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Reisig, Gerhard. June 5-7, 1989. Interviewer: Michael Neufeld. Auspices: DSH. Length: 4 hrs.; 111 pp. Use restriction: Permission required for access.

Reisig begins by discussing his hiring at Peenemünde in October 1937; temporary transfer to Kummersdorf; works on test stand measurements. Reisig discusses the A-4 and 25 ton engines; guidance control problems in the 1930s; organized December 1937 A-3 firings. Discusses experimentation with multiple guidance system (1938-41); origins of the pilot production plant; beginning of university involvement with the rocket program; development of the Wolman Doppler tracking system; development of the unbalanced gyro accelerometer, telemetry system. Discusses von Braun, Hermann, Thiel, Dornberger, Steinhoff, Klaus Riedel, Siemans and others. Discusses Wa Pruef 10; A-4 and A-5 launches, telemetry; A-5 oscillograph/camera system problems; A-4 mobile launch system. Discusses origins of "everything under one roof" concept for Peenemünde; its continuance in America; reason Peenemünde protected from outside influences; protectors of Peenemünde; Army-Luftwaffe conflicts not present at working level. Discusses JATO; rocket-plane work; Wasserfall; the SS; the Zanssen affair (1943); von Braun's arrest. Closes with reaction to Hitler assassination attempt; reasons for continuation to fight to the bitter end; evacuation to Central Germany.

June 5, 1989

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1Interviewee: Dr. Gerhard Reisig

Interviewer: Dr. Michael Neufeld

Place: National Air and Space Museum
Washington, D.C.

Date: June 5, 1989

TAPE 1, SIDE 1

DR. MICHAEL NEUFELD: This will supplement the earlier oral history interview done by the National Air and Space Museum. Okay, so Dr. Reisig--

DR. GERHARD REISIG: Gerhard is good enough.

NEUFELD: Okay. I want to build this interview on the other one, so I think that in the first one they did a very good job covering your youth and the early 1930's, so where I want to start is when you first came on board the organization, which--so you first went to Kummersdorf is that right?

REISIG: For a very short time, yes.

NEUFELD: What month did you start there?

REISIG: The date was the 1st of October, 1937.

NEUFELD: And you only stayed there a very short time?

REISIG: Yes. See, when I came to Peenemünde, to report, there was nothing. Of the test stand was just a concrete structure, no hardware installed yet. Von Braun said, "It's no use, you waste your time here, go to Kummersdorf and look what we've got there, and you see how we arranged everything, that we are to continue in Peenemünde." So I went back to Kummersdorf, I don't quite remember how long I have been there. By sure I was back by Christmas '37 in Peenemünde. A period of several months there.

NEUFELD: It was only literally a couple of months then, two or three months that you were there. When you first came to Kummersdorf, I know from the first interview that you say you primarily worked on the engine static tests.

REISIG: I was hired by von Braun as chief of the measuring group, the aspect of helium, technical, physics. Of course, essentially the first thing we had to test out were the power plants. As far as the control system is concerned, they had very little in Kummersdorf, hardly anything. That was properly

developed in Peenemünde then. So in the beginning, it was essentially power plant testing. I had to do the measurements and develop the measuring systems.

NEUFELD: And so that included propellant flow rates. I remember you mentioned that part in your first interview.

REISIG: Flow rates and all kinds of pressures.

NEUFELD: Combustion chamber pressure--

REISIG: This was the main pressure of course, but feeding pressures and the supply pressures in the tanks and pressure drops in the pipelines and things like that. So it was quite a number of measurements. And of course, the main thing was measuring the "Schub," the thrust.

NEUFELD: The thrust?

REISIG: Yes, and that's one thing I particularly developed. That was quite a problem in the beginning, since then we had the pressure cells only, to measure the thrust. But we had to buy what we could get from industry. It was not designed for our purposes. So I found quite a few flaws, particularly in regard to reliability and the accuracy of these pressure measuring cells. So I developed together with a scale factory particular scales to measure more or less the thrust in a mechanical way, and I think we were quite successful. Even we did it then for the big engine for the V-2.

NEUFELD: The A-4 motor. But when you first came to Kummersdorf, you were primarily working with the A-3 motor, later being in the A-5 also.

REISIG: Only A-3. Oh yes, the motor.

NEUFELD: And so that was what you, when you came to Kummersdorf, it was primarily Thiel's group there that you were working with, right.

REISIG: Ja. You see, there was a temporary separation. The major part of the employees went with von Braun to Peenemünde. The work on motor development was still going on in Kummersdorf, we couldn't cut it off. So Thiel stayed on, particularly because he developed already the prototype for the A-4 motor. And the agreement was that he would transfer with his group to Peenemünde as soon as the proper test stands there would be operational.

NEUFELD: Okay. Now, I remember from one of your articles, saying that a smaller test motor was built.

REISIG: Yes, that was one of the genius strokes of Thiel. Of course we had learned a lot already from our testing, experimenting with the A-3 A-5 motor, and we applied or thought we would apply these findings to the big motor. But the trouble was, the increase in throughflow was so big that we couldn't just copy it in a larger size, from the A-3 A-5 motor, and Thiel had a real marvelous idea. He said more or less, "I'll put a number of A-3 A-5 injector systems together in one engine," and that was the start for this funny 18 pot engine.

NEUFELD: Right, 18 injectors.

REISIG: Yes, injector pots. I don't know whether you have any technical comprehension of the injector problem.

NEUFELD: Well, I certainly read your article and I've got a fairly good picture of what you're talking about, yes. I've actually seen the motor too.

REISIG: Now, of course, Thiel couldn't handle a 25 ton thrust engine at Kummersdorf. So he started the four pot configuration, which delivered between four and five tons of thrust. That was really the starting point of the big engine. I still remember that he was very successful with this smaller thrust engine. And then he came to Peenemünde, simply talking, "Oh well, now we put it times four, times five of what we did in Kummersdorf."

NEUFELD: We'll just scale this up four or five more times to produce a twenty-five ton thrust.

REISIG: Right.

NEUFELD: That was the engine that you wanted for A-4. So the original idea for the sort of multiple injection chambers came out of combining A-3 injectors? But of course they weren't A-3 injectors, were they? I mean, a new injector design, the pots came in.

REISIG: Yes, but see, the basic elements of these pots were built more or less on the advanced model of the A-3 engine. For instance, we learned in the meantime to inject the fuel from one element in the center of the pot but the oxygen from a number of elements around the fuel injection. Each pot was designed the same way. And then a particular kind of injection nozzle we found most effective was the so-called Schlick nozzle, these little nozzles injecting the oxygen.

REISIG: I should have a drawing of this.

NEUFELD: Yes. I have your articles here, I don't know whether the diagram in there is of much more use to illustrate what you're talking about.

REISIG: You see, you are writing the article, and what the editor does about it is often different. So, you see, here is the central injector for the fuel.

NEUFELD: Right.

REISIG: And here around are the little nozzles for the oxygen injection.

NEUFELD: Around the "Stiel." The stiel--I'm not quite sure what the translation would be--on the injector. It's sort of like a little column that the injector is shaped as.

REISIG: There must be a special name.

NEUFELD: I'm sure there is but I can't think of the translation.

REISIG: Well, I can quickly get another drawing, so you see the real arrangement in the pot.

NEUFELD: Okay, so we're looking at a picture of the cross section of a pot.

REISIG: That's the A-4 engine, yes with eighteen pots attached to the engine head.

NEUFELD: Yes, looking toward the injectors from underneath, in the eighteen injector motor, at the nozzles. So around the outer rim of each of those pots are the alcohol injectors arranged. The alcohol comes out from five parallel rings of nozzles on the conical wall of the pot.

REISIG: You see the arrangement. The pots from the top.

NEUFELD: Okay, for the record, I'm looking at a diagram at the time of the standard A-4 motor with all of the 18 injectors. The alcohol was circulated first through the jacket of the engine.

REISIG: Fed in at the end of the nozzle here, and then going back through the double wall of the engine.

NEUFELD: And goes backward through the double wall of the nozzle up to the top of the engine and comes out of injectors at the wall of each pot.

REISIG: Actually, in the A-4 engine, the LOX is not injected

from a stem anymore. It was shaped like a little dome. This dome carried six parallel rings of injection holes, one hundred of them in all. It was easier to apply the injection holes for the LOX on a larger surface.

NEUFELD: The oxygen "nozzles" in six rings around the surface of the dome.

NEUFELD: So at some point then between '37 and '38, the shape of the LOX injection was changed from a long stem into a broader sort of dome for the LOX injectors, is that right? Why was that done? When or why and when do you know was that change made?

REISIG: I think it was found more or less empirically. You see, the main thing is, when you want to accomplish a really good mixture of the two propellants it was hard with the stem to really get a good mixture. With the injection arrangement we have a better distribution across the whole head of the motor.

NEUFELD: Okay, so just through empirical experiment with different injector shapes that Thiel's team changed that design from A-3 on, from, was this mostly after the failure of the A-3 launches on Oie in December, '37, that further changes in the motor were made or in experiments?

REISIG: The failure of the A-3 launches resulted from deficiencies of the control system. The propulsion system worked perfectly.

NEUFELD: Okay.

REISIG: I don't remember too well when Thiel actually started on this four pot engine. A lot of changes went on conceptually. I think I wrote it here in the article. See, the first idea of the A-3 engine was to have the combustion chamber as long as possible.

NEUFELD: Right. As you described in your article--

REISIG: Then the mixture was quite inefficient.

NEUFELD: But there was no way of knowing at that time because no one had done it.

REISIG: You have to learn it. We didn't have anything to copy from.

NEUFELD: Right.

REISIG: And now, I think I wrote it here in the paper too, the final idea was that the sphere for the whole combustion chamber is the optimum configuration. The length of the chamber

increases the combustion efficiency. As a matter of fact, you are losing speed if you have to push the whole mass of gases forward through a too long combustion chamber.

NEUFELD: This causes pressure fluctuations.

REISIG: Also, yes. There could be standing waves, you know, even in the chamber that would generate heat concentrations, causing burning through the chamber wall.

NEUFELD: Yes, and then you have incomplete combustion or irregular combustion.

REISIG: Right. So you see, the A-4 engine is basically a sphere with what's called the plate with the pots on top of the sphere.

NEUFELD: Theoretically this sphere is the best combustion chamber design.

REISIG: This configuration, yes.

NEUFELD: This configuration. But even so later on--

REISIG: Some people don't like that.

NEUFELD: So you wrote there that in the American engines the engine designers got away from that again.

REISIG: Yes, sure. They built cylindrical chambers which are easier to manufacture, but are not as efficient as the form we found in the A-4 engine.

NEUFELD: Okay. So yes, I'd wanted to ask you about Thiel, certainly about his personality. You mostly worked with him only for a short time directly, I suppose, at that time, when you came to Kummersdorf.

REISIG: We had pretty close contacts all the time. I switched then from the static test area with testing and measuring activities to the on-board measuring systems. There was nobody else in this field of development. So I had to separate myself from test stand measurements, and the guy who was in charge of these measurements in Kummersdorf took my part over. He was pretty experienced too.

NEUFELD: Who was that?

REISIG: Haackh. I knew him for many years.

NEUFELD: From Siemens?

REISIG: No, from Kummersdorf. He more or less introduced me to the measuring concepts in Kummersdorf. That's where I got my training.

NEUFELD: Well, even though you had only stayed there a short time, can you just describe Walter Thiel? What was he like as a personality? You knew him later on obviously well.

REISIG: He was a very capable man. He was not quite a genius like Wernher von Braun, but more or less he was in the same category. A rather quick thinker and a productive thinker. He was full of ideas. But he had such a good scientific training that he didn't get lost in his ideas. He always was striving for practical results.

NEUFELD: He was trained as, I think I saw this, he was trained as a physical chemist? I think he came out of chemistry anyway.

REISIG: He got his doctorate in chemistry. But well, the center of his education, I wonder if it was chemistry.

NEUFELD: Do you think his--

REISIG: He was very good in thermodynamics. Which is very essential of course for the development of rocket propulsion systems.

NEUFELD: Yes, when you're talking about heat transfer and combustion and the combustion process, right. Do you think that he was primarily responsible for the innovations in the engine design, the combustion?

REISIG: I would say so, yes.

NEUFELD: In 1937-38, he was the one who generated almost all of the ideas at that point.

REISIG: Yes. Of course he was in close contact with von Braun and gained competence from von Braun's experience in rocket propulsion. You see, von Braun then concentrated much more on the control and guidance problems. He knew he had a good man in propulsion, and he could rely on him and he had to build up the control and the navigation.

NEUFELD: Right, I know that that was certainly a major problem, since the A-3 just had failed.

REISIG: You see, the trouble with the A-3 was that we had, for this control system, to rely entirely on the industry. We hardly

had anybody who could claim that he was competent in control, I mean really competent, of course. Von Braun knew about the problems of gyros and servomotors and things like that.

NEUFELD: (crosstalk)--but he was coming to that from the outside himself.

REISIG: You see, the industry, which was "Kreiselgeraete," and Siemens too, didn't have any theoretical experts to speak of. The chief of development at Kreiselgeraete was Dr. Gievers; he was a good man, but--

NEUFELD: And he was Kreiselgeraete's--

REISIG: Yes.

NEUFELD: He was their chief of development.

REISIG: Right.

NEUFELD: Kreiselgeraete was run by this von Boykow, right?

REISIG: He established it, yes.

NEUFELD: Yes. Was he a presence at that point? He was older, I gather he was an older Austrian naval officer.

REISIG: Yes, he was.

NEUFELD: From the First World War.

REISIG: Von Braun knew him and he had a pretty high opinion of him. But I never met him. And he died pretty early, I forget when. Before the war.

NEUFELD: Right, so right around this time.

REISIG: Now, of course, we had all kinds of problems with these control devices of Kreiselgeraete because Kreiselgeraete essentially worked for the German navy.

NEUFELD: Right.

REISIG: I am not sure whether you know about this.

NEUFELD: Well, I've heard a little bit. I gather that they were working on gyros for torpedoes or something.

REISIG: Their main business was the control of gun barrels. I don't know whether you've heard of these modern German

battleships, the Bismarck and the Tirpitz and their turrets? They were terrific in their firing accuracy, and the reason was Kreiselgeraete, because of their design of control systems for the gun barrels, that the gun barrel was always oriented toward a fixed plane.

NEUFELD: Right. Right.

REISIG: See, the ship is--

NEUFELD: Is rolling --

REISIG: --what do you call it? Rolling or whatever. And the hitting of the target is a matter of accident. You hit or you don't hit. But the Bismarck and the Tirpitz were terrific in the accuracy of their hitting, because of this control that the gun barrel had always the direction which was reckoned optimally for hitting the target, and that's what Kreiselgeraete did, so it was pretty heavy stuff, you can imagine, to compose such a control system.

NEUFELD: Yes, very large control elements like servo motors.

REISIG: It was not exactly what we needed.

NEUFELD: No.

REISIG: A very good thing, yes.

NEUFELD: But that would also be controlling only one axis, too, wouldn't it?

REISIG: I'm not absolutely sure. Maybe there were two axes. You see, the ship rolled, but it heaved also like that. But I think it was two axes.

NEUFELD: But even so, to design a missile guidance system you have to shift from. Kreiselgeraete's very large equipment--

REISIG: The trouble was--

NEUFELD: --two axis but not three --

REISIG: Kreiselgeraete was the only firm which could build or design so-called control platforms. For our rockets, we had to control in three axes, and the gyros had to be put together in certain arrangement to each other and that was done with the so-called platform. This platform is suspended in the swivel rings, which allow the gyros three independent directions of motion.

NEUFELD: Right.

REISIG: The best thing, I can show you in the museum, in the Space Museum here.

NEUFELD: Three degrees of freedom.

REISIG: Yes, how they were built. So we had to rely on Kreiselgeraete for that. No other company maybe around the world could do that. I don't know how far Sperry was here in the USA.

NEUFELD: I know someone is writing a history of inertial guidance, so obviously what his conclusions are--I myself don't know--so I think in your article you mentioned somewhere, or it was in your oral history, your first interview, that the A-3 guidance failed to do the job on those launches in December, '37, and you said something about it being wrong in principle, that there was something in design--

REISIG: Yes, the main problem was, the rolling motion of the missile.

NEUFELD: Right.

REISIG: And there was a certain kind of roll control in this Kreiselgeraete platform, but von Braun proved it was not enough. It was absolutely underestimated, and the gyros are so sensitive, if the darned thing starts rolling,--see, they "kippen über," out of their own control.

NEUFELD: Yes, tipped over.

REISIG: Tipped over, yes. And that was the very thing. You can imagine, after we had these three failures on the Oie, there was a lot of saying, what was the reason? Who could explain it? And the guy who had the right idea was von Braun. I remember it so well, because I was in charge of all the measuring equipment. He just told me, since we had one spare platform, he told me, "Put it up, and put it on something which I can turn."

NEUFELD: Okay, this was--

REISIG: We didn't have anything to turn. So we took a box, a wooden box, and put the platform on it, and switched it on and powered the thing and everything for measurements and then we called von Braun. He came over and he just took two turns of the box, turned it quickly, jumped, the thing tipped over. That was the explanation. But that's typical von Braun. He had some imagination.

NEUFELD: Somehow he had an intuition--this story, this thing happened on Oie itself at the time, or did that happen shortly thereafterwards when you were--

REISIG: --afterwards--

NEUFELD: --when you were back at Peenemünde itself and talking about what happened, in the laboratory, so that happened in the laboratory. So just to backtrack for a second, von Braun asked you to go to be chief of the launches of the A-3's because he lacked anybody who knew in-flight measuring and he really needed you at that point to ?

REISIG: --see, I told you, when I came to Peenemünde, there were just concrete blocks for the test stands, and the instrumentation was ordered but it came in very slowly, so it wasn't very efficient in the first few months there. So von Braun got the firing in--on the Greifswalder Oie, the A 3 was planned, and he didn't have anybody to take care of it. So he assigned me manager of the Greifswalder Oie firing. I didn't have an idea what was involved. I just had to go on my intuition. And that included everything, building roads and shipments of the instruments and rockets and everything. It was very very amusing. But I learned a lot from that.

NEUFELD: Like what, what else did you learn from that experience?

REISIG: How to handle a missile. In the first place. And what supplies were necessary, and the power plant, and the propulsion supply and everything. Shipping problems. See, we had to bring the oxygen, the liquid oxygen from Peenemünde.

NEUFELD: It was about ten kilometers across the water or something.

REISIG: A little bit more, because the boat couldn't go straight ahead, it would run aground. It had to go around in some, what shall I call it?

NEUFELD: Deeper water.

REISIG: In a kind of straits.

NEUFELD: So Oie was pretty primitive, totally primitive when you first started the job?

REISIG: Like a farm. It was a farm.

NEUFELD: I know that later on you did quite a few launches from Oie.

REISIG: Oh yes.

NEUFELD: But this was the first.

REISIG: The very first, without any--and see, it was winter time, pretty moist, damp, and the ground there is clay. You can imagine. We sank in the wet clay with everything. So fortunately, the office which was responsible for the technical arrangements on the Oie had a light tower, you know.

NEUFELD: There was a lighthouse on it.

REISIG: The lighthouse, yes. And a little harbor and what not, and the supervision of all the things had a certain office in Straslund, "Wasserbaudirektion Stralsund," and I think from erecting this lighthouse, they had small tracks, 60 centimeter tracks lying there.

NEUFELD: You mean railroad tracks?

REISIG: Very small railroad tracks.

NEUFELD: Yes, small gauge railroad.

REISIG: And some lorries, and they permitted us to use these tracks, since we hadn't met any roads to use, from the harbor to the launching spot, we did our own trucking. I have pictures of that. So we could fight the weather to a certain degree. And things like that, I had to arrange.

NEUFELD: You had to--

REISIG: I had an agreement with the Wasserbaudirektion and things like that, all the way around.

NEUFELD: Was there a farmer there too at that point still?

REISIG: Ja. Actually, the Greifswalder Oie was a Domäene (domain, a state-owned farm).

NEUFELD: Yes, domain, as in--

REISIG: Belongs to the state.

NEUFELD: Okay.

REISIG: The Prussian state.

NEUFELD: Yes, originally it was probably a royal domain. And became a state domain. State lands.

REISIG: Yes. And the farmer on the Oie had a lease from the state, to use this domain. Of course, later on it was turned over to the army. The jurisdiction became--

NEUFELD: It became formally part of the Heeresversuchsanstalt Peenemünde or whatever the name was at that point.

REISIG: Called "Heeresgutsbezirk" administration, and army headquarters.

NEUFELD: Oh, I see. Okay. I didn't want to lose track of the control and guidance thing that we were talking about before, so at that point, soon after the failure of the A-3 to--the guidance to do a decent job--I gather that you went out and changed your contractor, right?

REISIG: No, well, in a subtle way, we did. To tell the full story, of course we had close connections with the Kreiselgeraete people and we knew their ideas and where we could, get the feel of their ideas on the A-4, but also we started to have a contract with Siemens. Does Siemens mean anything to you?

NEUFELD: Oh yes, of course.

REISIG: Siemens had already developed a two axis control system for aircraft, and so we thought, Okay, that's another source for supplying control systems. So we slowly started working with them. But actually, the Siemens system didn't turn out, and I think in late '39, with the improved platform of Kreiselgeraete, we could start firing the Kreiselgeraete platform in the new missile called the A-5.

NEUFELD: Right. With the A-5.

REISIG: With the Kreiselgeraete platform.

NEUFELD: Okay, so the A-5 when it was finally, I know the A-5 was launched in '38 without a platform, wasn't it?

REISIG: The very first. We just fired them up to see the stability of the missile itself.

NEUFELD: And then in '39 you launched A-5 with Kreiselgeraete--

REISIG: I think I have a listing of the firings.

NEUFELD: Well, so you have a complete listing somewhere of the launches as you've been able to reconstruct them?

REISIG: Yes, the firings with control system. No, I think even the first one in '39.

NEUFELD: The A-5.

REISIG: That's the A-5.

REISIG: I think two without guidance still. See, what we tested first with the A-5 rocket parachutes, in other words, recovery.

NEUFELD: October 19, 1939. Right.

REISIG: And there was no use to build, to put a platform in just for testing parachutes. And so I think we fired two without a platform.

NEUFELD:--even in '39. Because as I said, it's correct that there were firings in '38 of A-5's, or is that fact incorrect? I think that's what I've read somewhere. That already in 1938 there were a few A-5 launches, without guidance.

REISIG: It says here "without platform." According to my memory, it wasn't '38, but I'm not absolutely sure. I only remember, the first firing, vertical firing was with a parachute. See, that was a certain disadvantage of the military organization. It was set for a certain date, and everybody has to be in place, had to be brought over by boat to the Greifswalder Oie, and it was miserable weather. Fog all the way around.

NEUFELD: Right. Fog.

REISIG: And the commander of this whole thing, he said, "Well, we still fire, we have to find out." So we fired, and at about 100 meters the darned thing disappeared in the fog. And of course we heard the firing, the cut off, and then silence, nothing. Where did the darned thing go?

NEUFELD: Wait around and see if it would reappear.

REISIG: We were standing on a little platform ourselves to be above the ground, and everybody looks in a different direction to see, the darned thing must come down somewhere. And I happened to look in the proper direction, the little harbor of the Oie. And quite silently the thing came nose down with the parachute, in the water, and we had a boat out there immediately to recover it.

NEUFELD: But it was successful.

REISIG: Very successful.

NEUFELD: Parachute recovery was very successful right from the beginning on that. Okay. So as far as the guidance was concerned, you started Siemens in parallel developing another guidance system. Was that guidance, Siemens' guidance ever used in any of the vehicles or did you always stick with Kreiselgeraete?

REISIG: No, no, we mixed them. And just one more--yes, we started out with Kreiselgeraete, and I didn't make notes here, which one. But it can easily be found in archive reports.

NEUFELD: Right, it was sort of a comparison was being carried out.

REISIG: Yes.

NEUFELD: Experimenting with both, both firms.

REISIG: Of course, to get into the Siemens business we had to contact the air force, the German Luftwaffe.

NEUFELD: Right.

REISIG: Because we are using parts of the their aircraft control system. And then the air force people came up and they had another guy, a Dr. Mueller who developed another control system for airplanes, and he wanted to go with rockets, and just--I think at the end of April, '40, we had the first firing with the Dr. Mueller control system. But you see here, the case--

NEUFELD: That's Siemens, is it. Siemens Luftwaffe connected.

REISIG: No, Mueller was parallel to Siemens.

NEUFELD: Okay, you had another source, a third source.

REISIG: And then the navy came and wanted to go into business with us. And that was the most foolish thing you could think of. We have here this torpedo control system--wanted to take over our stuff. And we just looked, took a couple of torpedo control systems just in test stands and tested them with our static testing of the missile. The whole thing--we could have told from the very beginning. One of the very important things in control is the damping of motions of the missile, which is actually the stabilizing factor.

NEUFELD: Or it gets worse and worse, is that it? if it's not properly damped.

REISIG: See, the torpedo control system was designed for the damping characteristics of water, but we needed the damping characteristics of air.

NEUFELD: Right.

REISIG: And it just doesn't work together. And it was a hopeless case with the navy.

NEUFELD: What year was that approximately?

REISIG: '39, '40.

NEUFELD: So in effect there was a period, '38, '39, '40, where you were experimenting with a whole number of different platform concepts.

REISIG: See, that's one of the things in my book. I don't know, I don't remember, do you know the book of Hölsken?

REISIG: Oh yes, of course, I've read that.

REISIG: I think he wrote that our Oie firing was such a desperate failure because we didn't understand our own control problems. And so the development of the A-4 had to be dropped and we had to develop another small A-5. And that's not the story. I told you we knew why the first Kreiselgeraete platform did not work, what we had to change, and which concept to use, and we needed a test carrier for that, and it would have been foolish to build a huge vehicle just for testing control systems. It was much more efficient to use a much smaller, cheaper, easy to handle, just for the purpose of control development. But a fellow like Hölsken doesn't have a bit of understanding of things like that.

NEUFELD: The technological issues in the whole thing.

REISIG: Ja, and he thinks he can talk about it.

NEUFELD: Was it at the same time that you also decided to make the A-5 aerodynamically sort of a small A-4?

REISIG: Well, of course we knew that the aerodynamic properties of the A-3 were very unsatisfactory. It was designed in Kummersdorf before Dr. Hermann came in, the aerodynamicist, and he tried to correct a few things, but the best thing was to throw

out the whole configuration and start from scratch. And we had already initial measurements for the configuration of the A-4. So we said, "Why don't you take the same configuration for the A-5?"

NEUFELD: 1937, '38, is that when you decided?

REISIG: No, '37 in December we had the Oie firings, so in early '38.

NEUFELD: Right. I mean, when was the sort of basic aerodynamic shape of the A 4 worked out? Was it already in '36, '37, or was it a little bit later?

REISIG: I think at the very beginning in '36. You see, Dr. Hermann was assistant professor in Aachen in the Technical University, and he had built the first supersonic wind tunnel with Professor Wieselsberger over there, and they had a contract with us to start already aerodynamic investigations. So the configurations of the A-4, the very beginning was started in Aachen in the small wind tunnel, 10 centimeter.

NEUFELD: Okay. That was in '36 already.

REISIG: It started in '36.

NEUFELD: So the basic shape of the A-4 was decided on the basis of the supersonic wind tunnel at Aachen, the 10 centimeter by 10 centimeter.

REISIG: Yes, the very first measurements. We did millions of measurements in the wind tunnel at Peenemünde, and it was--

NEUFELD: But that was finished in '39.

REISIG: '39, yes.

NEUFELD: Right, so that the A-4 was--I gather from your article somewhere that the basic shape was based on an artillery shell.

REISIG: No, an infantry bullet.

NEUFELD: Infantry.

REISIG: A bullet, ja.

NEUFELD: It was based on a bullet shape.

REISIG: The so-called "S" bullet, which was proved by millions of tests by the army that it was very effective. So we took the

basic design. We had to start somewhere.

NEUFELD: Right.

REISIG: We wouldn't do it any more these days.

NEUFELD: Yes, right, but since you had to work empirically.

REISIG: Sure.

NEUFELD: Right. But of course that left the fin design to be done. That was worked out in that period, '36, '37, '38, somewhere in there in Aachen?

REISIG: I think the beginning was in Aachen, but the extended measurements were done in the Peenemünde wind tunnel. It's a pretty tricky problem, the configuration of fins.

NEUFELD: Yes, throughout a number of Mach numbers and different atmospheric pressures.

REISIG: We started subsonic--yes, well started out at low Mach number 1 and went through the whole range.

NEUFELD: Right.

REISIG: And we found it had to be stable in all ranges.

NEUFELD: And a range of atmospheric pressures too because of different heights.

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NEUFELD: There were some other issues that I wanted to pursue, but while we're talking about aerodynamics, it's an appropriate time to talk about Dr. Hermann, I guess.

REISIG: Yes.

NEUFELD: I gather that you had a close personal connection of some kind.

REISIG: Ja, we went to the same school, the same gymnasium in Leipzig. I knew him from the time as a student.

NEUFELD: He was a few years older than you.

REISIG: Yes. He was. But his father was our music teacher.

NEUFELD: In the Gymnasium. Was it by accident that somehow the

two of you came back together again at Peenemünde?

REISIG: No, he more or less got me the position. You see, von Braun was looking for a measuring specialist, and Hermann knew about my background, and so he suggested I might go.

NEUFELD: Can you describe him as a person, as a personality?

REISIG: Ja, well, he was different from von Braun and from Thiel. Thiel particularly was what you might call an extrovert.

NEUFELD: Thiel was an extrovert.

REISIG: Ja. He could get quite excited in certain situations, in a positive way. But Hermann is a typical scientific breed. Much quieter, but very thoughtful and very thorough in his work. Yes, and he was very concerned about his associates. When somebody did a piece of work--

NEUFELD: This was Hermann--

REISIG: Yes, particularly one friend of mine, Dr. Heybey, a physicist, he told me, when he brought a piece of work to Hermann, he would talk with him for hours about the outcome of his work. He wanted to understand and give suggestions and what not. So he took his time to appreciate the work of his people.

NEUFELD: Just thinking about Thiel and Hermann, were they easy people to work for? I get the impression from Dornberger's book that Thiel was very very brilliant, but sometimes a little hard to--Dornberger gives the impression that he was perhaps sometimes very upset or excited about what was happening so that he would be a difficult person to manage sometimes.

REISIG: Sometimes he could shout at you, ja, but he didn't mean it. It was just his temperament. Von Braun you know, he was a typical gentleman.

NEUFELD: In the case of von Braun, obviously he had a certain genius or talent for managing people.

REISIG: Right.

NEUFELD: That's natural. Did it also come from his background?

REISIG: Sure. What do you call it, a charismatic--

NEUFELD: Charismatic, a charismatic personality.

REISIG: Of course, his background, from the nobility, helped him

a lot.

NEUFELD: Yes, would you describe him as very, I don't know, very polite, very--

REISIG: Extremely polite.

NEUFELD: Extremely polite, extremely well mannered.

REISIG: And he accepted everybody. Not that he thought, "I'm the big man, but all you--" But he went into the workshops and talked to everybody, every worker, ja, shook hands and "How are you, how's your family, what are you doing, could you explain it to me?" No barriers of any social kind.

NEUFELD: Yes, maybe this is--

REISIG: It made him very effective in managing people.

NEUFELD: This is probably a good place then to talk, I wanted to ask you about his management abilities and things, building on what you wrote in your articles, so that in terms of his position as a--of course, at least, formally he was a Freiherr, he was a baron, he was a Junker essentially.

REISIG: At least his family, his father--his father was much more like that.

NEUFELD: Did you meet his father?

REISIG: Yes.

NEUFELD: At the time?

REISIG: Yes, he came visiting, and when we came over here in '45 we had the provision, apartments, and von Braun's parents lived very close to our people, came over very frequently, liked our children and we had very nice relations with them.

NEUFELD: Yes, because I gather from reading the elder von Braun's biography, autobiography. I've read his autobiography, and I get the impression that he was in some ways very old fashioned. That is, his views were shaped by the Imperial period.

REISIG: Sure.

NEUFELD: By the Kaiserreich, and that was his world. Did he have a problem, did he find it difficult at first to accept Wernher von Braun being an engineer in technology? I think

Dornberger says that, at first, maybe.

REISIG: The way the training was, von Braun was a physicist.

NEUFELD: Right.

REISIG: He got his degree in physics.

NEUFELD: Yes, that's true.

REISIG: Also he had attended the Technical University both in Berlin and in Zuerich. But he stuck to it, he got to the--

NEUFELD: University of Berlin--

REISIG: University of Berlin in physics, at the university. I couldn't remember that his father was--of course, later on he was very proud of his son.

NEUFELD: Right. Naturally. I mean, that would have changed very much. Dornberger just says in the early part of his book that maybe in 1933, when he started, that von Braun's father said that he was a little mystified as to how he came to you know, to science, to rocketry and so forth.

REISIG: You know what the reason was? His mother?

NEUFELD: Yes, von Braun's mother.

REISIG: When von Braun was confirmed, as we call it here, he was officially accepted in the church as a member, which was in our case when we were 14 years old, his mother gave him a telescope. So he looked at the sky. Everything came from there.

NEUFELD: Yes, I get the impression from reading that and from reading things that von Braun said and from reading his father's biography that his mother had a real scientific orientation. Maybe in some ways that she was the stronger influence of the two on his interests.

REISIG: Might well be. Might well be.

NEUFELD: So you don't have an impression of that from later on very much I guess, whether his mother had been the real influence in his life.

REISIG: It's understandable. His father was the typical administration type, Beamter.

NEUFELD: Prussian civil servant.

REISIG: Also he was a Grossgrundbesitzer.

NEUFELD: Yes, he was a large landowner.

REISIG: He was minister in the cabinet of von Papen and things like that, and he was Oberpräsident, I think, in the government of Ostpreussen and things like that.

NEUFELD: Yes, he was a high civil servant in Prussia.

REISIG: He studied law. And Wernher had the typical scientific orientation. So the two of them didn't give much to each other, and I think it was mostly the influence of his mother.

NEUFELD: Yes, it seems that way. It's hard to verify now. There isn't much, at least so far, I don't know much correspondence or anything. They lost so much.

REISIG: Do you know the book by Ruland?

NEUFELD: I've seen it. I haven't yet had a chance to go through it. Brent Ruland, Mein leben für die Raumfahrt.

REISIG: Yes, I think he wrote a little bit more in detail about these things.

NEUFELD: Okay, I'll have to remember that. So I had wanted to pick up on that, which you said before, so that, as far as Wernher von Braun's background was concerned, the upbringing made him very careful, very polite and so forth, but he wasn't formal or he wasn't, he wasn't haughty, he wasn't arrogant, in a sense that an aristocrat--he wasn't any aristocrat in that sense.

REISIG: He wasn't arrogant. He was too smart to be arrogant. He didn't need it. What for? He could talk to everybody as if his father were the president of the United States, it doesn't make any difference to him. He always knew how to behave.

NEUFELD: You know, given that background, he could have been a very, in a sort of arrogant kind of person, as a Junker or at least you know, and he seems not to have had that characteristic at all. So do you think, I think in one of your articles you say that this organizational plan of laboratories divided by subjects was essentially his idea, is that true?

REISIG: You see, the great gift, I think I put it out in the green write-up, that--

NEUFELD: Yes, in the second article.

REISIG: That he had a natural talent for systems organization. You see, at that time we didn't call it systems.

NEUFELD: Right.

REISIG: But he particularly did it, and he had the talent to bring everybody in contact with everybody else, and let--for instance, with my measurements is a typical case. I had to understand the physical effects and I had to measure. I had to talk to the Thiel people. And I had to talk to the structures people and what not, and all this went forth and back, so we hardly could do anything essential that way, but von Braun was successful.

NEUFELD: Seeing how to keep the horizontal connections essentially between different groups going.

REISIG: Well, now, I put it here in the first layout, introductory chapter. Of course I was for a few years professor of cybernetics in Tullahoma, in the Space Institute. That's what it was.

NEUFELD: So that effectively he did not want to have a hierarchical system, where everything had to move through channels vertically.

REISIG: No. That's the worst thing you can do.

NEUFELD: Because you see, the thing that's really interesting about that is that one associates firstly that kind of hierarchical bureaucracy with the army, with the military, and secondly, it even seems German in some ways to be that sort of structure. You know, but in fact you seem to be saying in your articles that it was in the United States where vertical bureaucracy was emphasized--

REISIG: Yes.

NEUFELD: --and in Germany where you have this, like a stereotype of the Germans, that the rigid hierarchical systems, where it did not exists, that it wasn't like that at all.

REISIG: There's another factor which is overlooked. As far as my experience goes, even in a large organization like Siemens, the chiefs of the different levels have pretty much freedom in making decisions. They didn't have always to go to the highest officer. If you were sure of yourself and you were doing something reasonable, you had authority to make decisions. And from our experience, in our judgment, I'm sorry to say, that's a

weakness of the American management. The almost military strictness of the hierarchy.

NEUFELD: Where did you see that most of all here? Was that in the corporations? Was that in the government?

REISIG: In the corporations, and for instance in NASA Headquarters.
NEUFELD: And--

REISIG: Of course we had trouble with NASA Headquarters. Particularly von Braun, ja. When he did something which he thought was more or less an optimum solution for a certain problem, von Braun made a decision, "Okay, we do it that way." And the some kind of a stubenhacker in Washington, "I want it the other way." Then von Braun started fighting, fighting back.

NEUFELD: This was in the 1960's.

REISIG: Prove it that you know it better. Ja. Then they-- (crosstalk)

NEUFELD: That was in the 1960's. Basically you're talking about conflicts in the Saturn program--

REISIG: Ja.

NEUFELD: --that you have over the bureaucratic organization.

REISIG: And the same story with the industry.

NEUFELD: American corporations.

REISIG: The worst case is Thiokol with the Challenger.

NEUFELD: Morton Thiokol, right.

REISIG: Just from the rigid and, in our opinion, stupid hierarchical decision making.

NEUFELD: Yes, the problem with the (O ring) seals was buried at some level here at Marshall, right, and they wouldn't talk to anybody else about it.

REISIG: Yes, a lack of communication, yes. And von Braun, as I tried to explain, had an optimum communication because he talked to everybody, and said, "Well, you talk to that guy, he knows about it." Contacts between each level.

NEUFELD: Is that a matter, that organizational concept,

difficult to maintain without a brilliant personality like von Braun?

REISIG: That might well be. But it's interesting. You see, before we went over to NASA, we had this Army ballistic missile agency, ABMA, and we got General Medaris as chief of, the big shot there. And he attempted for himself von Braun's methods. He can talk pretty harsh about his past experiences with the Army, and of course what happened in NASA, and in one of the speeches he said, "Well, I don't understand what happened with the Challenger. When I was the top of ABMA, I knew about every detail."

NEUFELD: And you think that he essentially learned that lesson from von Braun's concepts, management. To backtrack to Peenemünde, then, did von Braun's concept clash with the army's way of doing things at all? Or were you sort of isolated from that?

REISIG: It has two aspects. The first phase was in Peenemünde, certainly Dornberger was a genius too. And he protected us from the army bureaucracy. He always said, particularly after we had successes, that we didn't do it because of army regulations but in spite of army regulations, and that was his merit.

NEUFELD: That he saw the--

REISIG: Pushed the army bureaucracy back and said, "I'm responsible, and I do it the way we do it best."

NEUFELD: Do you think his contribution was, did he make a significant contribution to the concept of management, or was his thing basically protecting von Braun and letting von Braun decide--

REISIG: You see. von Braun started he was a very young fellow.

NEUFELD: Right.

REISIG: He was inexperienced, and he had so to speak a tutor in the person of Dornberger. Von Braun was told that, little boy, "We do this this way and that way," until it was realized, it was the right way. And I think it was a very understanding relation between the two. And then, all this political crap started during the war, and finally, Dornberger was more or less taken away from Peenemünde, and was assigned all this military preparation for the deployment.

NEUFELD: Right.

REISIG: And that, in a certain way then, the trouble started, because Dornberger of course was a member of the Heereswaffenamt

NEUFELD: Right, the army ordnance.

REISIG: And when he was taken away from Peenemünde, the Waffenamt wanted to regain control of the whole Peenemünde project.

NEUFELD: Right.

REISIG: So they established a new military division, so to speak, Wa Pruef 10 under Rossmann.

NEUFELD: Number 10 when Gen. Rossmann came. That was about--

REISIG: And they tried to introduce this army bureaucracy, and von Braun had a pretty hard time to preserve our concept of everything, and finally the Wa Pruef 10 people had to give in, because didn't know a darned thing about it.

NEUFELD: So this was, I gather from reading Hölsken and other places that that was about September, 1943, that this Wa Pruef Zehne, new division was created, and Dornberger was taken away.

REISIG: Let's see, when--

NEUFELD: It would have been just after, only weeks after that first air raid.

REISIG: Yes.

NEUFELD: That date is correct as far as you remember it?

REISIG: I don't know, Dornberger was still commander in Peenemünde when the air raid occurred. It was on the 17th of August, '43.

NEUFELD: Right.

REISIG: And it happened shortly after that, in fall, '43. I got my first connection with the Dornberger staff around October,

NEUFELD: Okay, October '43.

REISIG: It was a kind of liaison then, for the preparation of the deployment. I think we had documents which should tell the date.

NEUFELD: Yes, that certainly should be settled. I certainly

want to get to your period on the Dornberger staff a little later. But as far as you know, as far as this argument of the army bureaucracy versus von Braun's management concept, you feel that that almost totally was a conflict that appeared after 1943 or so.

REISIG: After Dornberger was gone, yes.

NEUFELD: And so, were relations with Rossmann not very good when he came in as chief of Wa Pruef 10?

REISIG: He was a typical army general. Dornberger was a technical man.

NEUFELD: He was an engineer, doctor of engineering.

REISIG: In spite of his army rank--so he understood our language, which he always emphasized. You have to understand the language of the people who are working for you.

NEUFELD: Right.

REISIG: Which Rossmann didn't do at all. He was much too much army.

NEUFELD: That's interesting. You see, in Huzel's book, From Peenemünde to Canaveral, he says that there was resentment of the army officers, but he only came in July, 1943, to Peenemünde, so maybe that coincides very well with what you're saying.

REISIG: Ja. Now, actually there was no reason that the situation was controversial, but as I know from my own experience, there were officers on General Rossmann's staff who absolutely didn't own any discipline. They considered themselves the big shots now in Peenemünde.

NEUFELD: They had no technical training--

REISIG: --whatever, and I remember very well the worst of them, who was a communications officer, and he went so far to send his lieutenants out in the field to our radio stations and give the people there orders what they should do. And our management didn't have any idea of what was going on, until after the fact. From such military interference things went haywire. In the Deutsches Museum is the draft of a letter von Braun wrote to Dornberger, even after Dornberger was gone, in which he's complaining bitterly about situation.

NEUFELD: About this interference by Army officers in the organization. So you say, just to make a passing reference in

that later article, that von Braun was sometimes accused even in the Peenemünde years of being a bad manager, and sometimes it was by people who were wearing, I think to use your words more or less, "brown or black uniforms."

REISIG: I think it's even written in the Dornberger book, the judgment of Kammler about von Braun. "This young fellow, who does he think he is" and "absolutely incapable of directing such a project." The trouble was, he couldn't live with Kammler who was in command of the whole deployment in the field.

NEUFELD: Kammler would have been helpless without having von Braun there still in a technical capacity so as far as you can remember, none of these accusations about von Braun's management or anything else occurred before 1943?

REISIG: Dornberger had proper ideas on von Braun and gave him a kick here and there, but it was always, I should say, in a friendly way, and just helping von Braun to get experience.

NEUFELD: You say in your article, you say in your interview before that he thought von Braun was too interested in non-military applications, or something?

REISIG: Well, Dornberger should blame himself on that. He was a Raumfahrtenthusiast to begin with.

NEUFELD: Dornberger was, was a space enthusiast.

REISIG: Dornberger, yes, so he met the right guy in von Braun. Basically they had the same ideas. But of course, von Braun was so interested in everything that he was a little bit in danger of going in a sideline, and Dornberger had to see that the whole thing came to bear, and so he had to call him back and say, "Go on the main line and forget about your sidetrack, until after the war."

NEUFELD: Don't think about satellites and space flight now, you have to produce a weapon.

REISIG: Even to put another gadget on the rocket, which von Braun thought it would be nice to have. Dornberger said, "No, no; no time now for doing that."

NEUFELD: Are you talking about just new developments, terms of refining the technology or?

REISIG: Refining and--

NEUFELD: Refining it to make it a better vehicle, but not

necessarily something you could make sooner.

REISIG: Ja. Takes manpower, takes time, not to speak about money, money was secondary at Peenemünde.

NEUFELD: I know that Dornberger sometimes could criticize him, so you didn't feel even in those early years that Dornberger at times was unreasonable in criticizing you or the team as a whole or von Braun for being too concerned about these other things?

REISIG: In a certain way, of course, he had a military tone, yes, and then he could get kind of hard but actually in a friendly way. I remember a number of occasions when we had a failure with a test, and of course Dornberger was disappointed too, and then he said, "Macht Holzpantinen!" You know,--

NEUFELD: Could you translate that for the record?

REISIG: You know, "wooden shoes," you could manage that but not a missile. It was a momentary--

NEUFELD: He could be sarcastic. I know, because for example I had this document which I found in the files which is a memo from Dornberger in the beginning of '42 which is kind of harsh over some failure, and I just wondered, you know, whether that kind of thing was just in some ways as you say just a passing thing with him, push you in the right direction, or did it cause resentment?

REISIG: You have to realize that with time going on, Dornberger became nervous.

NEUFELD: Yes.

REISIG: See, he had a responsibility to the highest command of the army, that something would come out from Peenemünde which could be used, and of course, I would have to read it in real detail, but I think that it's some kind of a reaction to something that went haywire, and he more or less expressed his nervousness about it.

NEUFELD: He mentioned some failure on test stand 1 which is the engine test stand in January--late January, beginning of February '42.

REISIG: I don't remember that one. But he, for the first test firing of the V-2, in test stand 7, and that was a very bad disciplinary or non-disciplinary matter, that the test engineer, the responsible test engineer at Prüfstand 7 fired this missile in a static test on his own initiative, and he was not allowed to do that. Of course the test went wrong. And of course on such

things Dornberger could get very mad, with good reason.

NEUFELD: Was that the very first missile, the V-1 and it exploded on the ground, I gather.

REISIG: Yes. Right.

NEUFELD: Was that a static test or an attempted launch?

REISIG: No, it was a static test.

NEUFELD: Static test and the first one blew up.

REISIG: Yes.

NEUFELD: And that was due to the carelessness of the--

REISIG: --of the test engineer. Certainly the arrogance of this fellow who thought he could do it by himself.

NEUFELD: Who was that?

REISIG: The name's on the tip of my tongue, but--

NEUFELD: How do you spell that?

REISIG: As far as I remember, Ferenberger.

NEUFELD: That may appear in the documents somewhere. It's not terribly--

REISIG: It may well be that there is an h after the r.

NEUFELD: Okay. But as far as you were concerned, this kind of communication, that he might get angry about a failure or something, he was not resented for that kind of thing. That was seen as justified or at least passing.

NEUFELD: Dornberger was a good fellow. He particularly protected me. I mean, I can't blame him for anything. For instance, I got decorations when I was on his staff, and he called me in, in his office, and put the decoration on, and he said, "I give this to you. The people in Peenemünde are forgetting you anyway." It was typical Dornberger.

NEUFELD: He also, like von Braun, had a good sense of managing people, keeping them happy.

REISIG: Ja. And considered what you'd like to call it--

NEUFELD: Considerate?

REISIG: Considerate in his contact with people. He was smart enough to say, "Well, I depend on these guys. What would I do without them?"

NEUFELD: Okay.

REISIG: And he was convinced that we were trying to do our best.

NEUFELD: So if he occasionally would write a memo saying--

REISIG: --bad guys--

NEUFELD: --"this is the old amateurism from Raketenflugplatz" or something, you wouldn't take that very seriously, as far as that is concerned. Okay. We've ranged pretty far afield. I want to come back to a couple of things that I haven't had a chance to ask you. One was, regarding the Versuchsserienwerk, the pilot production plant or something. Arthur Rudolph says in his book, the Franklin thing, that Dornberger sprang the idea of producing, mass producing the missile on him and von Braun on Oie on December, 1937.

NEUFELD: Not building on Oie, but when you were on the Oie in December, '37, that he told Rudolph to build the facility.

NEUFELD: Rudolph and von Braun had said, "We should build the production at Peenemünde." You weren't aware of that conversation when you were involved in launching the A-3?

REISIG: Well, of course we heard about it and we talked about it, but I was not present in this discussion.

NEUFELD: Yes, because according to Rudolph, that was the first time the idea of producing the A-4 at Peenemünde came up.

REISIG: Yes, but you have to distinguish--

NEUFELD: Yes, producing and the sense of mass producing--

REISIG: Ja, but actually, that was not conceived for mass production, but we realized that we needed quite considerable a number of missiles for our testing and, I don't know what the English expression--you see, in artillery and actually with any firing weapon, in the armies, you have to fire so and so many rounds to establish the firing tables. And we gathered, well, it would take several hundred missiles. And that was the idea of the Versuchsmuster. The reason they called it Versuchsmuster.

NEUFELD: The test.

REISIG: This was for testing.

NEUFELD: That's not the same as the--

REISIG: But not 5000 missiles a month.

NEUFELD: Yes, kind of crazy figure. So you had already the idea though of developing, you had a separate production, right, small scale production--

REISIG: Very small, ja.

NEUFELD: For the Versuchsmuster 1, 2, you know, the test missiles, right. So that this other plant, the Fertigungsstelle or whatever the pilot production plant was, it was started with the idea of producing missiles for test and then changed over to a bigger production?

REISIG: See, without the war, there wouldn't have been the idea of mass production. We would just have stayed with our Versuchsmuster.

NEUFELD: So you said something that has to be established--you were not connected at all with this pilot production idea.

REISIG: Eventually they would have come to me and asked certain questions, what kind of implementation you should put in and things like that. But the actual build-up of concepts of the Versuchsserienwerk, I was not involved.

NEUFELD: Okay, so I guess that will have to be discovered from other sources. Another thing that I wanted to cover was, we talked about the guidance problems in 1938, '39, '40 and so forth. Was that your main work in that period of time, after you came to Peenemünde. Was your involvement in guidance problems, or what other things were you doing?

REISIG: Well, in--

NEUFELD: In 1942 or early '43.

REISIG: Of course, in the very beginning, particularly when Siemens came into the picture, I was in charge of the measurements of the control systems in static firings. There was nobody else on board yet to take care of that. As a consequence, we had to build up a particular control and guidance division, of which one of the essential parts was the theoretical group, with Steuding at the head. He was another genius. Steuding.

NEUFELD: So he was the head of the theoretical division.

REISIG: And later on he was responsible for the whole control development, and he with his team of theoretical people, first of all generated the guidance theory that was not existing.

NEUFELD: That was about when, chronologically?

REISIG: He came to Peenemünde in summer of '39.

NEUFELD: Summer of '39. Did he come from the university?

REISIG: Well, originally he was a Privatdozent.

NEUFELD: Yes, here it's roughly equivalent to associate professor.

REISIG: No, not quite. In the technical university. And from there he went to the Deutsche Forchungsanstalt für Segelflug.

NEUFELD: Which is sail flight, sail planes, gliders, Okay.

REISIG: And from there he came to Peenemünde.

NEUFELD: Because I'm sort of interested in the university connection. It seems that an essential part of your buildup of the team was--I don't know whether it was just von Braun or whether a number of people were reaching out to both use the university institutes and to attract people.

REISIG: It all started out when we realized that once we had to accelerate our development, of course we needed help for that. We couldn't pull anybody out of industry because the people essentially were not there, as I tried to explain. The natural way to go was to the universities. You might have found in reading that we had this "day of wisdom" in Peenemünde, I think it was in September, '39, when we invited professors we knew from our own studies, invited them to Peenemünde, and told them the whole problem, and invited them to take over a certain part of the development.

NEUFELD: That was the famous "Tag der Weisheit." That was September, '39?

REISIG: I think it was September.

NEUFELD: So most of the university connections started with the war, is that right?

REISIG: Right. Except for Wieselberger, I can't remember--oh yes, Professor Hase.

NEUFELD: Hase?

REISIG: Hase, of the Technical University of Hannover.

NEUFELD: And you mentioned the first person, that was Hermann's--

REISIG: Wieselsberger.

NEUFELD: Okay, he was Hermann's institute director.

REISIG: Right.

NEUFELD: Okay. So essentially prior to the war the university institutes were not used very much.

REISIG: No, just Hase and Wieselsberger is what I remember.

NEUFELD: What was Hase again, what was his specialization?

REISIG: He was professor of engineering physics. A very capable man and very intensive worker. He had even a small firm of his own to build measuring, particularly temperature measuring instruments.

NEUFELD: And that's what he was basically involved in, measurement, with you, or did you?

REISIG: No, he stayed mainly in the field of power plant measurements. Essentially. He did some measurements of material testing too, I remember. But I had a particular connection with Hase because when Hermann told von Braun I should come to Peenemünde and join them, von Braun of course said, "Who is he?" And he couldn't say I was a friend from the Gymnasium. So von Braun got the idea, I should go to the technical university of Hannover where Hase was a professor, and Hase should evaluate me, so to speak, so I had to make a trip over and we spend a day together, and he made it in a very clever way. He took me through his institute and showed me every experiment and asked me questions, "What do you think about this? What do you think about that?" And he evidently was impressed by my responses and recommended to von Braun that he should hire me.

NEUFELD: Was Hase working already with Peenemünde then? At that time?

REISIG: Yes.

NEUFELD: On the measuring (crosstalk)

REISIG: I think he started out with Kummersdorf already.

NEUFELD: In Kummersdorf he started with measuring engines, testing?

REISIG: Particularly I think at that time in certain kinds of temperature measurements.

NEUFELD: Okay.

TAPE 2, SIDE 1

NEUFELD: I wanted to just finish up. I was asking you about all the different activities that you were involved in between say 1938 and 1942, early '43, when you left Peenemünde. So what other things were you involved in? You were still involved in test stand, engine test stand measurements throughout that period or not very much?

REISIG: Practically when Thiel came to Peenemünde and disbanded the Kummersdorf facility, I switched over to measuring systems on board the rockets, because there wasn't anybody else to do it. Keeping at the same time the test stand measurements would have been too much of a work load. So I gave this to this fellow Haackh in Thiel's team and started on board measuring systems. Now, I started very modestly, and of course we wanted to have recordings of the functioning of the missile on board, in flight. And there was nothing. So even for the A-5 firings, the first thing we developed was an oscillograph with a film camera which was put in a very heavy enclosure, which could take the many measurements and put it from the--

NEUFELD: You had a film strip then?

REISIG: Right.

NEUFELD: A series of, a movie of the oscillograph display.

REISIG: Right. And we could measure many physical quantities by commuting the input of various measurements, giving each quantity so much time, therefore, switching from one quantity to the next one, and then it came around, and started the whole sequence again. It was the very first thing, and the very first missiles in Oie with control systems were equipped with this, I should say primitive way of recording.

NEUFELD: That was in A-5 launches. Now you're talking about A-5. So you recorded.

REISIG: Right. You can see here the firing records, listing the types of measurement.

NEUFELD: Yes, you're looking at your records of the launches, right.

REISIG: Practically every one, and it was the first thing. At the same time, we realized that we needed a very accurate or very capable tracking system for the missile, particularly for checking the operation of the control system during flight, the behavior of the missile, not only inside the functioning of the control system as such, but all of the flight mechanical effects on the control system, and of course we had to do it from outside the missile by some appropriate tracking system. It turned out, all right, at that time that the tracking systems which were available were absolutely not up to par.

NEUFELD: Are you talking about optical tracking or radio?

REISIG: Both. Also, we got the most advanced optical equipment for tracking and it turned out not to be sufficient. So we developed a very special radio system which utilized the so-called Doppler effect.

NEUFELD: Right, I understand.

REISIG: That was actually the idea of Professor Wolman from the Technical University of Dresden.

NEUFELD: Yes, he was in Dresden. And that was developed about when? Was that already after the beginning of the war, '39?

REISIG: Yes, we started pretty early. But again we didn't have anything to copy from. Our first problem was the stability, the frequency stability of the transmitters. What we actually did, we wanted to compare the difference between the frequencies received from two transmitters, one transmitter on the ground and one in the missile.

NEUFELD: Right.

REISIG: The frequency received on ground from the transmitter on the missile is subject to the velocity effect which generates the Doppler effect. We had several contracts. For instance, one with the Technical University of Berlin, and one with Darmstadt for the consistency or constancy of the transmitter frequency, and that is a terrific problem. We actually didn't ever come to a real usable solution. Wolman had the idea to use just a transmitter on the ground, receive it up in the missile, the

frequency already modified by the Doppler effect. Then using the received frequency to control a transmitter with twice the frequency of the surface transmitter, then sending the doubled frequency back, and compare it with the transmitter on the ground. So you eliminate at once the constancy problem. The problem of frequency constancy is solved by taking the difference of the two transmitter frequencies because what you send out makes the same fluctuations in frequency as what comes back.

NEUFELD: Okay, so for the convenience of measuring that, the signal was doubled.

REISIG: Right.

NEUFELD: The signal frequency was doubled inside the missile.

REISIG: Right.

NEUFELD: So that provided, whatever it was. Of course it varied according to the Doppler effect. Whatever it was, it was doubled by the missile in the Wolman system.

REISIG: Yes.

NEUFELD: And then coming back--

REISIG: --transmitted back, yes, and compared with the doubled frequency of the surface transmitter.

NEUFELD: So that you would have to then calculate the actual velocity from the Doppler effect measured twice.

REISIG: It was velocity in the line of sight, which we could measure directly because that is given by the Doppler effect, and we utilized that and it was real funny, when we had visitors, high ranking visitors to show them how fast our missile went or how much faster it went, and the cutoff came when the missile gets slower and so forth. You just listen to this Doppler tone. So we just had a large speaker in the room there and listened to the Doppler tone.

NEUFELD: You hear it, it rises.

REISIG: A natural equivalent for the missile velocity. But we had to indicate the velocities in other receiver stations on the ground, to track the trajectory.

NEUFELD: Right.

REISIG: You obtained the distances from each receiver station to

the missile. The integral of velocity is distance.

NEUFELD: So you had to eventually triangulate the thing by how many receivers, two, three?

REISIG: We needed at least three. Sometimes we put five up. Sometimes for a certain receiver, the direction of reception was not optimum, because of the location of the missile, and then we substituted by another receiver which was more favorably located. And that was a tremendous success.

NEUFELD: In the long--

REISIG: As I mentioned to you before, we needed this tracking for checking the control system. The accuracy of this Wolman system was really such that we could do that. The Wolman tracking system was more accurate than the control system in the missile.

NEUFELD: Is that why you chose to initiate the cutoff, Brennschluss, from the ground?

REISIG: Right.

NEUFELD: Was because you felt that you had a better measurement of the trajectory and the velocity of the missile than--

REISIG: I think, as far as I can remember, we really achieved a tenth of a percent to measure both the velocity and the coordinates.

NEUFELD: How was the integration of the Doppler effects with the various stations done?

REISIG: That was a problem--

NEUFELD: --on the ground--

REISIG: First of all one of Wolman's co-workers had the idea to give these frequencies of all the stations on little thing devices called synchronous motors. The speed of the motor would be exactly according to the frequency fed in.

NEUFELD: Okay.

REISIG: But that wasn't very easy to do either. You wouldn't believe it--we did it very primitively. We oscillographed these Doppler frequencies on paper, and just counted them. A tremendous piece of work, but just as exact as it could be. We saw exactly what happened at every point of the trajectory.

NEUFELD: So how then did you determine the cutoff point? That wasn't a manual command, was it?

REISIG: It was even done electrically. We calculated for a certain distance, you want to fire, the corresponding cutoff velocity, and we just turned this calculated frequency in a bridge, actually it was a Robinson bridge, electrical bridge, just on the notch. Then we fed the received, actual Doppler frequency into this bridge, and when it coincided with the turned frequency, a relay clicked, and gave the command--

NEUFELD: --automatic command when you compared the two velocities. Okay. So you were at that time, or at least later on, experimenting with accelerometers, right, as an alternate way of having an on board?

REISIG: Right. Of course we to get away, for the employment in the field, from any radio communication.

NEUFELD: Right.

REISIG: Because we were afraid that it would be jammed and distorted by enemy action. Which by the way never happened. That's something we can't understand.

NEUFELD: The Allies never did figure out the--

REISIG: We just can't understand it. Of course, the system with the accelerometers would be what we call inertial.

NEUFELD: Right.

REISIG: In other words, independent of any radio connection.

NEUFELD: Did you finally integrate accelerometers?

REISIG: Yes, we did.

NEUFELD: In some models or some flight models?

REISIG: The ideal thing would be to do inertial measurements in the three axis directions. In other words, you would need three accelerometers.

NEUFELD: Right.

REISIG: But the development, for reasons I still don't understand, was so slow of these accelerometers for measuring in three directions, three axes. But one of the fellows in

Kreiselgeraete had a genius idea. He used an unbalanced gyro to measure the acceleration in the direction of the trajectory.

NEUFELD: I'm not quite sure I understand what you mean by an unbalanced gyro. Can you illustrate that at all?

REISIG: Well, the idea was, if the gyro gets the force perpendicular to what we call its sensitivity axis, it starts processing, in the perpendicular direction, according to the measured acceleration, in other words, the force of the acceleration. So this gyro during flight of the missile started rotating, and since you measure at every instance, the angle of rotation is already the integral of the acceleration.

NEUFELD: Okay.

REISIG: We just had a little device to say, this gyro has to make so many revolutions to reach the calculated velocity, and then gives the contact, which in turn actuates the Brennschluss. A real genius idea. But since we didn't have the platform yet to measure in three directions, axial directions, we measured only in the direction of the trajectory.

NEUFELD: Along the axis of the missile.

REISIG: A little bit more in the direction of the tangent to the trajectory, which is not necessarily the axis, it's just in the direction of the motion of the center of gravity.

NEUFELD: Okay. And as I understand the base of the pitch gyro was driven mechanically to force the missile into the 45 degree elevation of the trajectory direction, is that right? That gyro was through clockwork mechanism tipped? I'm not talking about the accelerometer gyro now. Okay, so I obviously did not understand this

REISIG: No, I think what you mean is in the direction of flight. See, we started perpendicular.

NEUFELD: Right, that's what I--

REISIG: --and it turned in a more or less 45 degree direction, and that was done with turning the base of the gyro which was responsible for the flat plane.

NEUFELD: Right, that's what I was saying. I wasn't making myself clear. That gyro was measuring that--so that the platform that you had for A-4 was controlled on two axes, i.e. two gyros, and did you try to go toward three axis control?

REISIG: No, we essentially used the Siemens system, even in the missiles which were fired at the enemy, and we didn't have the proper platform yet from Kreiselgeraete for the V 2. So we had the simplified system. In English we say it's "downstrapped." That was a plate which carried two gyros. One gyro was for the flight plane direction, and the other gyro could make two measurements, two separate measurements, one for the lateral deviation which we call yaw.

NEUFELD: Yes, yaw.

REISIG: And for the roll, the roll motion. This was combined in one gyro, so we had two gyros just in this simple plate which was fixed to the frame of the rocket. And in addition, we had this integration gyro which we called it then for measuring the velocity.

NEUFELD: That was incorporated mostly later on, I gather, towards the end of the war?

REISIG: Ja. Well, of course we used the Wolman velocity measurement even in deployment.

NEUFELD: Even in the field.

REISIG: With a certain number of missile firings. But the larger number was equipped with this integrating gyro. But the Wolman system was still superior to this gyro integration.

NEUFELD: --in terms of the accuracy of the cutoff velocity. And so it was maintained pretty much.

REISIG: Yes.

NEUFELD: Pretty much up to the end. It gets me a long ways off chronologically from where I was, but I gather you also, at the end of the war you also used radio guidance for trajectory sometimes, right?

REISIG: In a few, a small portion, actually. You might have read about the guidance beam of the Leitstrahl.

NEUFELD: Right.

REISIG: That was for controlling the lateral motions, cross-axis or yaw motions.

NEUFELD: The gyro system wasn't entirely satisfactory with the lateral dispersion of missile?

REISIG: It can't do it. That's the nature of physics. You see, what happens, let's say a big force hits the missile. The missile reacts in two ways. It turns its axis, according to the wind impact. And the center of gravity of the missile is shoved to the side, that's the second effect. So, this gyro system, the platform, can only take care of the motion of the axis of the missile but not of this lateral deviation.

NEUFELD: Right, it's pushed aside.

REISIG: And for that purpose we're developing these accelerometers. These accelerometers were of course connected with the lateral motions--

NEUFELD: --and tried to compensate--

REISIG: --the acceleration again, you have integrated to velocity, and then integrated a second time to distance.

NEUFELD: So without being able to finish all of those accelerometers, it became very, it was essentially impossible to ever, during the war, this is not to put down in any way the marvelous accomplishments in control, given the fact of where you started at nothing in the 1930's, but you never really effectively solved that problem of lateral dispersion due to wind forces because of the accelerometers?

REISIG: It didn't make sense to put these three accelerometers in the missile without a stabilized platform, because they needed a fixed reference, which is supplied by the self-control of the platform.

NEUFELD: So all that, here's where--

REISIG: --even if the accelerometers would have been available, come to a certain point of usefulness, it still didn't make much sense to use it because we didn't have the stabilized platform. I think the platform for the A-4 from Kreiselgeraete came very late in '44 or so. As far as I remember, we had fifteen test firings or something like that back there with the mobile platform, but not in the missiles which were used--

NEUFELD: --right, very late. Very late test series missiles.

REISIG: Yes, there was some kind of, what should I say, industrial--failure of industrial management, yes. You see, Kreiselgeraete was so occupied with these navy projects that our thing was on the sideline. It was very inconsistent, because actually the missile had the highest priority for production. But that's one of the sad stories.

NEUFELD: Well, it seems from reading that there was a constant battle for priorities. Obviously one of the basic problems was that Germany had its limitations in the amount of technical personnel and raw materials and a lot of other things--

REISIG:--for actual factory facilities--not like this country.

NEUFELD: Essentially I think as much as anything it amounts to, Hitler threw you into a war against so many opponents that there was no way that Germany could master all of those problems simultaneously.

REISIG: Impossible. Impossible.

NEUFELD: But as far as this battle over priorities was concerned, in the earlier part, I know in the early part of the war, '39, '40, '41, '42, there was a constant change in the priorities of the A-4. It went up, it went down, depending upon politics. Did that affect your work?

REISIG: By all means.

NEUFELD: Because I think in your interview you said you had appropriate amounts of funds, you didn't feel like you had a money problem.

REISIG: No, it was not so much the money, but it was materials, manufacturing in industry, and personnel. It was just not there. Or it was not to our disposition.

NEUFELD: So that in this period, when A 4 wasn't always top priority, you felt that you weren't getting all the materials you needed or all the experienced personnel you needed.

REISIG: It even went so far at the universities, I remember particularly Vienna, we had a very good contact with the communications institute there. If we needed parts for the instrument we had to build--where to get them? Well, if you can't do the experiment, produce it, you are thrown out of the door. What do you want here? We have to manufacture for the air force.

NEUFELD: They're pressing them to produce some components. And they can't keep up. So that you feel, I think I want to come to a stop pretty soon because it's getting late, but you felt at that point, the development of the A-4, and we should probably at some point talk about other projects too, but that was slowed down, not just going into production, which of course was a problem in 1942, '43, but the development, the finishing of the

development of the missile was slowed down in those years definitely by those priority fights and so forth. Okay, so thank you, we can resume this again.

Date: June 6, 1989

TAPE 2, SIDE 1

NEUFELD: The first thing I wanted to ask you yesterday, picking up where we left off, is, we were trying to cover all the different things you were involved with in the period from '38 let's say to early '43 when you were still at Peenemünde. And we talked about the guidance. We talked about the Wolman system, the cutoff, and I guess we talked about the fact that you were not involved in test stand engine measurements at all in that period, so was there anything else, other major activities that you were involved in, in your group or your section?

REISIG: You mean, after I turned it over to Thiel?

NEUFELD: Yes, after you left the engine stuff. Were there other things that we've left out, other major projects that you were involved in?

REISIG: Yes. One very significant project was telemetry.

NEUFELD: Yes, that's one of the things I wanted to ask about.

REISIG: And that was another sad story. You see, von Braun approached me shortly after the Oie launching of the A-3, he approached me that we need by all means telemetry, and as a matter of fact, I remember, we were going on the boat back from the Oie to Peenemünde together--

NEUFELD: And this was after the A-3?

REISIG: Yes. And I thought I had a fairly good background for that. You see, telemetry is a multiple communication system. You have so many physical quantities you want to measure and transmit, so there will be a large number of channels in parallel you have to transmit. And I happened to work, before I went to Peenemünde, at Siemens, in multiplexing telegraphy, so I had a pretty good idea about what telemetry should look like. Of course we didn't have the personnel nor the implementation to develop it in our own laboratory. I mean, we could write up very detailed specifications, what we wanted and how it should work but it should be taken over by a competent industry, and the industry for that project was Siemens, and I approached Siemens --of course I knew quite a few fellows from my former activities at Siemens, also from technical university, my Alma Mater, I knew

these fellows.

NEUFELD: You're talking about Technische Hoch-schule Berlin or Dresden?

REISIG: Dresden. I was a student of Barkhausen. I don't know whether you are familiar with that name.

NEUFELD: Yes, you pretty extensively covered that in the first interview, so I know.

REISIG: Barkhausen was one of three authorities in telecommunication in Germany at that time. And Siemens showed me absolutely the corridor, as we say, we have no idea, we are full to the rim, we can't take such a project.

NEUFELD: About what year was that, '38?

REISIG: I think it was late, the second part of '38, I should say.

NEUFELD: Okay.

REISIG: About the time when we had a crash buildup of the air force and everything was occupied with the production of the air force, equipment.

NEUFELD: So they just weren't interested in accepting a contract.

REISIG: Absolutely not. And one of the reasons, and that's a typical thing, particularly during the developing phase, industry is not very cooperative, because they are not interested in small series. They want mass production.

NEUFELD: Right.

REISIG: And it doesn't make sense for our development period. We need 50, 75 units of a certain piece of equipment, that's it. But not 3000 or 5000 or 10,000, as they like.

NEUFELD: Yes, and they wanted--

REISIG: --to make the money--

NEUFELD: --they wanted the big contracts from the Luftwaffe buildup.

REISIG: Sure.

NEUFELD: And so forth, just before the war.

REISIG: So the emergency solution, I should say, was that we went to a small firm in Berlin, which we knew already, the Firma Dr. Hell. I don't know whether you heard about Hell.

NEUFELD: No.

REISIG: Rudolph Hell, he was kind of a genius. He first worked for the government in Berlin-Adlershof and then got his own company. He had a bit of money and so forth. And he made a name of himself with the so-called Hellschreiber. That was a teletype machine, but not with keys, but with a spiral roll which was activated like a modern telegraph. Very, very clever system, a forerunner of digital signal transmission. He made a lot of money with that. Of course the German army and air force ordered hundreds and thousands of these units. That was typical. He developed it and put the money into it, and then when Siemens saw what they could make out of it, they got a contract with Hell and said now we manufacture it. And of course they had to pay license fees to him, so Hell got his share. But that was typical. Industry was absolutely not interested in developing it.

NEUFELD: Was the machine used for transmitting coded messages?

REISIG: Yes, sure. It was very much used. Well, there's a lot of details. It was a very safe system. For instance, if you're in a radio transmission, it's fading, something is shifting in the frequency band, this type of distortion. I don't know what you'd like to call it, the "Schreiber" could pick it up. It was a very good system. You could retune it when it was writing. So it was a very safe thing for transmitting messages. And this company said, "Okay, we know your problem, we like you," and they started fixing something together for telemetering. They were not experts in multiplexing systems. It was just a kind of "natural resolution," so to speak, because Hell had some experience with multiple command transmission for unmanned "Panzer." At least I was glad to have more than nothing. If you look in the books, for instance, Ordway made very unfriendly remarks about this rudimentary telemetering system. There was not a "bakery" I could go in and say, "Well, here, put me a telemeter on the counter," there was nothing like that. Nobody really could provide such a mature system.

NEUFELD: So nobody had at that point a radio version of multiplexing systems?

REISIG: When I was with Siemens, in this multiplexing laboratory, we designed a telemetry system for power plant data

transmission. These were particularly for the low frequency range, i.e. for small sampling rates. We had such a project with the power plants in Morocco.

NEUFELD: Yes, so you mentioned in your--

REISIG: They wanted to transmit their measurements for the power plant to a central point, along the high tension lines as data channels. So I had this experience, but not with the radio. Radio transmission is much more difficult.

NEUFELD: Why is it much more difficult?

REISIG: Because of the band width limitations in radio communications.

NEUFELD: You mean, squeezing that number of channels into a narrow band.

REISIG: A relatively narrow band width, yes. Of course, when you use these high tension lines, we didn't care. We just used what we wanted to. But it's much more difficult with radio transmission. The system we developed for Morocco for this power plant network did not use a carrier frequency. But with the radio transmission, you necessarily have to use a carrier, and that introduces this bandwidth limitation.

NEUFELD: So at that point you issued a contract to Firma Dr. Hell. Right. In '38. And what were the problems that developed over time? When did you finally receive some kind of unit from them?

REISIG: I would have to dig up the records. As a matter of fact, let's see,

NEUFELD: Even just an approximate memory of that would be Okay.

REISIG: I think I told you yesterday about this on-board oscillograph, this Hell Oscillography for the A-5 firings. The firing on the 21st of April, 1940, had an oscillograph; 21st of April, oscillograph; 30th of April 1940, 13th of June, oscillograph, oscillograph.

NEUFELD: For the record, Dr. Reisig is looking at his record of A-5 launches. So you never had any telemetry as far as you know now on A-5?

REISIG: Yes, not at that time.

NEUFELD: So the first experiments--

REISIG: --it might be much later but I didn't cover the details. There were two firings, one in November '41 and then in January '42.

NEUFELD: Of A-5's.

REISIG: Right, it was kind of late. It could be that we had telemetry on those, but that's again documented in archive reports so we would have to dig those out.

NEUFELD: Yes, but just from your memory you remember that it took a long time.

REISIG: Now, of course, there was a firing of an A-4, was it '41?

NEUFELD: The first A-4 was, well, let's see, the V-1 vehicle was the one exploded in March, '42, and then the first launch attempt was June, '42, June 13th.

REISIG: Yes, I think you're--I had it right here.

NEUFELD: I made up my own chronology and I remember it was June 13th, 1942, that the first A-4 launch, unsuccessful, was carried out. But telemetry probably wasn't used right away, was it?

REISIG: The Test-Missile V-1 was the 18th of March '42, the "V-2" on the 13th of June, '42, the "V-3" the 16th of August, '42. And the only successful one was the "V-4" in October, '42. I suppose we had telemetry in.

NEUFELD: You tried the telemetry on those first A-4 launches in 1942. You weren't very happy with the results, I guess, of the telemetry system.

REISIG: Well, it has two aspects. Of course we realized right away, because of this band width limitation, that we couldn't do a superb job. The people who wanted the measurements particularly in the control system with a tenth of a percent, good grief! Such a limited band width and as many parameters as possible, that was one thing. And the other thing was that the people there were pretty much unreasonable with their requirements of measurements. For instance, I remember very well that the power plant people wanted to have a continuous record, that is, a true analog record. We could multiplex, even with the small band width we had, by commuting. We had a turning switch which sampled so many measuring quantities in the cycle. But there were very smart people who said, "I don't care for these sampled things, I want a continuous record," and particularly the

power plant people. Now, that's absolutely foolish. They didn't understand their own system. They wanted to measure the combustion pressure in the engine, and that is a very slowly changing quantity, so there's absolutely no reason to continually measure it. I told them about this feature, but I didn't succeed. They had enough support from their supervisor, the director of engineering--not a measuring specialist. I couldn't make it. And that of course excluded other costumers who would have liked to obtain at least commutated measurements.

NEUFELD: Right. So Thiel backed them up in demanding?

REISIG: I think the responsibility for designing the combustion unit was already turned over from Thiel to Riedel III, who was the chief designer. And he was a hard guy.

NEUFELD: This is the second Walter Riedel, not Papa Riedel.

REISIG: Not Papa.

NEUFELD: Okay, so it was--and so he demanded that they receive this as a continuous measurement which took up a considerable fraction of--

REISIG: --of the channel, yes, with five other measurement parameters on this channel, to make other people happy.

NEUFELD: So as far as the quality of the telemetry was concerned, it was basically--you received adequate data on the ground, but you only had a very limited number of channels.

REISIG: Right. Right.

NEUFELD: That you could choose from. It wasn't a matter of the--

REISIG: With the developed system in this country on the Saturn V, I think we had at least 850 flight measurements which is probably nonsense on the other side, yes, who's going to (crosstalk)

NEUFELD: --NASA--

REISIG: --correlate them?

NEUFELD: --a massive outpouring of data which can only be evaluated by--

REISIG: --madness.

NEUFELD: Yes, only if the vehicle fails is it any use. There's

no way to use it in real time or anything else. So you only had maybe eight channels or something like that that you could pick, eight or so measurements, or, I mean, the exact number isn't important but it's only a handful.

REISIG: Yes.

NEUFELD: But your problem with the telemetry wasn't that the transmitters failed or anything like that?

REISIG: Not so much the reliability. As a matter of fact, the telemetry, even as primitive as it was, was more reliable than the oscillograph. We had quite some trouble with the film cameras, jamming and what not, and it wasn't exactly focussed on the screen of the cathode ray tube and things like that.

NEUFELD: So it wasn't a problem of the quality of the cathode ray tube, it was the filming of the oscillograph that was inadequate at the time. So as far as you can recall, you pretty much had telemetry on A-4 almost from the beginning.

REISIG: As far as I can remember, yes.

NEUFELD: In a limited way, and it wasn't possible to push it much further during the war, in terms of the development?

REISIG: Let me try to explain: we didn't have any support from the industry.

NEUFELD: Right.

REISIG: So we went to some universities, one in Darmstadt which wasn't very effective, and then to Vienna, and these people were extremely good.

NEUFELD: Was that a technical university?

REISIG: A technical university in Vienna, yes.

NEUFELD: Okay, and that was how, some time into the early forties at this point that you turned to Vienna?

REISIG: Well, if you're interested in details, I have to take out my file on telemetry.

NEUFELD: Yes. Well, we can hold the tape for a second. ...Okay, so we were looking at your note from this period and University of Vienna, Technical University of Vienna was involved in telemetry development in October, '42.

REISIG: I think we started in the middle of '42 with them.

NEUFELD: Did they develop a separate telemetry transmitter?

REISIG: I see from the notes here, the first thing they looked at was the receiver.

NEUFELD: Right.

REISIG: And I remember then that they did some work on the transmitter, improving for instance the power of the transmitter, and the linearity of the transmitter, and finally, they went into the multiplexing system as such, and that was '43, '44.

NEUFELD: Was an improved system based on Viennese studies incorporated into later developments?

REISIG: We improved it continuously, yes. So it was a general thing with our development work, that few missiles had the same types of instrumentation or even parts, internal circuits, so to speak, so hardly any test missile was like the previous one.

NEUFELD: Well, I'd better stop this now.

TAPE 2, SIDE 2

NEUFELD: So you have notes from conferences in Vienna on telemetry as early as February.

REISIG: As early as February of '42, yes.

NEUFELD: Okay. So it's about that time that they were brought in, you think, or maybe even that they were brought into trying to greatly improve the telemetry capability.

REISIG: Right.

NEUFELD: Later in the war. And do you think that the Technical University of Berlin was brought in at some point later on?

REISIG: Yes. As a matter of fact, they worked together with Vienna. We arranged for that, that Berlin would support Vienna, but Vienna went ahead as it was more effective than the Berlin people.

NEUFELD: Right, that's two groups working very far apart.

REISIG: Yes.

NEUFELD: There would have been coordination problems, I'm sure,

among other things.

REISIG: The fortunate thing with the Vienna people, they were very good in theoretical terms, but they could do something with it. They could build something.

NEUFELD: They could actually build an effective transmitter.

REISIG: The Berlin people were much more on the theoretical side. They had lots of ideas, but they had a kind of a hard time to materialize them.

NEUFELD: So then in conclusion, as against what, the rocket teams, you were at least somewhat satisfied with the telemetry development that you had on A-4.

REISIG: Sure, for the essential things, which were the power plant measurements and then the control system measurements. That was at least something.

NEUFELD: You had some kind of--given the limits of the technology, you did reasonably well in the long run in getting telemetry. I had another question that connected to this, and that is the issue of range safety. I noted from, I think it was reading Dornberger's book, the problem of accidents and you know, missiles falling back in the territory of the Peenemünde-Ost or sometimes even Peenemünde-West, and I wondered why you didn't have any destruct system, range safety kind of system that existed, that would be considered normal now, or was that considered?

REISIG: Well, I think, what should I say? Looking back, I think our military people who had the responsibility were kind of careless.

NEUFELD: Right. They didn't really consider the effects.

REISIG: Yes.

NEUFELD: The possible dangers.

REISIG: Yes. You see, you might have seen pictures, that we had a very nice residential area in Peenemünde where we lived.

NEUFELD: Right.

REISIG: And one of the missiles went back toward the settlement, and just was 300 meters from the boundary of the settlement. The thing went into the forest and exploded in the forest, by sheer luck.

NEUFELD: It could have been a terrible disaster. Did that give people quite a scare at that time?

REISIG: Of course.

NEUFELD: But had the concept of range safety, destruct system, even been considered?

REISIG: Well, the funny thing was, we didn't protect our own people carefully enough, but on the firing range, out by the coast, we had the navy and what not engaged in blocking off certain areas where they thought something could happen. For instance, beneath they would cut off locations, and then of course the impact area was sealed off.

NEUFELD: Right.

REISIG: By some kind of bulletin, navy bulletin, but not our area.

NEUFELD: It just didn't seem that likely that it would ever fall back?

REISIG: It was a kind of, what would you call it--

NEUFELD: The danger was taken for granted?

REISIG: Ja.

NEUFELD: As a fact of life, of business, at that point. I mean, it's not like you didn't take any measures. Obviously test stand seven, the main launch area, had a quite a big wall built around it.

REISIG: You see, ja, now, of course we had major commands there, but when should you shut it off? When would be the safest instance?

NEUFELD: There's no way of really knowing.

REISIG: It's not easy to decide.

NEUFELD: Especially if it's coming back toward the west.

REISIG: Right, you may make it worse, rather than let it go, and hopefully some farther out, ja, and then shut it off.

NEUFELD: Right. Okay. So as far as that is concerned, it really wasn't something that anyone considered seriously at that

time, Okay. So telemetry, connecting to, I think finally finishing up what we were talking about, all the different things you were involved in, have we left anything out on the major projects we've talked about so far in your involvement?

REISIG: We talked about accelerometers yesterday.

NEUFELD: Right. And we talked about the Wolman system, gyros, telemetry.

REISIG: Of course, there were certain gadgets which were in the measurement domain. For instance, the torque measurements for the control rudders which was still in my area, because it was carried by the missile. It was a pretty important thing, these torque measurements.

NEUFELD: In order to understand the workings of the control system.

REISIG: Yes, and to make sure that we had a large enough control range. In other words, was that we could supply strong enough control forces with the rudders.

NEUFELD: Right. You're not talking about the air rudders, you're talking about in the gas stream.

REISIG: Yes, both air and jet.

NEUFELD: Okay. I guess to finish up on that whole period, at some point Steinhoff came, right. Was he there only, I was under the impression he was there only from 1940 or so, am I incorrect? Was he there earlier?

REISIG: No, he came in late summer of '38. As a matter of fact, Steuding brought him to Peenemünde, had worked with him in this glider plane research institute, and it was an unfortunate thing that he brought him up. Steinhoff was a very controversial figure. I think in the end he did more damage to the whole project than he promoted.

NEUFELD: I think that's very important for historians to know, because I haven't seen that anyway. Why was he controversial? What was it that he did that created problems?

REISIG: Well, you have to realize that he was responsible for a huge area, all the electronics gimmicks.

NEUFELD: He came in as the head of all electronics.

REISIG: Yes. And he was incompetent. He had not a bit of

industrial experience, at least at the scene of the experience myself, and not to compare with Wolman or people like that. Wolman was here and Steinhoff was here.

NEUFELD: Steinhoff was so--

REISIG: --but he didn't want to admit it--

NEUFELD: --wasn't very good at all.

REISIG: He had always a big mouth. And he was actually kind of a bad Nazi. Forcing people into things which were unreasonable, like most of the Nazi things were unreasonable. The program he forced--you see, I was actually the one who built up the electronics department there.

NEUFELD: Right, it certainly sounds that way.

REISIG: Of course, I didn't have any experience in control at that time, I was in test and measurements. Steinhoff allegedly was a control man, but he wasn't. The control man was Steuding. He really looked through things.

NEUFELD: So Steuding was really responsible for control theory, as you say,

REISIG: Yes.

NEUFELD: For the development of that whole side. What was Steinhoff's training? He was an engineer, I assume?

REISIG: He was a--

NEUFELD: --was he a Diplom-Ingenieur?

REISIG: Yes, from Darmstadt. I think he basically was a meteorologist. And he got into flying. The thing he really could accomplish was flying. He was one of the most outstanding pilots I ever encountered.

NEUFELD: Right.

REISIG: And he was particularly, in the beginning at least he was a glider pilot, and he made some records.

NEUFELD: Right, so you described in your first interview, that he was (crosstalk)--had a long history of--

REISIG: Ja, he went from Wasserkuppe, which is near Frankfurt, not too far from Frankfurt, into Czechoslovakia, and that was a

deed.

NEUFELD: At that time in an unpowered aircraft, right, in a glider. So he was then a real party man? Sort of a bad Nazi?

REISIG: He wasn't actually active in party matters, but he had the--

NEUFELD: --enthusiastic --

REISIG: Yes and the, what do you call it, the attitude of the typical Nazi, "I tell you, you do that, you'd better do it."

NEUFELD: Authoritarian.

REISIG: Yes, absolutely, and many many instances, absolutely unreasonable, because he wasn't competent enough. Now, he got his doctorate then from Darmstadt, but I don't know how he got it. He made a thesis about glider instrumentation. But I don't know, I can't prove anything, but I--

NEUFELD: You have your suspicions. Can you specify any particular things over which there were big arguments, like telemetry or anything, where he might have done damage?

REISIG: He was so erratic in anything he did. He could give an order, right now--two hours later he'd give the contrary order, and things like that. Which is unbearable in such a complicated thing like a missile system. Well, as a matter of fact, I had to pay for my opposition to Steinhoff, because I am, "My dear friend, I can't do the things you order me to do because it's absolutely unreasonable." Of course he didn't like to hear that. And then some day there was a showdown, with von Braun being present. This was a shortcoming of von Braun, he was too loyal to his people. He should have thrown Steinhoff out, because he did so much damage to the organization. And one day, it was actually, turned out to be a discussion about telemetry, and Steinhoff complained that I didn't make enough progress and didn't improve it fast enough, and I said, "My dear friend, you know I have these personnel problems. I have hardly bodies to consistently work on that." Then I was fortunate, I got actually an officer, a signal corps officer who actually was an engineer in telecommunications, a very nice fellow, very agreeable in everything when you worked with him, and I just noticed that he didn't work on the assignments any more, and we discussed it in the presence of von Braun, and von Braun said, "You are naive, don't you know that Steinhoff gave this man and his co-workers to the commander to operate a radar communications link?" I almost fell from my chair. He didn't tell me. And then I was naive enough to ask von Braun, couldn't we go to Oberst Zanssen and ask

him if he would be kind enough to return these people? He was reasonable enough to realize, telemetry was more important for the whole development and even what purpose it was for. And then von Braun said, "You are naive. Steinhoff offered this man two times as much. He didn't demand it. Steinhoff said: I have it, I can give it to you." That was typical of Steinhoff.

NEUFELD: About what time was that, can you pin it down?

REISIG: It was in early '43. And of course, Steinhoff got cooking mad at me that I lifted all this, public, and the result was that, in the factory at noon, I got a telephone call from the administrative man who said, "You go to the military officer in ? and pick up your order to join the army."

NEUFELD: Right. So you were sent, I remember the story from the first interview, you were sent into basic training and then drafted back in the VKN, Versuchskommando Nord.

REISIG: But the thing was that I wasn't trained in Germany. They sent the whole division out to Russia.

NEUFELD: Crimea.

REISIG: To the Crimea. And Dornberger was reasonable enough to say it was impossible. And Zanssen was upset about the whole thing. And they called me back, to the program.

NEUFELD: So in fact they only called you back after they found that you'd already been transferred away? In other words, you were drafted, they got you back later?

REISIG: (crosstalk) ... I don't know whether we should put that on the tape.

NEUFELD: Let's start this again. Now, before we go on with the discussion of what happened to you after you were drafted and came back, I guess I just wanted to make sure that I'd covered all of the other issues that were left. Certainly one of the questions I have, and I don't know how much you want to talk about it, is you certainly described Steinhoff as a controversial personality. Was there anybody else who stood out as being a problem at that time? You know, for some members of the team.

REISIG: Is the name Klaus Riedel something to you?

NEUFELD: Yes, I know who that is, yes.

REISIG: He was a genius in his own right, and he was actually the first director of the testing team, testing operation, and

von Braun wasn't satisfied with him, although he was a friend of his from very early days.

NEUFELD: He dated back to Raketenflugplatz.

REISIG: Yes. Which I can't understand. I don't actually know what was wrong with Riedel in that respect. He had a very good technical fantasy, but maybe it was too much and it wasn't systematic enough for the test stand. For instance, a typical engineer, he didn't want to write, but if you run a test, you'd better write about it, and that he left entirely, and von Braun got mad at that, and so he substituted him, but he switched him. He had this problem with preparing the A-4 system into a military operation.

NEUFELD: Right.

REISIG: I don't know, did I mention that von Braun went to Dornberger and asked him, "How shall we do that? We have no idea. You said there would be six general staff officers doing it. They never came. And so we had to do it ourselves." And von Braun remembered the good technical fantasy of Riedel, and that was a total hit. It was amazing, how Riedel solved this problem, with its mobilized employment. It was fantastic.

NEUFELD: So that after Klaus Riedel was pulled off the test stand 7, I guess that's where he was--

REISIG: All the test stands.

NEUFELD: All the test stands, he did do very good work with the road mobility, mobile launch system.

REISIG: Not only putting the things on trucks also, but the whole arrangement of the firing procedure. It was all his idea, what the general staff officers were supposed to do. He did it with his technical fantasy. It was absolutely amazing.

NEUFELD: That would be considerably different, obviously, than launching it from a well instrumented test stand.

REISIG: Of course.

NEUFELD: So that meant developing a whole set o --

REISIG: You see, I think, essentially, as I recall it, there were 13 different vehicles which had to participate in the firing. Well, at a certain moment, one vehicle might be in the way of another one. And so on. And he solved all these problems.

NEUFELD: This was largely in 1943, that he solved them.

REISIG: Ja, when I came back from Russia--

NEUFELD: He'd already started.

REISIG: And he, don't you know how he did it? I wonder if I gave it in the first interview? He did it this way. He got little toys made to his specifications, little cars, little trucks, with whatever they had to carry, a tank or a missile or whatever, and he played around with these things, ja, and that's the way he found his system for the arrangement for the firing. It was absolutely, I was amazed.

NEUFELD: So the problems with him before, when he was heading the test stand, was basically an issue of, he wasn't a good manager in some ways of that whole thing in terms of writing things down. It wasn't a matter of his being an abrasive personality.

REISIG: No, he was a very likeable guy. Had a very good sense of humor. Just typical little things. We lived pretty close in the Siedlung.

NEUFELD: The settlement.

REISIG: In the settlement there. And he liked jazz pretty much. He had a very very fine music box at that time already, very expensive thing from Siemens, and he liked jazz so much, and sometimes he was a little bit loud with his music, and we talked about it. He said, "Well, you must understand that I am more or less born with liking jazz. You know why? My father was a navy officer, and he spent so much time in the tropics, and I think I inherited it from him, that I like jazz, from the colored people." That was his typical kind of humor. And another thing which is typical of Klaus Riedel, at lunch time we met in the cafeteria, and there was a certain round table always together there, and we all liked coffee, but at that time coffee was a rarity, and we would get mad at that, "Goebbels.... says Kanonen statt Butter" and at that time --

NEUFELD: "Guns instead of butter"--

REISIG: Ja, and at that time the import of coffee was more or less shut off by the Nazi government, and the reason was, not that we needed the material for armament, but we didn't have enough foreign currency to pay for that, and Riedel said, "They are shortsighted, those people, the coffee makers, they dump their coffee into the sea to raise the prices. Such an idiocy,"

he said. "Now, couldn't we pay the Brazilians for their coffee with industry goods, for instance with trucks," trucks were a big thing at that time, so "we send them the trucks over and they send the coffee, exchange kind of business. Now, if the Brazilians don't like our trucks, they can dump them in the sea, instead of the coffee, I wouldn't care." That was typical Klaus Riedel.

NEUFELD: Not that they were too likely to send trucks overseas when they were starting the war. So he was a very amusing person.

REISIG: And very likeable, very social oriented.

NEUFELD: Okay, I had a question regarding that deployment issue. At least the sources say that earlier on, von Braun and some others really would rather prefer a big bunker.

REISIG: Yes.

NEUFELD: And that in effect they were forced to take this road mobile system by Dornberger later on.

REISIG: Yes. You know the story of these bunkers?

NEUFELD: Somewhat. I've read some of the material on that, and of course also a factor was that Hitler liked huge bunkers too.

REISIG: Of course. He liked huge concrete buildings. He had pretty good success with the bunkers for the submarines in Brest and St. Nazaire and what not, and so he got the idea, oh well, we could protect our submarines, why couldn't we protect the missiles? And he ordered them, the building of these big, really huge structures, and of course, the British intelligence found this out, what was going on, and they started bombing these construction places pretty badly, put a big effort in that, and as a matter of fact, these buildings were never finished. I experienced such an air raid myself. I went with the chief communications officer on Dornberger's staff. I went to France in May, '44, and we went to one of these construction places, and the sirens went off and it was in the afternoon, 3 o'clock or so, the British came in, and we had a good bunker, air raid bunker, so we were not endangered, but I experienced such a bombing of such a construction site, and in the air all these wooden structures--

NEUFELD: --scaffolding--

REISIG: Yes, anything, yes. It was terrible.

NEUFELD: Very frightening.

REISIG: Ja, and so Dornberger was from the very beginning, he had a better feeling for that. He was an artillery officer from the First World War. He had a much better feeling for that, and propagated this mobilized deployment right away, and of course after these things happened, these construction sites, it was clear that the end was the mobile.

NEUFELD: Only the mobile was going to work. I guess for a while they were actually in parallel, right. They started working on mobile in '43 already, and building the bunkers at the same time.

REISIG: No, well, then they said, Okay, we can't finish these buildings but we will pretend that we continue constructing them, because we attract the British and American bomber force to these construction sites, instead they are going to Berlin and Hamburg and Köln and destroying our cities. So it was a kind of camouflage for a certain while, to continue. But there was not the idea that they would be used.

NEUFELD: Is it true that earlier on, before this whole argument was finally settled, that in fact it was von Braun and other members of the team who really did think that you needed a bunker, sort of like in some ways a military version of a test stand, to deal with such a complicated missile.

REISIG: No, I don't think this was so much the idea, rather than to have good protection for the preparations, for the preparations. What he, of course, did not know, what Dornberger knew from his war experience, that camouflage is a big factor in your deployment, and so we could do all these things which von Braun wanted to do in a bunker with our fleet of trucks and they could go in any place. As a matter of fact, they usually would look for a small forest, smallest size, and go into, well, you couldn't call it a road, just a path rather than a road, go in there, and for camouflaging, they took the tree tops and bound them together, to camouflage sight from the top. Never anything happened. Not a single time.

NEUFELD: It was just almost impossible for the Allied aircraft to find those batteries.

REISIG: Impossible. And it was so flexible in building up and packing in again, and could be done in such a short time, that even if the resistance fighter would notice they are there, before the message was in London or what not, in the bomber command, they disappeared in another direction, nobody could tell.

NEUFELD: And they made a point of moving fairly often, in order that it would be hard to find them.

REISIG: Sometimes even attack was necessary, and these are things I don't understand. Of course we had intelligence people all over the place, and we had two groups, we had the southern group, deployment group, and the northern, and the northern deployment group went into Holland, and they fired from Den Haag.

NEUFELD: The Hague.

REISIG: Yes. And what they did, there was a hall from a Dutch movie company, and they cut a hole in the roof, and fired from this hall. They were never molested. One explanation would be, of course, they must have found out that we were there, through the Dutch resistance and what not, but really to do enough damage to our firing units, they would have destroyed too much of the town and killed people and so forth.

NEUFELD: --civilian casualties.

REISIG: Right.

NEUFELD: That was certainly a consideration in bomber command targets, you don't bomb friendly civilians.

REISIG: But in the end they got so mad, from British intelligence reports, they got so mad that they said, we don't care any more, we have to kill the Dutch people just to get rid of these darned missiles.

NEUFELD: Right, it was frustrating for them. Obviously we have to get into discussion of your whole period with the Dornberger staff.

REISIG: Right.

NEUFELD: From 1943 on. I had a question that, about Dornberger, though, from an earlier period, that I didn't get to ask before, which sort of got lost, and then I want to move into this other. You used the phrase, Dornberger's phrase, "Alles unter einem Dach," everything under one roof, to bring all the development laboratories and everything together into Peenemünde, which sort of implies in effect that the government owned laboratories are much larger and stronger compared to the contracting industry companies. Do you know about when he said that phrase? When he came up with that roughly or do you have any recollection of the origin of that?

REISIG: I didn't see any document on that, but I suppose it was

a decision made at the time when they realized Kummersdorf was much too small, insufficient facilities. At that time they designed the plan for Peenemünde, and the concept was, everything under one roof.

NEUFELD: A big development facility.

REISIG: Right. You see, Dornberger in a certain way had experience with industry, from his solid propellant rockets, the artillery rockets and so forth, and he knew what he could expect from industry and what not. And he knew that the industry, contrary to what they said afterwards, the industry was not at all interested in rocket development.

NEUFELD: And didn't know anything about it.

REISIG: Ja, and they thought they couldn't make enough money with it. Much too costly for them.

NEUFELD: So at that point he didn't feel that there was much that could be done. Were there secrecy considerations too?

REISIG: That too. Of course.

NEUFELD: In concentrating the rocket development in one place. So that concept seems involved then with the plan for Peenemünde, as far as you recall it. The interesting thing about that, and again, it sort of jumps out of chronological order, is the way it compares to the American way of doing things, especially what became the Air Force and NASA way of doing things, that is primarily you contract out, and the contractors build the equipment, they do a significant part of your development, you don't have a big in-house capability.

REISIG: Yes, but the real story is, when we came over to this country, we more or less continued Peenemünde.

NEUFELD: Right.

REISIG: The same concept. One of the reasons again was that the industry didn't have any capability. They had to learn from scratch. And the Army didn't want to lose time. They felt, what was going on with Russia and the cold war and what not, they'd better get busy, and so they supported our concept. You have to admire these officers who had enough understanding for that, and Toftoy and even Hamill, our commander and so on.

NEUFELD: Was that also due to the fact that in the US Army there was an arsenal tradition, an arsenal system of US Army owned arsenal facilities, do you think that had anything to do with it?

REISIG: It might, yes.

NEUFELD: I mean, I think this has to be explored by historians. At least many historians now are suggesting that there was a real divergence between the Army and the Air Force way of doing things; when the US Army Air Force split away, it always had a close relationship with aircraft contractors, whereas the Army had an old arsenal tradition.

REISIG: Yes. And they had a very strong armaments ordnance corps.

NEUFELD: Right.

REISIG: With technically trained officers. They were pretty good, by the way.

NEUFELD: So that you were fairly, although obviously the American rocket development was greatly behind where you were when you came, you were impressed with the US Army Ordnance people.

REISIG: Yes. At least the understanding. They didn't say they know everything better, because they didn't know much about rocketry, and they were very willing to learn. As a matter of fact, we had pretty good personal relations with the officers, contrary to NASA. It was not easy to deal with the NASA people.

NEUFELD: The NASA management.

REISIG: They were arrogant and "we know everything better and you shut up"--and of course, we were a team, we were strong enough, particularly with such a leader as von Braun. So let's prove who's right and who's wrong.

NEUFELD: Right. Now, I want to come back to the American period, but I'm still interested in this comparison. In Germany you evolved what amounted to a strongly government-owned facility where you had a very large development capability in-house and even a manufacturing ability on a small scale in-house. Do you think that was mainly because of the inadequacy of industry at that time, German industry, to deal with this, or lack of interest?

REISIG: There are two things. First of all, there was no capability for such kind of a development in industry. On the other hand, at the same time, we had to learn, we could do it only in an empirical way, before we could tell the industry, do it this way and that way. We had to find out ourselves. I think

it was the most efficient way to do it.

NEUFELD: So it was far too slow to send out contracts.

REISIG: Of course contracts are something for manufacturing, but not for development.

NEUFELD: Right. Okay. So do you think it started with the sense that the industrial corporations didn't have the capability, and over time you really also became convinced--this is seemingly what's been indicated in other sources, that you became convinced over time that it was also the most efficient way of doing things? That you liked working with strong development laboratories and even a production capability in the team, in the group. You concluded that that was the most efficient way?

REISIG: To give you an example, particularly with control and guidance--as I told you, it was Kreiselgeraete who did control systems for the navy, but it was Siemens who did control systems for the air force. Of course we said, "Well, let's use that capability." For instance, Siemens was pretty strong in manufacturing of gyros and of the servo motors for the rocket action, but they didn't have anything of control theory. It was absolutely lacking there. So we had to build it up ourselves, and we were very lucky that we had people like Steuding and his co-workers, like Geissler a very talented man in guidance, and Hässermann and Dr. Ludwig. So the industry in those terms could not hardly offer anything significantly. I remember many discussions with Dr. Gievers of Kresielgeraete. Of course, since I was in measurements I participated even then in control engines and so forth, and Gievers was pretty frequently in Peenemünde. He had a hard time to follow Steuding.

NEUFELD: Steuding was too advanced for him.

REISIG: Dr. Gievers was a nice fellow and had definitely a lot of experience from his navy projects, but as far as the theory was concerned, he couldn't measure up to Steuding and his team.

NEUFELD: Steuding was in another category altogether in his knowledge, so that taking this example, in regard to the development of in-house or government laboratory capability versus corporations, if you hadn't done it yourself, the control and guidance thing, you wouldn't have had it done at all properly.

REISIG: Probably not.

NEUFELD: Or it would have taken too long.

REISIG: The time delay. Unbearable delay. And we carried over this concept even into NASA, when we were transferred to NASA, in spite of their objections. Of course, we had a lot of discussions among ourselves also, the team members, about the Challenger tragedy. And we said, "Now you see it. Threw our concept overboard, and had this contracting thing--" And they had to bear the consequences.

NEUFELD: Right.

REISIG: I mean, it might sound arrogant, but it's not only my opinion, but the general opinion in our team, if we had been in charge, Challenger would never have happened.

NEUFELD: Well, yes, this gets us a long way from where we were, but I'm just curious. Would you even have gone with the solid rocket motors and so forth? No? That's what I thought.

REISIG: We told NASA Headquarters at the very beginning of the Shuttle program, "Stay away from solid boosters." As soon as human life is concerned, then, we want to go safe, and the liquid propulsion is much safer than the solid--

NEUFELD: --solid rocket boosters--

REISIG: And you've got the proof of it.

NEUFELD: Yes. And unfortunately now we're committed to using those things for a long time to come.

REISIG: Yes, because, particularly since the industry lobby is so strong, that it would take a very strong effort to go back to liquids.

NEUFELD: Yes, I know there's this whole thing about the advanced solid rocket motor and that was heavily criticized recently, and some people said they should have developed a liquid booster instead.

REISIG: And why didn't we start in Germany with the V-2 as a solid propellant rocket? There's two reasons, very important reasons. The first thing is, the efficiency of power per weight is about 30 percent less than the same weight of liquid propellants. So you punish yourself weight-wise. And the second thing is, with these huge sets of solid propellant units, they are pressed in the container like these rings.

NEUFELD: That's because at that stage the only solid propellant you had was black powder.

REISIG: No, we had other things too. But these machines, the machine tools are not available to manufacture such large units of solid propellant.

NEUFELD: Okay. But wouldn't you have been in the long run, I mean for a military missile capability, the United States has become almost totally solid rocket motors because that's the thing that makes sense militarily. Yes, firing it on a moment's notice and not worrying about fueling

REISIG: The matter of response, response, ja. But I think you have to distinguish between the tactical employment and the strategic employment. For the tactical, this quick response might be essential. Not for strategy. I mean, if you fire a missile at 11 o'clock or 11:15 or 11:20, it doesn't make a bit of difference in strategic terms.

NEUFELD: Yes, but if you're in a current nuclear arms race where you have to worry about almost instantaneous launch, then there is, then the solid is better. That's another story altogether, what you were involved in--

REISIG: I don't believe in this deterrent thing. It's just simply foolishness.

NEUFELD: Yes, well, I guess that could get us a long way from where we were. We were talking about--

REISIG: But of course, I mean, in the long run we have to make such considerations, for instance, the difference between solids and liquids, and it goes on and on, the Shuttle program and the follow-on projects.

NEUFELD: Okay. My tape is almost over.

TAPE 3, SIDE 1

NEUFELD: When the transcript is done you will definitely get one. Okay. ...So we were just discussing the issue of how you managed to accomplish what you accomplished at Peenemünde technologically, in an environment where there was seemingly so much infighting and chaos in the Third Reich, and you were going to explain your reasons.

REISIG: At the moment three reasons come to my mind. The first thing is, as I mentioned, that Hitler was not interested at all in the rockets, until we could show him our success with the first long distance firing. Then he caught on. But it was in '43, and it was about six, seven years after our start on the

whole project actually. That's point 1. Point 2 is Dornberger personally, that he protected us so well. It was very hard for outsiders to interfere. And the third thing is geographic; we were so remote that the most even high level people didn't know that we existed.

NEUFELD: Did Dornberger's good contacts with higher officers and things play a big role as well?

REISIG: He had very good support from von Brauchitsch. See, Dornberger belonged to the artillery command under von Brauchitsch.

NEUFELD: Was that in the First World War, do you know?

REISIG: Oh no, that was in the Reichswehr.

NEUFELD: In the Reichswehr in the 1920's.

REISIG: Ja, essentially, as we call it, von Brauchitsch was Dornberger's "Regimentsvater."

NEUFELD: His regimental father.

REISIG: His personal contact. And of course von Brauchitsch was in his high position after the Fritsch affair in February, '38; he was on the top level.

NEUFELD: Right, he was commander-in-chief of the army.

REISIG: And pretty much independent, and that was a big advantage for us.

NEUFELD: From '38 to late '41 he was commander-in-chief of the army, and then even, I think even after that, when Hitler threw him out after the failure in front of Moscow, December, '41, he was still commander of the home army, was it not, or do you remember?

REISIG: On no.

NEUFELD: Von Brauchitsch, wasn't he Befehlshaber der Ersatzarmee?

REISIG: No, he was just retired then.

NEUFELD: I wondered.

REISIG: But it was Fromm who was the highest commander in Ersat-Army, and Chief of Army Armament.

NEUFELD: Did Fromm come in immediately at that time, or was it earlier?

REISIG: Oh yes.

NEUFELD: I seem to have been under another impression. I'll have to check that, check the facts. Were there any other important allies, in your view, in power, that helped you get your priorities?

REISIG: Particularly military, there was Professor General Becker. Karl Becker. He was another kind of father to Dornberger in the armaments experience.

NEUFELD: Because he had been Dornberger's superior in the Heereswaffenamt, in the German army ordnance.

REISIG: Right. It was Becker. And then when Becker killed himself--you see, he had such arguments with Hitler that he couldn't stand it any more--which we very much regretted. He was a fine gentleman, Becker. And then came Leeb, General Leeb, and he was very favorable to us. I remember very well, when he came for the first time to Peenemünde and introduced himself, and I was invited to sit in on this conference. He was always in favor of us, and then of course there were other officers, high level officers who were so impressed with what we did that they were favorable, just admiring what could be done. That was in the military sector. And then in the more or less civil sector was Speer. You see, Speer was chief of construction in Peenemünde. He more or less built the whole facility. He was an architect, you know.

NEUFELD: Right.

REISIG: And he made friends with us, and he liked that we were all young fellows. He liked that. He wanted to be a part of it.

NEUFELD: Right. He was about the same age approximately.

REISIG: A little older, I think, but not very much. So he was very much in favor, and at least until he became minister of armament, then he had a much more difficult position. He had to fight industry. He had to fight the military. He had to fight Hitler.

NEUFELD: He had to fight the SS. And that was--

REISIG: Do you know this book by Herr Speer?

NEUFELD: I've looked at it, his memoirs.

REISIG: No, not the memoirs.

NEUFELD: Not the Inside the Third Reich?

REISIG: Der Sklavenstaat.

NEUFELD: I haven't read it yet, I know about it.

REISIG: I recommend to read it. It's very illuminating. And he has two chapters on Peenemünde in this book.

NEUFELD: In his SS book. I guess I'll definitely have to take the time out to read that. I've just read the references to it in the Hoelsken book, Die V-Waffen.

REISIG: Oh yes.

NEUFELD: Which is a book you don't like.

REISIG: He gives such a weird interpretation of many things, you might be pretty well misled.

NEUFELD: What specifically do you dislike? I think you mentioned one thing yesterday, when you said that he misjudged --now I can't remember what it was, it was the development of the technology or something and he didn't understand it.

REISIG: You see, I have never met him personally. I just wrote one letter to him and he sent me one letter and then I thought it was useless to be in contact with the man. It's hard for me to appreciate him, what kind of fellow he actually is. He is an historian and sociologist--it might be that he's pretty much on the left wing. That's my suspicion. And he makes so many negative statements about our work and about our personalities. For instance, what business does he have to criticize Dornberger in a negative way? He never met him. He never understood him. And he's so arrogant, to write about such things, as "oh, I know everything." The funniest story, just to illustrate it. Last year, we had a visit of a group of Germans from industry and armament and former air force people, and among them was a Professor Detmering. He's a professor in Aachen Technical University, and he was in the Krupp corporation, a high ranking official, so he's a man of standing. And he by some accident met Hölsken. Detmering was a young fellow in Peenemünde in the air force part of it. He worked with the V-1 experiments. And he met, by some silly accident, Hölsken and he came in discussion about his book. Detmering is a very nice fellow. He said to

Hölsken: "Well, what you wrote, is not actually what we experienced. There are many things quite a bit different." And Hölsken answered, "But I have the documents."

NEUFELD: So as far as he was concerned, he wasn't very interested in what Detmering had to say.

REISIG: Yes. I appreciate very much your approach to your project. But Hölsken collected sources and wrote a book about it. He never contacted any one of our team, neither at Peenemünde-Ost or Peenemünde-West, before he finished his book.

NEUFELD: Right.

REISIG: He blames us for that. We were not cooperative. How did we know that he wrote such a pamphlet--it was actually his dissertation.

NEUFELD: Yes, it was a dissertation. I find the book useful just because it puts together a lot of information that isn't available. But the interpretation--

REISIG: Of course, he has some merit for his survey of the resources.

NEUFELD: Right. Basically it's Freiburg resources he used. As it's getting late, I guess this will almost bring this to an end. Next time we can talk about the 1943-45 period, the Dornberger stuff. One thing that came up, you mentioned the Luftwaffe, that I'd always been meaning to ask. I certainly get an impression from that book, from Hölsken's book, and also to some extent other places, that there was a deterioration of the relation between the army and the air force.

REISIG: Yes.

NEUFELD: Especially when they built what became the V-1. As competition for you. But at the earlier time, that it was a fairly close relationship, but what is your view of that whole evolution of the army and Luftwaffe, West and East Peenemünde facilities?

REISIG: It depends on which level you are discussing it. In Peenemünde as such, we had the very best relations to the Luftwaffe people. We made a lot of friends there, still have some of these friends around, like Max Mayer who was one of the chief pilots.

NEUFELD: What's his name?

REISIG: Max Mayer.

NEUFELD: Max Mayer. Okay.

REISIG: And we called him "Mäxchen." He was a bachelor still at that time. We had a lot of fun together. And then we worked very close with the chief pilot at Peenemünde, Erich Warsitz. He was a fine fellow. Unfortunately he got pretty sick. He wasn't allowed to fly any more. And lots of others. And we helped each other. There was not a single moment that we had a feeling we are fighting each other because of our projects. It was technical people, we didn't even think of that.

NEUFELD: Okay.

REISIG: For instance, I was in charge of the Wolman tracking system and I also had from the beginning the optical tracking system. We tracked together. We tracked their V-1 flights with them, and they tracked our V-2 flights with us. There was the closest communication; we had the same communication network and everything. It's absolutely foolish to say that the only thing we needed was guns to shoot at each other. That's absolutely silly.

NEUFELD: Yes. So as far as you were concerned, throughout the whole pre-war and the war, at the working level between West and East, there was no tension at all over any specific details. It appears that there was tension, but maybe that's incorrect too, at the level of the top and the army leadership and so on. Were you aware of that at all, that on the other hand there was a fight going on in Berlin?

REISIG: That's a very complex matter. Just some kind of an experience I had personally which might illustrate it. Dornberger in my opinion made a big mistake when he invited high level people to our first missile firing, which went out of control.

NEUFELD: Right, that was June '42.

REISIG: I was detailed to observe together with the high level party this firing, so I could listen to their talk. Definitely Milch and Speer were there and other high level people, and I remember very well, when the firing was over and we stepped down from this--

NEUFELD: --platform?

REISIG: No, it wasn't a platform.

NEUFELD: Viewing stand?

REISIG: It was just a pedestal on the roof of the BSM Building. I was standing on a step, and Milch and three other government people were passing by, and Milch said to Speer, "We've already got that." And Speer said, "What?" And Milch said, "Kirschkern." You see, that was one of the--

NEUFELD: --other names--

REISIG: --other names for the V-1. That's what I heard with my own ears. On this top level was so much criticism when your project "A-4" became better known, particularly after our first success firing, that Speer said, "Well, we must bring things to order," and he established the so-called firing commission.

NEUFELD: Yes, the Entwicklungskommission für Fernschiessen.

REISIG: Yes. Which had Professor Petersen as the head. And the air force people--the ministry people hoped to kill the V-2 project on this occasion. There was a conference in Peenemünde in which I took part to a certain extent. There was a pretty good protocol of this conference.

NEUFELD: Are you talking about the day of the so-called Vergleichsschiessen? Comparison shoot of May 26, 1943? Or are you talking earlier on, before that competition firing took place? (crosstalk)

REISIG: I'd have to look--

NEUFELD: I guess in May '43 you were away at that point probably, were you not? That was about the time that you had gone into basic training.

REISIG: Ja. It was before then.

NEUFELD: Before then.

REISIG: I have it. You didn't run into this document yet?

NEUFELD: No, I haven't, but I haven't done much primary documentation yet.

REISIG: I wonder whether I could pick it right away. Let me try.

NEUFELD: Okay, let me turn the tape off. ...

REISIG: I remember the commission we had before this date,

before I went to Russia.

NEUFELD: So you were at this meeting of the 9th of September, 1943?

REISIG: No. I think it was in the fall of 1942.

NEUFELD: Under this commission which has been translated as the Long Range Bombardment Commission.

REISIG: Kommission für Fem-schiesssen.

NEUFELD: Yes, right. And what was the outcome of that meeting? What was the gist of it, the way you remember?

REISIG: Well, the air force people hoped they had Speer on their side, and they could kill the V-2, Petersen and his committee members visited then the laboratories in the BSM building, my old lab where I worked. He said, "I'm so"--oh, I think it's even here in this protocol. He put it that way, "We have come here because we thought that we had to help you, and now we have seen all that, and know your ideas and the way you are working. We now realize you should help us."

NEUFELD: I thought that though was early in '43. That Petersen came and was so impressed.

REISIG: And the outcome was, and I think Dornberger suggested it, let's do it in parallel, V-1 and V-2, follow both projects, and Petersen I think at once was in favor of that.

NEUFELD: Yes, I gathered that deal was sort of completed in this so-called comparison shot in May of '43.

REISIG: Yes, The air force was not too lucky with their demonstrations, firings, and we had more luck for one reason or the other.

NEUFELD: Right. So just to sum up then, as far as conflict with the Luftwaffe or cooperation, it was entirely a high level fight.

REISIG: Yes.

NEUFELD: Never affecting, in your opinion, the relations between the West and the East parts of Peenemünde.

REISIG: And do you know who took advantage of that? It was the SS.

NEUFELD: How did they do that?

REISIG: They were politically smart enough, they said, "Well, you are fighting each other, we have to do it for our country and we will take it over." And I think they were right.

NEUFELD: They were right only in the sense that the inter-service battle was stupid.

REISIG: Yes, and that they, in a certain way, improved the efficiency of the whole operation. They were selfish, there was no question. They wanted to be the big shots. But fortunately they helped us.

NEUFELD: You mean, in terms of coordination.

REISIG: Right.

NEUFELD: Okay. Well, let me--

REISIG: --pushing things through, with their power.

NEUFELD: Okay. Let me hold that because obviously that's going to come into the next part, which is especially about your time on the Dornberger staff. Bzbv Heer, to use the German abbreviation. Okay. Thank you very much.