chamber. It became very much of a problem when the F1 engine. But, just speculating do you think that's why...

[Note: The Navaho booster rocket had two engines. A later planned version, but unbuilt, was to have three engines. The question was incorrect. Starr]

Dannenberg: I did not know that the Navajo had three nozzles, as you said, three exhaust systems. I thought it was pretty much like the Redstone engine with one exhaust nozzle. But I do know that we had to make a few changes from the Navajo engine to the Redstone. And that of course was done by Rocketdyne. That was done by Rocketdyne even before von Braun had decided to really buy that engine. That was Wernher von Braun's decision. We had to decide if we wanted to develop our own independent propulsion system in Huntsville at that time. Wernher decided against it. He knew exactly what Riedel III was doing at Rocketdyne. He decided since he had the engine and the engine seems to work let's use that engine and let's build the first project on the basis of that engine.

Launius: Ok

Starr: Well, what was it like to work with Dr. Walter Thiel?

Dannenberg: Well, I liked him very much. Of course, he was the one who hired me. He was really a very good a very excellent propulsion specialist. He also made relatively big advances on rocket systems. He replaced Wahmke, whom I mentioned earlier. He also had learned from

Wahmke what you should not do. Thiel was basically not a design engineer. He was a chemical engineer. So he understood what went on with Wahmke and what you shouldn't do. In the long run, he even studied the possibilities to switch the A4 to a storable system . So as a chemist he worked on that quite a bit but again when the design freeze came, that was certainly one of the decisions which was not to make this change. The A4 would be deployed by the Army using liquid oxygen. This propellant combination is very nice for space flight, but it is really not necessary for Army applications or ballistic missiles with a 200 mile range. You can easily do that with storable propellants, you do not need high performance liquid oxygen. And Thiel saw that and he was really the head. He also proposed to use hydrogen peroxide to drive the turbine. It was decided to use the turbo pump so we could use lightweight containers. Otherwise the tankage weight would be have been way too heavy. All these basic ideas came from Thiel of course, working together with von Braun and many others. So I think very, very highly of him. It's in a way a shame, that he was killed during the bomb raid. He probably would have promoted space flight if he would have come to this country. Even much more than Wernher did. In a way, I look to him almost as being parallel in his position to Wernher von Braun. Although, Wernher was the boss, there was no question about it. But both of them had excellent ideas, both of them were really space buffs and they really wanted to build a relatively large flying liquid rocket.

Launius: You mentioned Wernher von Braun as well. What was the culture in terms of dealing with von Braun? He's pretty legendary from the Huntsville era in the way in which he maintained internal communications among all the key people who were involved in projects. He was able to keep everything on track and people talking to each other, which we have found repeatedly, this is a lesson we keep learning, that communication among the various groups is the central most important issue for engineering a successful project. Lack of communication is

what caused the Mars failures last Fall, in '99. Can you talk a little bit about how von Braun ran things, both at Peenemünde and Huntsville?

Dannenberg: Of course, in order to take care of this problem, you just outline the area for all projects as a systems engineering group. We did not have a systems engineering group neither in Huntsville not in Fort Bliss, and also not in Peenemünde. I would say Wernher von Braun was a systems engineer and he had a great talent. He really understood guidance and control problems as well as propulsion system problems. Although as long as Thiel was still around, he made all the decisions in the propulsion system but von Braun also, always very devoted. I think, he was the one who really brought all the people together in Peenemünde and later on also in Huntsville. We had normally a meeting on Monday morning, all the people, all the key people came together and we discussed the problems that we still had to be solved. That way, of course, all the people really knew what the problems in other areas were and where they could probably help, contribute quite a bit. Like the guidance and control system of the rocket engine, of course, ties in quite a bit with guidance and control. For the earlier systems we had jet vanes and thereby jet vanes that protrude into the jet of the rocket engine. When we later on introduced the swiveling of the engines, it was a much simpler project for the guidance and control people, as well as for the propulsion people. We did not have to worry about the burning up of jet vanes. Wernher was one who worried about all these things and he also did not only worry about the vehicle itself, that's what some people do in today's projects. He also worried how do we get the vehicle to the launch site. How do we get our propellants in. So he really worried about the entire, what we call today, the system. The entire system. Of course, he had some people helping in those areas. We had the ground systems group which tied all these ancillary systems together. But he was always the one, and I think the Monday morning meetings helped quite a bit. In Peenemünde we had to write weekly reports. At the end of every week you had to write a report, what have you accomplished, what are the problems that

are still open, and they had to be turned into Wernher and very often we had to turn them in on Friday evenings. Friday at quitting time. And very often on Monday morning he had read already either all of them or a large amount. Then he followed up and asked, well, what are we going to do with this area, what is going to be the next step in order to solve our existing problems. We had a similar system, of course, in Huntsville. We had also weekly meetings and Wernher always tried to be in that meeting. Although he traveled quite a bit, he was not always there but he always tried to be there. I think he also had the good nose, so-to-speak, for selecting people. You probably know his deputy, *Eberhard Rees*, and in many respects he was quite different than von Braun. He had not the big long range ideas, he really didn't care to go to Mars. He wanted to accomplish the project which was going on right now. He was very good in that area. He worried about all the nuts and bolts that you have to find somewhere in order to complete the project. He very often held von Braun down in making too big promises. He very often said, "Wernher, I don't think we can do this". So let's rather cut it down and promise, so and so. I think that was a very clever trait of von Braun to pick people like this in order to be his deputy. If von Braun was not in the Monday meeting, Rees was certainly there. Although, the two also often traveled together. Probably to keep von Braun under control that on his trips he didn't make too many promises that could not be kept. (laughter) I'm talking about von Braun, and I think are we still on Thiel?

Starr: I think we're on von Braun.

Launius: Yeah

Dannenberg: He also had always a very good relationship with his bosses. That was certainly true in Peenemünde, Dornberger and von Braun were really a team, at that time already. And even with the higher ups, even the chiefs of Dornberger in Berlin. He had always a real good

relationship to them. Whenever he was in Berlin, he dropped in and he talked to them. He reported the advances which had been made but also the problems, which still had to be resolved. And of course, on the other hand, he also had always a good relationship to the people who worked for him. Also in this country, of course, the team is still very happy. Only a relatively few people left the team. Some of the them did, of course, eventually. He also had in this country always a real good and excellent relationship with our bosses here. Toftoy, who brought us over and who made the first contact. In Fort Bliss, we had Hamil, James P. Hamil, as our commander there. These two were a very nice team. Of course, later with NASA, he had excellent relationship with Webb. It would even go all the way up to Kennedy. Kennedy and von Braun had a real good response to each other. I think without that, Kennedy might not have decided to stick out his neck and promise that we would go in that decade to the moon. I think von Braun had really convinced him that we had a fighting chance at least to do it.

Launius: Von Braun, instituted a culture of, some people have referred to it as perhaps an excess of testing everything, testing components, making sure it all works. Using the test stands pretty extensively and flight testing before you go to operations. All fairly lengthy process. And a fairly involved, more involved process than some people thought was necessary. Could you comment a little bit about that process of how you, designed, developed and tested and then put into operations the components that you flew?

Dannenberg: Of course, the poor designers know we don't have too much experience with what the problems really are. So normally, almost any design has to be sent through a very detailed testing process. And besides testing the entire vehicle, von Braun also insisted that all the individual components underwent very thorough testing. That goes for all the valves, control valves, main flow valves, also main propellants. And also all the electrical equipment, he always insisted, and we had relatively fancy laboratories to do this component testing. After all

the components worked properly, then of course, we put them all together. Very often von Braun also insisted that we make first a flow test without, for example, igniting the engine. So we tested the turbo-pump or the main control valves without burning the engines because that way you save your propellants and you can even use, in many cases, cheaper propellants. Propellants which do not burn like, liquid nitrogen. Which is basically for free if you make liquid oxygen anyway. And of course, you normally replace your fuel by water and so he insisted on a very extensive testing process. Also, we had the rule that no vehicle will ever be launched unless at least it has undergone one successful static firing test, ground test. That was a ground rule, that always existed. Even later on after the individual stages of Saturn 5 for example, had been tested, we had initially the idea that the first test should only launch the first stage. We used dummy upper stages. We did that kind of test on Saturn I. That was also the plan for developing Saturn V. Then there would be a second launch with only the first and second stage active. Later on, due to the fire on Apollo 1, and some other problems with the project, we were running out of time. You probably know that George Mueller decided that we go "all up". Von Braun was first very reluctant and there were many discussions between George Mueller and von Braun and others, like Sam Phillips. The managers in Washington wanted to keep the time schedule and von Braun finally agreed , yes if we go my way, we can't do it on time, and he finally agreed, yes, that's try "all up". But it was basically a little bit against his philosophy. He would have preferred to do it the other way. Again, as you said, this process adds to the time you need.

Launius: Yes. I guess I've got to ask you the question about the "all up test". Because, that is, many people consider them the most, the gustiest, I guess, the greatest risk, that anybody of any decision made during Apollo.

Dannenberg: I fully agree with that. It was a great risk. And if George Mueller had not had the luck that it worked, he would have looked very bad. But he was lucky, and of course, also von Braun, after he had agreed to the decision, he did everything he could in order to really make the upper stages work properly. Again, we did a lot of static firings of our stages already. There were many reviews, I was not involved in the Apollo program anymore at the end. I was only involved in the beginning. I was amazed by the large amount of effort that was put in, particularly, also after the Apollo failure, Apollo 1. Particularly, on the S-II, second stage, which was a real big problem. A big team of our people from Huntsville, spent a number of months in California in order to help NAA to be sure that the S-II really works properly. Again, all this effort, I think, in the long run it paid off. The Saturn Program today is the only program which doesn't have a single failure.

Launius: 100%

Dannenberg: Yeah. I think that is due, to what you pointed out, the very, very, extensive testing of components, of systems, and finally, of course, the entire flight system itself. Even including all the equipment, also a lot of effort in Huntsville, for example, was put into the Apollo system, the arms that had to be attached to the vehicle for the actual launch. A lot of work was done in that area. It was not just a one time design, built and then it worked. People were absolutely sure that everything goes. And that went also for the earlier vehicles, for the Redstones were tested very extensively, the Jupiters, the Jupiter deployment and the erection was tested in Huntsville so that it was not only just designed and built it was really ready to go by the time it was available for launch.

Launius: Ok, we wandered a little far afield, I was going to try and go chronologically. But that's ok. But let's go back a little bit and talk specifically about Bumper. Do you have any particular questions you want to ask on Bumper?

Starr: Well, one question was, we've heard the statement that there were German engineers present at every V2 launch that the United States conducted. Do you know if there were members of the staff that were present here at Cape Canaveral for the Bumper 7 and 8 launch?

Dannenberg: To my knowledge, there were no Germans here. I think I mentioned earlier already at some time. After about launch number 30, the Army had been trained enough and the Army really did all the firings from then on.

Starr: Ok.

Dannenberg: When the Bumpers took place, we were really moving, at that time, from Fort Bliss to Huntsville. To my knowledge, all the people were really doing the move and of course, it wasn't only the personal move, but it also all the technical things we had to take along after we moved. I am not even sure if von Braun was even at the launch here. Even Pickering, I asked him, and he did not remember it. He asked one of the other people and no one knew. I personally almost think, he was not here because it was also the time when he was married. You know, he came over as a bachelor and he married his wife around that time. I don't know the exact date, which of course, is recorded some where. But I personally think that neither von Braun here, nor Rees, were here nor any of the other Germans because the entire operations had been turned over to the Army and the Army did a pretty good job. They knew all about it, they had all the equipment. They had the Mieller-wagen to transport the V2 here and so, to my knowledge, there was no one involved. I, personally, certainly was not involved.

Starr: Well...We're becoming more and more interested in looking into Dr. Kurt Debus' background. Of course, he really started Kennedy Space Center, what became Kennedy Space Center and was a great leader in the launch business. Can you comment on your, how you may have worked with him in the past, and if you did, in what capacity he played.

Dannenberg: Of course, Kurt Debus became a great leader in launching activities. He did that already in Peenemünde. He launched towards the end, most of the V2s that were launched for scientific purposes. We made already in Peenemünde a number of short straight ups in order to make measurements like we did later on in Fort Bliss. Also, of course, to find out a little bit more some of the problems of the V2s, which had still quite some problems at the time when they were deployed. Debus was in charge of those. [see correction below] There were, of course, beside that already, a lot of military launches. He probably helped to write the book I showed you earlier, the A-4 Fibel. During that time, he had accumulated a lot of experience, he knew what can go wrong, he knew what is needed to get the propellants in time and during the war, of course, it was sometimes tough to get even alcohol and in particular lox was a big problem because there were so many launches if you also included the Army activities that the facilities were not always sufficient to supply all the basic materials.
[Mr. Dannenberg provided the following information after the interview: Hartmut Kuechen was the launch director for Test Stand VII. Debus participated in launches as a representative of Guidance and Control Division (Bodgerate, Steuerung & Messwesen (BSM)) and was later involved in mobile, P-X launching.]
Starr: OK.

Launius: We talked a little bit, yesterday, experiences at the Cape that you first encountered, do you want raise some issues along those lines?

Walters: What was your first visit to the Cape area, approximately?

Dannenberg: Pardon?

Lori Walters: Approximately in years, what was your first visit here to the Cape area?

Dannenberg: I think it was around '51. I was here for the early Redstone launches and I don't remember the exact date but must have been, certainly the early '50s. And I would guess it was '51 but I don't know the exact date.

Walters: So it was early?

Dannenberg: It was very early. It was really the follow-on at least for the Army side of the Bumper launches. It was to launch Redstones. Now the Air Force, in the meantime, launched Larks and Hound Dogs, I think and some of the early vehicles, which were not rockets, but air breathing launch vehicles.

Launius: Right.

Dannenberg: Some of them were really automated airplanes. Some of them were, initially, propelled with rockets, like the Navajo. But probably our Redstones were the first additional rocket launches at least as far as I know. All the other Air Force vehicles came later, the Atlas came quite a bit later, even the Thor at about the time of the Atlas. The Thor was really designed and built because the Atlas had a lot of problems. In order to have a weapon against

Russia the IRBM, the Intermediate Range Ballistic Missile, Thor and Jupiter on our side were designed. So that was relatively late. And the Titan, of course, came quite a bit later.

Launius: Much later, in the sixties. What was your first impression when you came down to the Cape? Did anything strike you about the area?

Dannenberg: Well, I think in the discussion we had yesterday, it was really very descriptive. There was practically nothing here. I even, seem to remember scorpions, do you have scorpions here? Yes, I think we encountered scorpions and, of course, mosquitoes they were very bad and very big. Even cockroaches in the hotels. I didn't see them at the launching site but in the hotels we had big ones. And also as pointed out yesterday, there were hardly any facilities available. I think I pointed out we could not stay in Cocoa Beach, where people stay today. There were practically no restaurants, no places maybe some people rented out a room but there were no hotels or motels at all. We normally stayed in Melbourne which was quite a distance away from the launching site. And of course, the Army provided transportation that was brought in for that purpose and I think, eventually, Debus and Gruene and the people who eventually came here and stayed here. Inititally they were also here only on TDY. They came only for the launch. Of course, for them it took probably more than a week to prepare the vehicle. I was only an observer. I was involved in the Redstone development, of course. I was responsible for the engine which we had bought from North American as I said earlier. So I saw quite a number of the early launches because the propulsion problems are the first problems you have to solve and only after you solve that you worry about guidance control, measuring, Doppler and all these other additional attachments. That's why I saw most of the early launches. And then, later on, after we had switched or were switching even to the Saturn project. I don't know if you're aware the Saturn project was started by the Army.

Launius: Oh yes.

Dannenberg: The Saturn was not initially a NASA project. Then I was in charge of producing the Redstones with Chrysler, so I was a Chrysler liaison engineer again, von Braun always liked to keep his finger in the pie. So he had liaison people. I don't know if you know, that Magnus, von Braun's brother was even stationed in Detroit.

Launius: I didn't know he was stationed there.

Dannenberg: He was a local liaison engineer. Initially, he did it from Huntsville but again there was finally so much work when they really got the production under way he stayed and he lived in Detroit.

Launius: Was he the only person from the team who was there full time?

Dannenberg: There were some Americans, he was the only German.

Launius: Ok

Dannenberg: who was there on the team. But I know a Bill Davidson who was also there and there were also two or three others. They mostly had the job of course, even when Chrysler finally produced the Redstone, there were still some unanswered questions. Very often you could not get some things that you wanted to have like valves, control valves. So there still had to be quite a bit of design work to be done. That's what these people, these extra people, besides Magnus, what they really did to make design changes. Often Chrysler wanted to produce a part in a somewhat different way, so these people made the new design and I think

Magnus had the authority to approve the design so that it then could enter into the contract with Chrysler.

Launius: So there was very, very strong contractor penetration?

Dannenberg: It was very strong contractor penetration. They had to design and build to our drawings and all our drawings had to be approved and signed by government personnel.

Launius: Ok. On the Redstone, of course, there, it was originally built as a ballistic missile. The reliability rates, I think, were quite excellent for it.

Dannenberg: They were very good. Although they were a few failures but it was much better then any other launch vehicle.

Launius: Oh yeah. The decision was made probably in the '59 timeframe, I'm not certain of that, to use the Redstone as the launcher for the Alan Sheppard flight, the first American to fly. You were involved in preparation, I guess, of the Redstone for mating it to the Mercury capsule.

Dannenberg: Not so much for the mating. That was an enhancement but I purchased the launch vehicle that was used for his launch from Chrysler. But it had to be modified so that it went to Huntsville. They made all modifications, they put the capsule on, and I think the final assembly was done here. I'm not too sure.

Launius: I believe so, I think so.

Dannenberg: I think so. The final assembly was done here.

Launius: Did, I mean obviously this was going to be the first spacecraft that's going to fly a human being from the United States.

Dannenberg: I think you are right, initially it was planned that the Atlas with somebody like John Glenn would be the first one.

Launius: Right.

Dannenberg: But the Atlas had a lot of problems at that time.

Launius: Terrible.

Dannenberg: Even the last one before Glenn, malfunctioned, failed. So I really give Glenn a lot of credit that he was risking his life to get on the next one. Of course, he knew some changes had been made and the vehicle should be better but it was still a lot of risk. And in order to have a successful launch that's why, I think, it was pretty late, in the late '50's certainly to decide "old reliable", Redstone was known as the "old reliable", that it would be used for the first two manned launches.

Launius: Did you do anything special to prepare, other than, obviously, the modifications made to the rockets so that it would be able to have the capsule attached? Did you do anything special?

Dannenberg: We had in my group, a team of people under Dr. Joachim Kuettner, I don't know if you ever heard the name. Can't even think of his first name right now. Joachim, Jack we called

him, Kuettner worked very closely with all the astronauts not only with Glenn because initially it wasn't even clear who would be the first one to fly. And of course, the astronauts had the opportunity to push a button, to ignite the escape tower and then only the capsule itself would have been separated from the vehicle and would land on a parachute like they all finally landed. We were pretty much involved in those activities. We had a small group of people who worked out the basic requirements with the astronauts and of course, we did not build the capsule, as you probably know. That was built by McDonnell Douglas. But we had to give them the specifications, of course, interface with the launch vehicle.

Launius: Let's go back again a little bit and talk about Sputnik for just a minute. That in retrospect it's somewhat hard to believe that was such a crisis. But there seems to have been a real crisis of confidence of Americans that resulted from the Soviet launch of Sputnik being the first shot. And of course, the United States had selected the International Geophysical Year project, Vanguard that the Naval Research Lab was working on. You all down in Huntsville had put forward, what became Explorer earlier and had not been allowed to proceed with that project. But at the time of Sputnik, did you have specific reactions or thoughts to it, what went through your mind and the minds of other people that were down in Huntsville at that point?

Dannenberg: I think in general and in my own personal feeling, was "we told you so."

Launius: Ok

Dannenberg: We knew that we could have launched our satellite much earlier, then what finally happened. It was generally known, particular by people like Ernst Stuhlinger. You probably know Stuhlinger, who had contact with Russian scientists on many international meetings. He was concerned that they were going ahead, that they would make it in that geophysical year or

maybe a little bit later. It was unclear. He was fully convinced that they would build a satellite that they would fly a satellite. Von Braun, as you probably know, tried to convince his superiors and decision maker. In the beginning we were even in competition with the Vanguard. Then it was finally decided after even a special committee that looked into all the possibilities. President Eisenhower finally proposed to use a Vanguard as a peaceful project. He did not want to use a military vehicle like the Redstone. I think Dr. Pickering touched on it briefly yesterday. I don't know if that was an afterthought, so to speak, or if von Braun had from the beginning the idea to use a Redstone to simulate a very high reentry velocity to build an innovative Jupiter C, a modified Redstone with JPL upper stages. And if he later on got the idea if he put one more stage on top of it then we could launch a satellite. I personally, think it was the other way around. I personally think Oberth, Herman Oberth was at that time in Huntsville, and with his help and many others who were involved, I think they had contemplated using a Redstone to launch a satellite. So they added three (3) upper stage for a total of four (4) stages. In the beginning and only if the satellite project was turned down, then von Braun came up with the idea, in order to give us a go ahead to do at least some steps in that area why don't we use a Redstone to simulate reentry velocity for the Jupiter nosecone. There came the go ahead to build a three stage vehicle with two solid propellant upper stages. You probably know some of them were launched. One of them had a very successful range of over three thousand miles. The nosecone of this mission was even shown eventually by President Eisenhower on television to show that we also can do something, not only the Russians. But, of course, it was a very small accomplishment compared to what the Russians had done. So our people, in a way, were prepared for that and you probably know that at the time when the Russians launched their Sputnik we had a big meeting with the Secretary of Defense, Neil McElroy, in Huntsville. We learned at that time that the Russians had launched their satellite. At first, people didn't believe it. But then from all the radio transmissions it was clear that they had done it. Wernher, and General Medaris gave McElroy a detail briefing. He did not really hear and

see what he came down for to Redstone. They basically talked about what we had to do in order to launch our satellite. We did not get immediate permission to go ahead and launch it. But that came about a month later. In a way, we were very happy. And now in hindsight, I personally am very happy that it happened this way. If we would have been the first, I think then there would have been no great attention. Every one would have said well the Russians can't do it as fast and as good as we can. And we would not have had all the excitement we had after the sputnik launch. So I personally think in the long run it was beneficial for the program. And also the fact that they launched a man first.

Launius: Right

Dannenberg: That helped in the long run. Without it, Kennedy may have never said let's go to the moon.

Launius: That's probably true. Do you have some other questions?

Starr: Well, to go back to the local environment here in Brevard County. What did you do for recreation when you were down here on TDY supporting these things? Or was it all work?

Dannenberg: Of course, during the Apollo program Cocoa Beach had really been built up. There was even the Astronaut Hotel at the end of the street. Many other hotels, Ramada Inn I think was one of the earlier ones. We very often stayed at he Ramada, I remember. And of course, we had pretty good facilities at that time. The old conditions were really not existing any longer. It had been built up. I think, all of Florida was really already getting benefits from the space program at that time. Right now I talked about the Apollo program it was not that way during the early days of Redstone and then later on the Jupiter for people from Huntsville. The

Jupiter days were already pretty acceptable. We still went for quite a number of years to Melbourne we liked the, what was the name again?

Walters: Tradewinds

Starr: Tradewinds

Dannenberg: Tradewinds, Tradewinds, we liked it. Although we could stay at that time in Cocoa Beach we still went very often there. And we particularly knew the bartender quite well. After a successful launch we always had a big party of course.

Starr: Were there any restaurants that you remember? Any...

Dannenberg: The one which is still here, was here already and we always liked to visit it. And I can't think of the name right now

Starr: Bernard's Surf?

Dannenberg: Bernard's Surf, right.

Launius: I'll ask a couple more about Apollo and Saturn. You mentioned it and it's well documented that the S-II stage had enormous difficulties. If you could, please, could you outline a few of those problems and how you went about solving them?

Dannenberg: Well, right now, I even don't remember what the main problems were. Of course, the basic design is a very difficult design with a common bulkhead between the LOX tank and the hydrogen tank and that was a big problem. And I even seem to recall that even manufacturing it North American had great problems. And of course, later on it had to be tested and the early tests showed even some separation in places where it should not be. On the other hand, of course, in the long run it worked fine. As we said earlier we had no major failures there were smaller shortcomings, but it worked fine. We had to spend a lot of time to do it. And, of course, the J2 engine was a relatively new engine. It had been tested on the Saturn 1B. That was really the original purpose of the Saturn 1B to test this engine. Later on it became a very versatile launch vehicle and it did a lot of Apollo testing. Well, it was a new engine and of course with a new engines you always encounter problems particularly if we finally started to cluster them. Also, to do testing at North American created again timing problems. You had to get all the parts together. And of course, the early battleship versions for stating firing were somewhat different from the final version. Of course, we also had problems, at that same time, with the Apollo Command Module. So it was really a dual problem taking place at the same company at the same time.

Launius: Where they over extended? I've heard people suggest that?

Dannenberg: I, personally, have a little bit of that impression. I think they chewed off too much.

Launius: Ok, that makes some sense.

Dannenberg: Also, I think there were some management problems. You probably know that "Stormy" Storms was finally fired. Some people even blame von Braun that he had griped about

him and that he was of the opinion that he did not do enough, particularly to keep the time schedule. It was to a large extent also a timing thing.

Launius: One of the problems that they had on the Saturn was Pogo. What was known as Pogo. Can you describe that a little bit? And what's does it play when that takes place. And How did you all go about trying to solve it.

Dannenberg: I understand that Pogo is a little bit the problem we discussed earlier. It starts with your rocket engine. The firing generates some vibrations. These vibrations are so powerful that they really go through the whole system. In fact, the astronauts, and there were some Pogo effects on some of the flights, they noticed it so they felt it. I understand the final solution was when the lox came through the lines it started to boil already and that triggered the pogo because when your air bubble or your lox bubble gets into the combustion chamber, of course, it gets less amounts then if it was a liquid and that starts the Pogo effect because you don't have a perfect combustion at that point. I understand that Karl Heimburg was doing the testing at our place. We solved it by bubbling some nitrogen, I think it was, through the line. That apparently somehow solved it so that at least the lox did not bubble. Now I don't physically quite understand the things but by some measures on the test end that problem was at least made much easier. It was not quite as bad. I think there was still a little bit of pogo danger.

Launius: Anything else?

Starr: Well, I had one to go back to the A4, that era. You talked about storable propellants versus the liquid oxygen as an oxidizer. When the Redstone was built, it was built as a field deployable missile using liquid oxygen whereas the American group was under JPL was developing the Corporal which was somewhat similar but was using storable propellants.

There's been this discussion of pros and cons of the two and the exhaust velocities were not as good with the nitric acid as an oxidizer. Why do you think that the decision was made to stay with the liquid oxygen for the Redstone.

Dannenberg: I think von Braun had a strong hand in convincing the Army, he was always a good convincer, convincing the Army to use the developed technology of the A4. To apply it again to a big American launch vehicle. I think that was the main reason. Von Braun, again, always liked to take step by step, small steps, and build on what you have. I think he was really the one who convinced other people, Toftoy, and other people in the Pentagon to stick with the existing hardware. And...

Starr: Like you say it's better for a Space vehicle not necessarily for a missile...

Dannenberg: Yes, that's why von Braun pushed it, for going into space, he wanted to get experience with liquid oxygen. And you probably know initially, he was not in favor of liquid hydrogen. I think the explosion in Kammendorf, that I mentioned earlier, was part of the reason. Although they did not use liquid hydrogen, but they used in the explosion hydrogen peroxide, H2O2. So that in connection with hydrogen made him a little bit reluctant to eventually switch to hydrogen for the upper stages. Although he always admitted that the performance would be better. There's no question about that. But he was just afraid because hydrogen has many problems. I'm still surprised that it worked finally so well. Even today with the Centaur upper stage, you hardly hear of any problems and the problems are more typical mechanical problems like dirt in your pipeline. But I never have really heard of a big problem with hydrogen. So after you know how to do it it's probably a relatively common thing. On the other hand, I would not want to use it for military vehicles. So I think the military made the right decision. You probably know that von Braun tried to sell the Jupiter to the Navy. That's why the Jupiter is fatter than the

Thor. The Thor is smaller in diameter so you could get the Jupiter into the submarine. I fully agree. And I think I did even at that time. That the Navy decided against the Jupiter. And that they rather wanted to use storables and they went with solids. All the solids have other disadvantages but they are not that important for the Navy and the Army. They don't want to go to the Moon, they don't want to go to Mars. They stay down here on earth and you can do that with storables easily.

Launius: Do you have any other questions? Ok. I guess one final question and that is, about your involvement in rocketry as an amateur back in the 1930's. We've come so far, or so it seems, since then. Did you envision at any time that we would actually go to the moon, that we would fly to the planets? Is all of this that has taken place in the last half of the 20th century, amazing to you? Or did you foresee it all?

Dannenberg: Of course I was a Mars man right from the beginning. Max Valier convinced me that eventually you can go to Mars. And he, basically, was a flyer during WWI. So he attached rockets to airplanes and so he built bigger and bigger airplanes even to go to Mars. I was surprised, though, on the one side that we did not build the space station first. I was a believer in Herman Oberth, who said we should first have a space station and then use the space station as a spring board to go eventually to the moon and eventually even to Mars. That has not happened even today. I understand the docking is going to take place tomorrow, yes?

Launius: I believe it's today.

Dannenberg: Today, even. But we still don't really have a space station yet.

Launius: Not yet.

Dannenberg: I am surprised that we made it so fast to the moon. And of course, it was really a political decision to go to the Moon. Without Kennedy, without, what we said earlier, the Russian manned launches, we probably still would not have landed on the moon. I think, in a way, politics really helped in this case the space program to take some very major steps. I always had expected that one of these days we would be on the moon. So that is not really any great surprise. I'm happy that it happened that early. I'm still a little disappointed that we are not further ahead on the space station. I, personally, am fully in favor of having an international effort although some people, particular with the Russian problems, say we shouldn't have done it. In fact I would like to invite the Chinese to also participate. I personally am convinced that we will be one of these days with people on Mars. When? That's still the big question. So in that sense I am not quite as happy as I would like to be.

Launius: I think that is the perfect place to conclude this.

Dannenberg: Ok, I am so glad I have been able to make this contribution.

Launius: Thank you so much, I appreciate it.