Hoelzer, Helmut. November 10, 1989. Interviewer: Michael Neufeld. Auspices: DSH. Length: 3.25 hrs.; 66 pp. Use restriction: Public.

Hoelzer begins by discussing his birthplace and early education; parents background. Discusses engineering education and doctoral dissertation on ground speed measurement. Hired by Telefunken (Berlin); worked on wave propagation and TV systems. Ordered to work at Peenemünde 1939. Discusses the development of the guide plane system (1939-40); guide beam; Schlitt lateral accelerometer. A-5 guidance system; problems with instability in the A-5/A-4 guidance and control systems; in-house capability; role of corporations; the origin of the mixing computer and simulators for guidance control systems; development of simulation and the analog computer; development of accelerometers. Discusses von Braun, Dornberger, Zanssen, Rust and others.

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Interviewee: Dr. Helmut Hoelzer

Interviewer: Dr. Michael Neufeld

Location: Huntsville, Alabama

Date: November 10, 1989

TAPE 1, SIDE 1

DR. NEUFELD: We always like to start with some biographical background, so could you give your full name, your birth date, birth place?

DR. HOELZER: Okay. First name, Helmut, no middle initial. My father thought it wasn't necessary. Born February 27, 1912, in Bad Liebenstein in Germany.

NEUFELD: Where in Germany is Bad Liebenstein?

HOELZER: Well, let me explain this very exactly. If you take the old Germany before the First World War, and cut it out and balance it on the needle, and where this needle hits the paper, this is where Bad Liebenstein is.

NEUFELD: It's in Thueringen?

HOELZER: Yes, it's in Thueringen between Eisenach and Meiningen. About half way in between them. All right, and then I went to school in Bad Liebenstein, the Paedagogium Bad Liebenstein and then from there, this was to what we here in the USA call graduation year at this level, high school graduation, and after this, I went to Bad Salzungen which is not too far, about 50 miles or so from Bad Liebenstein, and in Bad Salzungen the Oberrealschule, which means, you know, from after the normal graduation in Germany you could go two ways, either to the Gymnasium, which has nothing to do with what we call gymnasium here, so you could say you graduated from Gymnasium, or the Oberrealschule. The Oberrealschule was more scientific and the Gymnasium was more old languages, Greek, Latin, Hebrew and what have you.

NEUFELD: Traditional humanist education.

HOELZER: Yes, humanist education.

NEUFELD: Can I ask you a question about your parents' occupations and what your father did?

HOELZER: My father had a department store in Bad Liebenstein, and also a nursery. The department store he inherited from his mother, and the nursery from his father in Bad Liebenstein. My mother was also born in Bad Liebenstein, and her parents had kind

of a resort hotel in Bad Liebenstein. I have a picture here, if you want to know how this business looks like.

NEUFELD: So you went to Oberrealschule, which indicates that you were already oriented toward the sciences?

HOELZER: Science, yes, science, mathematics and things like this. Okay. This worked pretty good. At the same time, my parents had financial difficulties in Germany because this was the leftovers of the Depression which we had also in Germany, and so I couldn't go to college which I wanted to do.

NEUFELD: When did you graduate Oberrealschule?

HOELZER: 19--when was this, '28? No, 1929, probably. In Bad Salzungen. But fortunately I got a scholarship from the state of Thueringen, and this enabled me then to go to the Technical University in Darmstadt. But before I went to Darmstadt, I had to go through an apprenticeship, a training. You know, if you were to become an engineer in Germany, you have to do one year of practical work, foundry and everything else.

NEUFELD: Were you at that time going into mechanical engineering, Maschinenbau, or were you going into?

HOELZER: At that time I didn't know exactly what I wanted to do. This was a Reichsbahnausbesserungswerk. Complicated name, it means --where they repaired motors and cars and so on for the Reichsbahn, yes.

NEUFELD: For the national railroad.

HOELZER: For the railroad, yes. Okay, fine. And after this then I went to Darmstadt. And in Darmstadt first I wanted to become a chemist. Well, I really didn't know what I wanted, so I went to all kinds of lectures and tried to find my way around.

NEUFELD: You would have started then about 1931?

HOELZER: In 1932 I finally made it to Darmstadt. In between was this period in Meiningen and in the Reichsbahnausbesserungswerk. In Darmstadt then I decided to--well, let me start it this way. At that time, it was very difficult for an engineer to get a job in the first place, so if you wanted to become an engineer, you really had to know why, and I didn't. Okay. But I knew one thing, flying was coming up. If I studied aeronautics, aircraft statics and this kind of business, plus, meteorology, because for sure you cannot fly when it rains at that time, okay, so you have to know what the weather is, and you have no wire going up so you need wireless communications, in other words, you have to know something about electronics also, and I tried to do all three of

those. The chemistry I forgot about in the meantime. So I concentrated on those three things, which naturally didn't work, because there just isn't so much time. Okay. So the first thing I dropped after a couple of semesters was meteorology. I have to mention that in spite of the stipendium aid from the state, that I still had to work in the vacations and once in a while skip a semester to work and just earn money, to be able to pay the tuition. Okay, so this was the first thing I dropped. I still continued the two other ones, and then came the catastrophe. In the meantime Hitler came to power, and students were required to join the student SA. For this business I had no time.

NEUFELD: This was the NS Studentenschaft?

HOELZER: Ja, it was the Studentenschaft. I was, I joined a student fraternity in Darmstadt in the meantime. But this had nothing to do with the Nazis. The Nazis required that you joined the SA Studentensturm. Okay, now this took up too much of the time. I had at that time very little idea about what this all was about, so I tried not to go there, which brought me in trouble with the Studentensturmfuehrer. And well, tempers got a little bit high and rough, and he threw me out of his office. Funny enough, a couple of weeks later my money from the state stopped. Whether it had anything to do with our difference of opinion, I don't know. It just is the coincidence.

NEUFELD: Striking coincidence.

HOELZER: Striking coincidence.

NEUFELD: Maybe no coincidence. This was mid-'33 I assume.

HOELZER: This was about '33, ja, mid-33, end of '33 I would say about then. So I had to stop studying and continued to work, joined some companies to make money. Then I left Darmstadt. I did not join the SA. Well, at that time, I got an offer from a college in Bad Frankenhausen, Kyffhaeuser College, Kyffhaeuser Technikum was the name in German. This was an aviation college, and they were looking for somebody to teach there. Okay, now I didn't have the exam yet, and no degree and nothing, and I applied anyway. So they invited me to Frankenhausen and I gave a couple of lectures, after which I got the job.

NEUFELD: Was this in Thueringen?

HOELZER: This was in Thueringen also, yes. Okay, so I got that job, and I had this job for, this was in 1935, in '35 I got this job, beginning of 1935.

NEUFELD: What did you do in '34 then in the meantime after you left Darmdstadt?

HOELZER: I worked in Gera. Also in Thueringen, on an overland electrical net we put up, electrical power lines and things like this. It was not related to my work. It was something electrical, heavy current electricity. Okay, so this was the beginning of '35 in the meantime, and in Frankenhausen I taught mathematics, aircraft statics, electronics--you see what kind of hodgepodge this was. And well, this was in Thueringen. And then after a while I found out that I'd better go back to Darmstadt to study, to continue my studying and get a degree. All right. In Darmstadt. Well, I was busy at that time with motorless flight. Frankenhausen being an aviation college, it had a large group of motorless flight people there, a club, so I participated in this business also, and there I learned that in Griesheim, very close to Darmstadt where I went to the university --

NEUFELD: Technische Hochschule.

HOELZER: Technische Hochschule then, very close is a village with the name Griesheim, and in Griesheim there was the Deutsche Institut fuer Segelflug, German Institute for Motorless Flight, under Professor Georgii. Georgii was the guy, the professor in Darmstadt who taught meteorology. ...Georgii was about, what, seven feet tall, barely could go through the door. I knew Georgii when he was my professor in Darmstadt at the time when I still was doing meteorology, which I dropped, and so I --and I found out that in the meantime the Institute for Segelflug, for Motorless Flight, had a department number 12 which was an Ingenieurschule, an engineering college, aviation engineering. Okay, so I went to Georgii and asked him about the possibility of teaching here, told him I was at Frankenhausen which he knew, and I got the job finally in Darmstadt.

NEUFELD: At Griesheim or?

HOELZER: At Griesheim.

NEUFELD: What time?

HOELZER: Well, let me see, how long was I at Frankenhausen? This was '37, I believe, something like this. And in Griesheim, I could teach and could study at the same time. I had a motorcycle, and traveled from Griesheim to Darmstadt and back and forth, taught in Griesheim and studied in Darmstadt. By the way, I did the same thing already in Frankenhausen. not quite to that extent. After the first two semesters in '35, I convinced the director of the Institut in Bad Frankenhausen that he should compress my teaching to the first three days of the week, Monday, Tuesday, Wednesday. Wednesday evening I would leave and arrived in Darmstadt around midnight. Thursday, Friday, Saturday, Sunday, I was studying in Darmstadt at the Technische Hochschule, and Sunday night I would go back to Bad Frankenhausen, arrived in the

morning at 8 o'clock and at 8:30 I was teaching again. Okay, so I did this for a couple of semesters, and then I went to Griesheim. All right, in Darmstadt, I worked also on the Institute of Practical Mathematics under Professor Walther--Praktische Mathematik--

NEUFELD: Praktische Mathematik...

HOELZER: . ... I mention this now because I have to come back to this business later on, because in Peenemünde we had a contract with Walther, to do computations. This was where the computer comes back in. Okay, now let me go back to Darmstadt. I finally graduated then in Darmstadt, but not quite. My Diplomarbeit, my master's degree thesis wasn't completed, wasn't even started, but I had to make money so I joined Telefunken. And I had an agreement with the Institute for Electronics and the Technische Hochschule in Darmstadt that I could work at Telefunken, and also complete at the same time my master's degree thesis, which I did, and this is fine.

NEUFELD: Okay, what year did you complete the Diplom? Was that around '39 or so?

HOELZER: '38, late '38.

NEUFELD: And Telefunken was in Berlin?

HOELZER: In Berlin. And the high frequency research laboratory is where I worked. My boss was a guy by the name of Rosenstein, Dr. Rosenstein, and well, I also, after I had my master's degree, I asked the Institute in Darmstadt whether I could start with my PhD at Telefunken, work there, go back to Darmstadt, and get a PhD. All right, now, this kind of business didn't work out because in the meantime the war broke out and I got drafted, civilian draft. Let me go back to Bad Frankenhausen once more, for the following reason. While I was doing motorless flight in Frankenhausen, and around Eisenach and other places, I always thought that there was one thing missing. You could never be sure that you had, what kind of speed, comparable to the earth you had in your glider, in your plane, because the only thing you could measure was the speed relative to the air.

NEUFELD: Air speed relative to --

HOELZER: Air speed, yes.

NEUFELD: Air speed relative to the air.

HOELZER: To the air. Not to the ground. Okay, so at the same time there was a contest by the German, what was this? Forschungsanstalt, Deutsche Luftfahrtforschungsanstalt. German

air research institute. One task was to find out how to measure the speed, the aircraft speed versus ground, and the second one was to determine how much fuel you have left in your tank. At that time this was very primitive and had to be improved. Okay. Prize: 10,000 marks, which I needed badly. So I tried to work on this contest. This was in 1935, by the way, the idea was to measure the acceleration with a spring mass-damping system and then intergrate this valve to obtain the velocity. But pretty soon I found out that I needed a laboratory to work in, which I didn't have, and which wasn't available in Frankenhausen. So I went to Darmstadt, and went to my electronics professor, Busch, Hans Busch. Hans Busch, by the way, was the guy who first computed the first electronic lenses. Okay.

NEUFELD: What do you mean by "electronic lens" ?

HOELZER: The way you have them in TV tubes.

NEUFELD: Okay, cathode ray tube.

HOELZER: Cathode ray lenses. To focus cathode rays. Okay, not magnetic focus, an electrostatic focussing, this was his work. I don't know whether he ever worked on the magnetic focussing side too. I don't know. Okay, Busch said, "Well, this is a king sized enterprise, what you want to do here, but you can't do this here. You have to go to the Department of Measuring Techniques." This was Professor Hueter. So I went to Professor Hueter, and Professor Hueter wasn't there, so I talked to his assistant professor. His name was Debus.

NEUFELD: A name certainly well known.

HOELZER: Okay. So I talked to Kurt Debus, and he said, "Well, wait a minute, we have to talk to the Old Man, I can't say yes or no. Let's go to the Old Man." Okay, so we went to Professor Hueter.

NEUFELD: This is not the same as Hans Hueter, who was --

HOELZER: No, no relation. Okay, Professor Hueter then finally said, "This is again too much. If you want to do some doctor's thesis later on, you come back and you can work on things like this. Even for just a normal pre-diploma thesis, this is far too much, and you would have to spend too much of your time here," and so he turned me down. And so this was the end of this business. And I worked on this idea theoretically a little bit, but this was all I could do at that time, and I didn't get the 10,000 marks either, so -- Okay, now, this was, back to '35. In the meantime, where were we? Telefunken, Okay.

NEUFELD: '38 we are, '39.

HOELZER: '38, '39. In fall '39, the war broke out, and in Germany we had two kinds of drafts. One was the military draft, you became a soldier, and the civilian draft, you had to go and work in the defense industry some place. One was a blue letter, the other one was a pink letter, and I got the pink letter, or the other way around, I forgot. Anyway, I got orders, report in Peenemünde. Where this was, I had not the slightest idea. By the way, in Telefunken I worked on wave propagation, and to a certain extent, at least, wireless aircraft guidance control systems, but this was secondary. The most important thing was wave propagation then. Okay.

NEUFELD: Wave propagation as applied to what? What specific technologies?

HOELZER: This was, we would say microwave propagation, because if you understood the wave propagation, how they propagate, and if you wanted to do this research in the laboratory, you have to do it with very short waves. So we had high frequency, so centimeter waves. For instance, how do centimeter waves propagate on a sheet of metal with a trap in the middle of a quarter wave length or so? Would they jump over it? Would they stop there? What would happen? Okay, so I worked on this business. For instance, how to design a TV antenna without having the surface current on a shielded cable antenna spill into the inside of the cable? You know, high frequency currents stay only on the surface because of the skin effect. They cannot travel inside of the metal. So in a concentric cable, you have three surfaces. One is the one in the middle, what do you call this, the middle conductor, and the other one is the inside of the cable shield, and the other one is the outside of the cable shield. So you want to eliminate what comes outside of the cable because the cable usually goes from the ground up to the roof, and you collect all kinds of sparks and what have you, disturbances, which then spill into the antenna, or can spill into the antenna, and from there go to your TV set. So the idea was, how to block this? This was one of the things I worked on.

NEUFELD: So Telefunken at that time was trying to look at the very earliest TV systems.

HOELZER: Oh yes, we had TV systems at that time, yes. There was TV, yes. Okay, so those were the things I worked on, and also how to use centimeter waves to guide airplanes, for instance. All right, now, while I was in Berlin at Telefunken, before I got drafted, one day I had some visitors in the evening. They whistled on my window and I looked out and here were three guys, two of whom I knew from the Institut fuer Segelflug in Griesheim. One of them was Dr. Steinhoff, whom I also knew from my studies in Darmstadt. The other one was Dr. Steuding, who was in Griesheim also. And the other one was a guy I didn't know, young

guy, whistled all the time. I asked them, "What are you doing in Berlin?" "Oh, we are here on business. How about we go and have a glass of beer?"

NEUFELD: Who was the third man?

HOELZER: I'll come to this. The third man didn't pay any attention to what I had to tell. We talked about old times and so on. And he listened to the music and whistled and was quite happy. After a while, he turned around, "Say, I want to ask you something. You work on wave propagation and things at Telefunken?" I said, "Yes." "What do you do if you have a flying body and you want to keep this flying body on a straight line?" I said, "Well, it just depends on the body. There's a difference. If you take a piano and throw it down the Empire State Building, or if you have an airplane, you have to use different methods. So what kind of a flying body is this?" He said, "Well, I can't tell you." I said, "Well, I can't answer it." So things like this then occurred a couple of times during the evening, and he always said, "I can't tell you," and I told him, "Well, I can't answer, sorry." So this was then the conversation on that evening. All right, and soon afterwards, a couple of weeks later, I got my blue letter, to Peenemünde, and in Peenemünde I arrived then on Monday morning, and no, not the Monday morning. Monday morning I left in Berlin, I arrived in Monday evening.

NEUFELD: Was this September '39 already, right at the beginning of the war?

HOELZER: The war had already started.

NEUFELD: A few weeks before, months before?

HOELZER: A few weeks before. A few weeks before. Then I went in October, I believe, October, something like this, end of October probably, in '39. Okay, and when I arrived in the evening they that took me to the officers' club in Peenemünde, and I was introduced to the same guy I met in Berlin. His name was Wernher von Braun. I said, "What are you doing here?" He said, "Well, I'm so to speak in charge here, and you better listen to what I have to tell you." Okay, fine. So we talked about things then and they explained to me what I have to do. The problem was, they had a rocket, the A-5, not the A-4, the A-4 was still on the drawing board, but the A-5 was ready for flight but the wind blew it away. There was a little island on the Baltic Sea, Greifswalder Oie where the A5 was launched from.

NEUFELD: Yes. Are you really thinking of the A-3 failure in 1937?

HOELZER: No, A-3 was before my time.

NEUFELD: That's what I'm saying, the A-3 in '37 was a failure from a guidance standpoint, so that --

HOELZER: They were all failures from the guidance standpoint.

NEUFELD: So in '39 when you came in, they had just about been ready to launch A-5 with guidance, right?

HOELZER: Ja, well, not, we have to make a differentiation between guidance and control. The control is the gyro system, and guidance was something new, either inertial or wireless guidance. And on the A-5 even the control system, the gyro system, didn't work well enough, right. It worked after a fashion.

NEUFELD: Okay, before we go on with that I just want to finish the Wernher von Braun question. So the first couple of times you met him, first time you didn't even know who he was.

HOELZER: Sure.

NEUFELD: He didn't even give his name at that time?

HOELZER: Well, they introduced me, ja, but I forgot the name. It didn't say anything.

NEUFELD: Didn't mean anything to you.

HOELZER: --brown, yellow, green.

NEUFELD: At that point, after you came to Peenemünde, did he strike you as an impressive person right away? Or too young?

HOELZER: No, he was impressive even at that evening in Berlin.

NEUFELD: In which way?

HOELZER: The way he talked and the way we talked about things, I had a very good impression about him there. Naturally, what was in the way was that he couldn't talk and I couldn't answer. But otherwise I had a good impression about this guy. A good mind.

NEUFELD: I'm curious about his manner and so forth. Did he strike you at that time as aristocratic?

HOELZER: He was, he was. I thought he was really arrogant at first.

NEUFELD: Like he was Prussian type?

HOELZER: No. No. He was more kind of, at that time, he was very young, he was at my age, we were both 1912, so at that time how

old was I? I was very very young.

NEUFELD: 27.

HOELZER: 27, something like this, and he was too. And he listened to the music and sat there, and they played, I still remember this, he played the Belle Ami.

NEUFELD: It's a brass band?

HOELZER: Ja. So he was, he was very much more interested in the music and the girls who run around and everything, instead of what I had to tell, ja. He couldn't say anything anyway. Okay, so this was--now, in Peenemünde now, he told me what this all was about, and he said, "I want to show you something." This was next morning and he took me to a test stand. The test stand was not quite completed yet. They had several of them, and there were heavy boards for your protection in front of it, so, and there was a hole in one of them and a knot hole in the other side and he told me, "Now, you look through this hole, see what happens." And I looked through the hole. He looked through the other hole. Then all of a sudden main stage came of the engine, they were testing voom, and the board got loose from the shock wave and blew in my face, and I had a bloody nose. So he laughed like mad. He said, "Well, this will happen more often to you here in Peenemünde, not only with this board but other things".

NEUFELD: What size engine was that? Was that a 25 ton thrust engine?

HOELZER: I don't know.

NEUFELD: It was a smaller one, probably.

HOELZER: I don't know. At that time I didn't know what an engine was. And nothing, I was just introduced to this whole business. I don't even know which test stand it was, because we had to drive through some type of forest there, winding roads, and --

NEUFELD: --you didn't know where you were.

HOELZER: I didn't know where I was. Okay, fine, so I got told what my job was. Here is the A-5, the A-5 flies all right with gyros but it gets blown away by the wind and "you do something about it." Okay, so I told him, "Well, I can do it in two ways. Either you can measure the lateral acceleration and integrate it twice." This was the old idea in Frankenhausen, the one that was turned down by Hueter in Darmstadt. If you interate once you get the velocity and if you integrate once more, you get the distance, the distance from the original path, and those three components - lateral acceleration, velocity and deviation - you

can use now for a guidance system. Or you can also build a wireless guidance system. In other words, you create a certain radiation pattern with antennas on the ground and you have an antenna in the rocket, and this antenna can determine in the field whether it was left or right from the guide plane, and then correct this business. Okay. So we decided to pursue both of these possibilities. We had a contract then with Hueter in Darmstadt. The man on Hueter's staff was now a Dr. Schlitt, Helmut Schlitt, and Schlitt got now the job to build this accelerometer which I was not allowed to build in 1935. So Helmut Schlitt came up with an accelerometer for this purpose.

NEUFELD: On which principle? Because of course I was talking to Dr. Mueller and he was explaining to me his development of accelerometer on the basis of unbalanced gyroscope.

HOELZER: Ja. That's Okay, ... I have a publication here, a very early publication, I think, let me see what I can --I thought you might want to have this. No, it's not in here. Okay, now we have it here - this is a reprint, you said you have this--

NEUFELD: "German Missile Accelerometers," 1949, written by Thomas Moore.

HOELZER: Okay, fine. Now, those accelerometers here were used for two purposes. One, to measure the acceleration of the vehicle, to find out the velocity when the thrust cutoff should occur.

NEUFELD: Right.

HOELZER: To determine the range. And the Schlitt accelerometer here was used, could have been used for this purpose also, but it was then actually used for lateral guidance, to measure the lateral acceleration of the vehicle. And Schlitt then used, to jump ahead a little bit now, he used then my integrators from the analogue computer development to integrate this to get the velocity, the lateral velocity, and integrate once more to get the lateral deviation, and the Schlitt accelerometer was mounted on Mueller's stabilized platform.

NEUFELD: The later SG-66 platform.

HOELZER: We had several of them there, I don't know which one.

NEUFELD: 66 was the A-4 stabilized platform.

HOELZER: Okay, that should be in here, I forgot, I couldn't keep the numbers in mind. All right, this is where the Schlitt accelerometer was used.

NEUFELD: That was developed at Darmstadt.

HOELZER: Ja, at Darmstadt, Schlitt was at Darmstadt and came later on to Peenemünde and joined my department in Peenemünde for this purpose. It was his accelerometer and my integrators which made the first inertial guidance system out of this, and these were successful flights. I forget now many we had, not many, two or so, ja, with the Schlitt accelerometer test flights, and worked perfect. Accuracy, I don't know, I forgot.

NEUFELD: Okay. So let's backtrack to '39 then. So you came in, and they said, the A-5 guidance system as it exists, and I assume they were talking about the Kreiselgeraete system, that had been developed, or was it the Siemens system?

HOELZER: Let me clarify this business. We had contracts with four companies to build a gyro control system. One was Siemens, the other one was Kreiselgeraete, the next one was Anschuetz, and the other one was --what, Anschuetz and the other one was --

NEUFELD: The navy was involved in there somewhere, wasn't it?

HOELZER: No. Wait a minute. It will come back in a minute. They made theodolites too. What was that, the name of the theodolite company?

NEUFELD: Theodolites.

HOELZER: Theodolites, yes. Anschuetz was one. Askania.

NEUFELD: Askania, Okay.

HOELZER: Okay, so this was Kreiselgeraete, Siemens, Anschuetz and Askania. And all four of them developed a contol system for the A-5.

NEUFELD: Including gyroscopes on all four?

HOELZER: Including gyroscopes. This was a gyroscope controlled system. It was always with gyroscopes, ja.

NEUFELD: All four.

HOELZER: All four, ja. All four used gyroscopes, three gyroscopes, and used also Wendezieger. This means rate gyros.

NEUFELD: Rate gyros.

HOELZER: Okay. And with one exception, and this was Anschuetz, no, Askania, Askania had Mr. Moeller. Mr. Moeller had a special kind of a rate gyro. This rate gyro was built in such a way that it could also measure angular acceleration, kind of, it was not directly acceleration but it came to be very close. And so

Anschuetz could afford to build a control system where no position feedback of the jet vanes was necessary. This was, we called it Laufgeschwindigkeits-Steuerung instead of Stellungszuordnung. Stellungszuordnung means you have position feedback of the position of the jet vanes to the gyro, and Moeller used the velocity of the jet vanes, not the position. But this he could only do because he could also measure in some fashion the angular acceleration. Okay. Now--

NEUFELD: --let me just stop you here for a second...

TAPE 1, SIDE 2

NEUFELD: Okay, continue.

HOELZER: All right. Now, neither one of those control systems worked satisfactorily. For instance, there was the Kreiselgeraete stabilized platform system, which used the stabilized platform plus some mechanical links from the stabilized platform all the way down to the vanes, to the jet vanes. Our theoretical guy Dr. Steuding did some stability investigations, theoretical investigations, and found out that the Kreiselgeraete control system should be unstable. But it wasn't, it was stable, on the test stand. So there was a big argument between Kreiselgeraete and Steuding. Steuding said it doesn't work and Kreiselgeraete said, "Look at it, it works." This was the difference between theory and practice. Theory is if something is supposed to work and doesn't, and practice is if something works and nobody knows why. Okay, anyway, Kreiselgeraete said, "We can make it even work better." They took an oil can and oiled the mechanical linkages in the feedback system, whereupon the control system became unstable the way Steuding had predicted it. It's a well known fact, that a friction in the feedback system, has a stabilizing effect, and this is what saved the system, and after they oiled it, it didn't work any more, became unstable. All right. They finally got their systems to work on the A-4. No, in the A-5. For the A-4, this was a different story, and I have to come to this especially because this leads then to the introduction of analogue technology into the control system and the overall rocket development. But let's do one step at a time. We finally developed then a system, a wireless guidance system which worked for the A-5.

NEUFELD: You mean a radio control?

HOELZER: Radio guidance system. We had a contract then with Lorenz company, and Lorenz company built those things. Lorenz was chosen because they had something similar before, for airplanes, but there were some shortcomings in the Lorenz system. Do you want to go into the technical details?

NEUFELD: Yes, absolutely.

HOELZER: Okay.

NEUFELD: I'll try to follow. At times I'll have to stop you.

HOELZER: Okay, just ask. I will try to explain the system without pencil and paper and at least the essential things. First, the radio guidance system was not a guide beam system but a guide plane system, the yaw plane only. A vertical plane. This plane was the symmetry line of a radiation pattern produced by a directional antenna system which was located on the ground about 10 miles behind the launch site and therefore in direction flight path. On the right hand side of the symmetry line of the radiation pattern the field strength increased and on the left hand side it increased also. This produced a kind of valley in the radiation field strength in which the rocket should fly. To distinguish between the right side and the left side of the valley, the left hand side was modulated with audio frequency f«MDSD»1«MDNM», and the right hand side with audio frequency f«MDSD»2«MDNM». The two frequencies, however, were not radiated permanently but switched in a 50 Hz rhythm. About two years ago I sent a description of this system, including diagrams, to Mr. Winter of your organization (the National Air and Space Museum in Washington).

NEUFELD: And this is a diagram of the guide plane as developed for the A-5.

HOELZER: And for the A-4. It's the same thing.

NEUFELD: The same one was applied.

HOELZER: The same one was applied and put on the A-4.

NEUFELD: There wasn't any significant change between --

HOELZER:--there were no changes, no.

NEUFELD: So was there a switching back and forth between those two beams?

HOELZER: Ja. Ja.

NEUFELD: The beam switched, flipped back and forth between those two --

HOELZER: A diagram of amplitude versus time is here. You see. This was flipped from this side to the other side, this side, the other side, this side, like this, back and forth. This is the diagram. What Lorenz did, Lorenz Company was, they modulated this

with 5000 cycles per second and the other side with 7000 cycles per second, and when both are equal, you are in the guide plane, when the left hand side was larger the rocket was to the left and vice versa.

NEUFELD: Okay, that shows the position of the missile relative to the guide beam, in order for the control system to bring it back into the center.

HOELZER: The control system then received this value, and used it then to bring the missile --

NEUFELD: --so that the amplitude of both beams should be equal at the point where the missile is riding down the center.

HOELZER: Okay. Now, the difference between the old Lorenz system, to my version was this. Lorenz measured the amplitude of the modulation at the left hand side and the amplitude of the modulation here at the right hand side separately.

NEUFELD:--of the two beams.

HOELZER: Yes, they had amplified these two audio frequency amplitudes in seperate amplifiers to avoid DC-amplifiers and their problems. The audio amplifiers introduced noise. Let's assume channel A is 110V plus 1V noise and channel B is 90V and 1V noise. Then the noise is about 1% in each channel. If we build the difference we have Δ = 110V - 90V = 20V. The noise does not cancel out, it adds up to 2V which is, based on 20V signal, now equal to 10%. This is a lousy percentage. This percentage gets worse the closer you come to the guide plane. My own solution was not to use the difference in the audio frequencies at all. I used the amplitude of the switching frequency of 50 Hz which is zero in the guide plane and increases the more you deviate from the guide plane. It changes phase when it goes through zero. If you study the little write-up to Mr. Winter, you will agree. This is the way the system was finally built.

NEUFELD: That's the amplitude of the difference between --

HOELZER: Yes, the amplitude of the high frequency of the carrier, not of the modulation. And the amplitude of the carrier, you see, is zero here.

NEUFELD: When the missile's riding the center of the beam, the amplitude of the carrier --

HOELZER: --is zero. On the other side, the phase has shifted, it's now like this instead of like this. This means left. This means right. And this is the fundamental difference between the

system I devised in Peenemünde, and the original Lorenz system.

NEUFELD: Okay, so that when the missile is riding the center of the beam, essentially the two beams, the amplitude of the carrier wave, the carrier waves essentially cancel each other out, zero each other out.

HOELZER: And the phase of the switching frequency, which changes 180 degrees when going through zero, is measured against a permanent 50 Hz frequency obtained from one of the still existing audio frequencies, but this has no influence on the accuracy or the noise at all.

NEUFELD: Okay, so that gives you the phase.

HOELZER: Gives you the phase, yes.

NEUFELD: Okay.

HOELZER: Okay, so that was this thing. You can have this copy if you want to.

NEUFELD: Okay, was your first task then to do the --

HOELZER:--this was my first task.

NEUFELD: Was to produce the guide beam.

HOELZER: Ja.

NEUFELD: Modify the guide beam so it could be used for A-5 initially.

HOELZER: Ja, right, that was my first. All right, naturally the very first thing you do is to do some computations and find out how the rocket in flight would behave in the guide beam. I found out, this thing's unstable. To explain this accurately is complicated but let's try this anyway. Suppose the rocket is on the right hand side of the guide plane. to get it to the middle, we have to turn the rocket so that the nose points to the guide plane. To do this, we have to tilt the jet vanes in yaw. After the rocket has turned, it flies now toward the guide plane. When it arrives at the plane, the rocket is still tilted even if the jet vanes are at zero position where they should be, because at the guide plane they receive the signal zero. So the tilted rocket overshoots the guide plane to the other side and the game repeats itself, only next time it is worse because of the inertia of the other components of the system, like servo motors, which also take some time to operate.

NEUFELD: You produce a greater and greater --

HOELZER: --greater and greater--

NEUFELD: --deviations from the central beam of the trajectory.

HOELZER: Once there's a deviation, the deviation would get larger and larger and then finally at propulsion cutoff the thing would go off in some wild direction.

NEUFELD: This was assuming that you had an electromechanical control system, I assume, where you had some kind of mechanical device, as in Kreiselgeraete's gyro system, to turn the vanes.

HOELZER: Either this or some electrical device, ja.

NEUFELD: Even if it was electronic.

HOELZER: Even if it was electronic, this wouldn't help. Okay now after the vehicle has gone through all this kind of business, time is over. It should be in the middle and it should be straight again, but it isn't, it's like this and it shoots in this direction.

NEUFELD: It overshoots in the other direction.

HOELZER: It overshoots, and it overshoots even worse when it comes back here, and so on. And this is the instability I'm talking about. So the thing was unstable. Now, according to the stability theory, which I will explain later, you need the first derivative of the measuring value to mix this into your control signal. In other words, to make sure that --where is our substitute rocket? In other words, to make sure that when the rocket tries to go back to the guide plane, that it turns before and slides in like this, not like this.

NEUFELD: It doesn't overshoot.

HOELZER: Doesn't overshoot. You have to straighten it out before it gets there. You have to measure or compute the lateral velocity of the rocket and mix it then with your other signal to stabilize the system. The control system in contrast to the guidance system uses for this purpose the rate gyro. The rate gyro measures the angular velocity. The free gyro measures the angle of deviation, the angle itself. All right, now, in the case of the guide beam system, or better guide plane system, there was no possiblility to measure the lateral velocity, so it had to be computed, and the mathematical operation to compute this is differentiation. In other words, you have to have a little apparatus which can differentiate, and then after this you have to have an adder to add the original value plus its first differential quotient together, and feed this then into some amplifier which would send the combined values to the servomotor

which turns the jet vanes.

NEUFELD: Okay, to control the position of the vanes.

HOELZER: Ja. Okay, now how do we differentiate? All right, now, this is very easy for a guy who comes from the electronic side. You could use for instance the charging current of a condensor, which is the differential quotient of the voltage on the condensor. And you need an amplifier which then can amplify the DC current so to speak or better a very slow changing DC current. All right, so the very first thing I did then was to build a DC amplifier, which didn't work. The DC amplifier was not stable enough. It could not be used in such a rocket. It worked fine on the laboratory table, but that was all. So the question was, what then to do? You had to find out why is it unstable. Okay, it is unstable because in a vacuum tube amplifier or any other DC amplifier for that matter, the input current or the input voltage and the supply voltages cannot be distinguished from each other. So if the output of a DC amplifier shows some value, you don't know whether this comes from a fluctuation of the supply voltages, or whether there was an input voltage which was to be amplified. Okay, now, the easiest thing to do is separate those two. The supply voltages, plate voltage, grid voltage and so on, on an electronic tube amplifier, or in a transistor amplifier nowadays, have to be DC. But there's no reason why the input voltage, the signal voltage cannot be modulated on a carrier, which enables you then to distinguish it from changes in the supply voltages. All right, so this I did. I used a modulator and modulated my signal value on a carrier. And then this carrier voltage could be amplified And could be rectified again on the end with no error. The modulator we used for this thing was called a ring modulator, and then after the amplification came a phase bridge rectifier, which could differentiate between left and right also, and this was then the ersatz of a DC amplifier. In other words, it was a DC amplifier which used an AC carrier. Okay. So --

NEUFELD: Well, some of that leaves me behind. The fundamental point here is you're trying to, you had to somehow combine the rate gyro measurement with the --

HOELZER: But there was no rate gyro. There was a condensor whose charging current was the value you are looking for. And this signal was very small and had to be amplified.

NEUFELD: So there's a substitute for a rate gyro. You would mix the differentiation with the original value.

HOELZER: Ja. Right. Except that we are talking here not about the gyro-or attitude control system, but the guidance system. We will come to the substitute for the rate gyro a little bit later.

NEUFELD: --the correction to be sent to the jet vanes, and then you would produce the proper set of control signals, in order to move you back to the center of the guide plane.

HOELZER: Ja. But this is not the only problem. suppose you have a steady wind. This is the guide plane here --

NEUFELD:--vertical plane --

HOELZER: Right, and suppose you have a steady wind. If you have a steady wind and you want to correct for this, you have to counter it. Suppose the wind comes from the left, you have to counter this with a little tilt of the rocket, to the left so the rocket wouldn't fly like this, with the nose straight ahead, it would fly like this, because this component of the jet force and the wind force should compensate each other, so that the thing can fly --
NEUFELD: It flies at a slight angle of a couple of degrees away from the guide plane?

HOELZER: Right. Okay. Now, if you must have such an angle, if you want to keep this up, you have to tilt your vanes. In other words, if the wind would be zero, your vane anger would have to be zero. But if the wind is like this, your vanes have to have a little deflection, actually permanent.

NEUFELD: Actually two vanes, right, the two opposing ones.

HOELZER: Well, in this case both work in the same direction. The two opposing ones were used for Drall (roll). What's this?

NEUFELD: Pitch or roll.

HOELZER: Roll, for roll, the two opposing ones were used for roll. The ones we are talking about were both used in the same direction. So in other words, you need now something which is very close or analogous to a trim flap in an airplane, As long as the wind is permanent, you have to have a permanent deflection of some trim surface. In other words, you have to build up some permanent signal which stays even if the input is zero. The mathematical operation to do this is integration. So your little apparatus to differentiate should do one thing more, it should also integrate. All right. We had to build another computer part, a part which could integrate. All right, what do you use? You use a condensor again, but this time you have to force a current through the condensor, and the voltage which then builds up on the condensor is the integral of the current which you have forced into the condensor. All right, so you reverse this whole business, use the same AC amplifier, plus modulator and phase bridge rectifier, but use the condensor a little bit different. The other way round. So we have now some instrument, some gimmick

on board which can differentiate, which can add, and which can integrate. All right, now, this was the original guide beam receiver computer combination. All right now, so far so good. But now came the A-4.

NEUFELD: Okay, let me anchor this down time-wise. You were given this guide beam assignment in 1939 as your first task, right?

HOELZER: Yes.

NEUFELD: And over what time period did you solve these problems that you discussed?

HOELZER: This was in about, I would say, a couple of months in 1940. In 1940 we had the first laboratory model of the guide plane system.

NEUFELD: At the beginning of 1940, would you say?

HOELZER: Well, that's a hard question for an old brain to remember.

NEUFELD: Because you were there in October -- a couple of months --

HOELZER: Probably late in 1940.

NEUFELD: Okay, so it was more like a year.

HOELZER: About.

NEUFELD: Yes, so it was more like a year it took to finally produce--

HOELZER:--a laboratory --

NEUFELD:--a laboratory system. You mean suitable only for ground purposes, or suitable for installation in an A-5?

HOELZER: I think--well, the thing was this. There was no series fabrication for the A-5. Everything for the A-5 was laboratory style built in Peenemünde.

NEUFELD: Yes, of course.

HOELZER: And I think we had something at the end of 1940 which flew in the A-5.

NEUFELD: So around that time you tried the A-5 with the Leitstrahl.

HOELZER: Ja. Okay, now comes the A-4. Well, something else happened in the meantime. Before we come to the A-4. Once you have some building blocks which can compute, which can integrate, which can differentiate, which can add, which can multiply, multiplication is a modulation process, we had three modulators which could multiply, so you have some ingredients already of something which can compute. Now, this was not the idea in Peenemünde, to build things which can compute. This was playing, and this was verboten. So one day von Braun came into the laboratory, on his visits, and he said, "Well, how far are we, and what is this?" And I very proudly told him, "This is something you can compute with." And he said, "You'd better stop that nonsense. Go back to your job there. Quit playing with electronic toys, and that's the end of it, period." Okay, so this was the end of the beginning of the computer. ...Okay, I just wanted to mention this. This happened about at this time. Now comes the A-4. Do you have any questions about this?

NEUFELD: Well, yes. I want to go over it relatively slowly, because it may take a few hours, because there's a lot of detail. I had some questions about the nature of the organization and the buildup that took place at this time. Obviously you can only say what happened after you came in October, '39. But it's apparent that, you know, up through the A-3 launches in '37, Kummersdorf and Peenemünde were very dependent on Kreiselgeraete for guidance systems, and then the A-3 guidance system was a failure, although an informative failure, and they started reaching out, and they brought in Siemens in '38 to build a gyroscopic system, and as you say, they looked at Askania, and they went to, what was the other?

HOELZER: Anschuetz.

NEUFELD: Anschuetz for gyroscopes, so they launched a number of competing things, and then somewhere around the beginning of the war, they really started building the guidance and control laboratory up as a significant in-house capability, right, which became BSM.

HOELZER: Dr. Steinhoff's --

NEUFELD: Right, so Steinhoff came probably not very long before you, did he not?

HOELZER: Steinhoff? Yes, he came before me. Steinhoff stayed in DFS in Griesheim until, I don't know, anyway, he left before I did, in Griesheim, and he became a pilot for some airline before Peenemünde, and he must have come to Peenemünde in '37 probably.

NEUFELD: Actually I think it was as late as '39 or '40.

HOELZER: No, no. He was with the group I met in Berlin in summer '39. This was before I went to Peenemünde.

NEUFELD: I had the impression though that at that time he was still working at Darmstadt and may have been --

HOELZER: No.

NEUFELD: Because he finished his PhD about 1940.

HOELZER: In Peenemünde. In Peenemünde, ja. He came to Peenemünde with a master's degree. And he got his PhD in Peenemünde.

NEUFELD: So that his PhD was finished at Darmstadt in '40 but he was physically located at Peenemünde before you got there certainly.

HOELZER: Steinhoff had a master's degree from Darmstadt, ja. Went to the institut für Segelflug in Darmstadt. Became then a pilot to some airline in east Prussia. Went then to Peenemünde, and this must have been in 1937, because I came in '39, and Steinhoff was there already at least for a year, because in summer of '39, he and von Braun and Steuding met me in Berlin.

NEUFELD: I have to check the facts. I had the impression he was there later.

HOELZER: No. Absolutely not. In summer of '39, shortly before the war broke out, Steuding, von Braun and Steinhoff were in Berlin on the TDY from Peenemünde.

NEUFELD: Yes, that I can believe. The thing is I talked to Reisig and he told me, he had started in '37, and he didn't mention Steinhoff appearing before '39.

HOELZER: He didn't like him.

NEUFELD: I know he didn't like him. That was clear. But he also left me the impression that Steinhoff didn't arrive there until '39.

HOELZER: No. Steinhoff arrived in '37. Let me see, just a moment -- let me check in his book "Weltraumfahrt" (space travel)-you are right, it was early in 1939.

NEUFELD: Okay, so we decided that he must have come in '39 some time, but it was certainly earlier in '39 that he had become, so --

HOELZER: Reisig was in Peenemünde earlier than Steinhoff. That's true.

NEUFELD: Went back to the end of '37.

HOELZER: The only thing which happened was, von Braun made Steinhoff in charge of the works and not Reisig, ja, and this was a bad thing for Reisig. And Steinhoff did an excellent job.

NEUFELD: Right. Reisig's dislike of Steinhoff went all the way back to the beginning.

HOELZER: All the way back to the beginning, ja.

NEUFELD: Yes, because obviously when I talked to him--

HOELZER:--Steinhoff got the job and Reisig didn't. That was the reason why.

NEUFELD: So there was a concentrated effort then somewhere like '38, '39 to build up an in-house research and development capability in guidance control, electronics.

HOELZER: The whole works, for two reasons. It was the same reason why we did this here later on in Huntsville, when we worked for the Army and the Army Ballistic Missile Agency. You know, the industry, no matter whether you talk about the German industry, the American industry or anybody else's industry, the industry is interested to make bucks, to make money, and not so much interested in whether the government spends a little bit more or a little bit less. Okay, so, since Peenemünde was an Army outfit, the same as the Army Ballistic Missile Agency was an Army outfit here, they got burned their fingers a couple of times by unbelievable prices from the industry. Really unbelievable. For instance, let me give you an example here in Huntsville. We tried to introduce computerized milling machines here in Huntsville. Before we did this, we went to some company in Georgia, and asked them to produce, manufacture a certain part. This cost so and so much money. Some container ring or whatever it was. Okay, now, this was a little bit high and we tried to get the company to lower the price, which was impossible. And then in the meantime while this squabble was going on, we had milling machines ourselves, automatic milling machines, and we produced this part here for, oh, I believe one tenth of their price or something like this. And after we could show them that we could do it for this and this price, they came down. Before, it was ten times as much. That's the way the industry works. And in the German industry, they were no angels either. So the idea was in Peenemünde at least to do enough work to keep your fingers dirty, and to know, to always know what you're talking about, and then after you know this and you know exactly what the price should be and how long it should take, then you can go to the industry and tell them, I want to have this and this built or this and this done and so on, and what is your price. Then you can tell them,

no, I don't buy this. Usually the price is too high. That's the way it starts.

NEUFELD: Right. So you certainly felt you had to have an in-house capability, to judge --

HOELZER: --oh yes--

NEUFELD:-- and develop.

HOELZER: This was the original idea in Peenemünde, yes. But there were lots of orders coming from Dornberger. Dornberger was the military commander like Medaris here.

NEUFELD: Yes, certainly, I know him well.

HOELZER: Okay. Dornberger's instructions always were, don't do it yourself, give it to the industry. But know what you're talking about. You see the conflict there.

NEUFELD: Yes. I got the impression though that Dornberger in some ways was probably the leading person in developing a large army facility as opposed to contracting everything out.

HOELZER: No. Not so. No, no. Only testing and this sort of business, and part of the original manufacturing was done at Peenemünde, same way as it was here. And after this everything was farmed out, to Siemens, to Kreiselgeraete, to Anschuetz, to Askania, in the guidance and control field, to the Zeppelin outfit, where was this, in Friedrichshafen, for certain portions. The fuselage was farmed out into the industry. This was all farmed out.

NEUFELD: Yes, but most of that came later. Except for guidance and control where there was no experience, so that, I mean, almost entirely all engines and fuselages and everything were built in-house until '41, '42, '43, almost all parts except guidance. That was my impression. Maybe turbo pumps. Were built mostly in house, and --

HOELZER:--and guidance too--

NEUFELD:--and Dornberger was the first to push the idea of building even the large production plant, Versuchsserienwerk, there. In Peenemünde. It was his idea, according to Rudolph. I interviewed Mr. Rudolph about that.

HOELZER: Peenemünde was later on transformed into the Electromechanische Werke. This was a private outfit.

NEUFELD: Well, it was government owned.

HOELZER: A Goco. A government owned company.

NEUFELD: But that was definitely not Dornberger's idea. That was against Dornberger's ideas.

HOELZER: I don't know. I don't know. I remember, very often I got told, don't do everything yourself, get industry in wherever you can. In a lot of instances we just couldn't buy, or we didn't know what to buy. This is why we did lots of things at home. For instance, how to buy a simulator? There was no analogue computer available in industry. So we had to do this ourselves. And I played with this thing. Von Braun said, "This is no good, stop playing with electronic toys." This was the end of the business. Even the Mischgeraet, the computer, the onboard computer was stopped by Dornberger. Ja. At a meeting at Siemens, ja, this was stopped, ja. The following, we come now right in the middle of this business. Do you still have enough tape left?

NEUFELD: I'm getting very close to the end of this side, but there's more.

HOELZER: Okay, fine. Then let's go now to the A-4, to the A-4 guidance and control development. While I worked on the final portion of the A-5 guide beam system for the A-5, von Braun one day came in or--let me tell one thing in between, how this was organized. I guess you have to know this too. In charge of BSM was Steinhoff. There were different Abteilungen (departments). One was the measuring department, Reisig. The other one, ja, Reisig was measuring plus the Entfernung, the distance control. There was wiring, cable nets and so on, under Professor Wierer. A fitting name for a wiring -- yes, Professor Wierer. This gave some confusion later on after the war when the American interrogators asked him, "What's your name?" He said, "Wierer." Or, wirer, "Okay. What did you do? " "Wiring." No, your name." Okay --

TAPE 2, SIDE 1

NEUFELD: So as far as you were concerned, in terms of building electronic things in-house and guidance and control in-house, you were receiving orders from the army bureaucracy and Dornberger to buy as far as possible outside.

HOELZER: Orders from von Braun himself.

NEUFELD: And from von Braun too. So from your perspective, this buildup in in-house capability was just more or less forced by the fact that you had no place else to go to get things, right?

HOELZER: In certain areas this was the case, yes. But we were under a kind of pressure from the industry. At least very early,

ja. Later on, I think it was complicated to get something from industry because lots of it was destroyed already, and this was one of the reasons why they continued to do this in-house. But this was much later. And let me tell you one more thing. One of the reasons to do the manufacturing of the V-2, so to speak, in-house, was first, in-house in Peenemünde, and then came the air attacks, and then came the order, everything underground. And then they moved the manufacturing outfit, Rudolph, to the Mittelwerke in middle Germany, where they manufactured jet motors for jet machines, for airplanes, and they put him there, including a handful of engineers. Everybody else couldn't move. So this was then done, because those underground facilities were owned by the government, or better, the government in Germany was a complicated affair; they were owned by the SS, by Mr. Himmler, and this is where they moved Rudolph.

NEUFELD: Yes, well, there's a lot of --actually the underground facilities were owned by somebody else, but anyway, the SS came in. It was owned by something called the Wirtschaftliche Forschungsgesellschaft which was a company that owned underground storage locations for fuel and other things, and they created this Mittelwerk company, and obviously the SS was brought in as guarding prisoners and as kind of a cooperation.

HOELZER: It was more complicated. I tell you a story about this too. Matter of fact, I have a book about it.

NEUFELD: Do you have the "Geheimprojekt Mittelbau" by Bornemann?

HOELZER: No, the book I have by Speer.

NEUFELD: Oh, Speer's book, yes. I can find Speer's book.

HOELZER: Well, Okay, fine. I'll give this to you later, the name is "Infiltration".

NEUFELD: The reason why I mentioned Dornberger is because you know, he launched that Versuchsserienwerk or Fertigungsstelle Peenemünde, as obvious to me that he was concerned about building up a big operation inside Peenemünde. But it's interesting if you see from the inside it looked different, in the sense of maybe he saw limits to the whole thing and said, as you were saying, he was getting pressure from industry.

HOELZER: Yes, we got pressure from industry very early, and later on the pressure from the industry went down a little bit because the industry was to a large extent destroyed. So he had no other choice than to do it himself.

NEUFELD: I had the impression though that when Speer and those people came in, they were pressuring him to transfer much of the

operations over to either private companies or government owned companies, as opposed to doing it inside the army.

HOELZER: This happened also. There were two, well, let's talk about this later when we come to it, huh?

NEUFELD: Right. Okay. So I sort of sidetracked you from the guidance and control system.

HOELZER: Right, because we jumped to 1945 or '44 or so, and quite a few number of things happened in between.

NEUFELD: So I had turned you off. You wanted to go back now to talking about where you were from A-5 to A-4.

HOELZER: Okay. Let me show you how it was organized. There was Reisig. There was the guide beam outfit, the guidance outfit, that was Hoelzer. There was Wierer. There was --I have an organization plan I can try to find later on. Maybe I still have it. Okay, anyway, and in charge of the whole business was Steinhoff, who wrote this book here, and if you want to have this you can have it if you promise to send it back.

NEUFELD: I can get the reference. I have the name. So then this was BSM as it was called most of the time, at least until later.

HOELZER: Okay. All right. Let me go back to, from A-5 to A-4. In the meantime the A-4 was now ready. This was end of, where are we?

NEUFELD: '41 maybe? '40, '41? You said that A-5 was, first version was the end of '40.

HOELZER: End of '40, yes. Okay, we are now end of '40. And end of '40 the following thing happened. We were working in the laboratory and von Braun came in. He was quite excited. Von Braun told me, "Say, I have a question. You need for the guidance system this lateral velocity and the integral of the position," they call this Isodrome, who invented this expression I don't know. I don't even know where the word came from, Isodrome. It's equal to integral of position, better write it down. ...Okay, that's the integral of the position then, no matter whether you talk about the control system or about the guidance system, the Isodrome is obviously an integral. All right, so you need the lateral velocity and how do you measure the lateral velocity? You can't measure the lateral velocity. And could you also measure the acceleration? I said, "We can do this too but we don't need it." He said, "Okay, here's the story. Siemens and Askania plus Kreiselgeraete plus Anscheutz came all and told me, for the A-4 their control systems don't work. They need so many millions more marks and so many months more time, and both we don't have. Can

you use your method to obtain the angular acceleration, which they cannot measure?" I said, "We cannot measure this either. We compute it." "How do you do this?" I said, "With the thing you have forbidden me to do a little while ago." All right, then he said, "Well, can you do this? Can you do the same thing you did for the guide plane system? Can you also do this for the gyro control system?" I said, "Well, maybe, I don't know. We have to try to find this out." And he said, "How long would this take you, how many months?" I said, "Well, not months, it's now 10 o'clock a.m , why don't you come back at 5 p.m." He said "Ha ha ha" and pointed at his head and said, "You're nuts." I said, "Okay, see you at 5." He left. All we did was, we didn't do any theoretical computations, we just experimented along on the simulator we had in the meantime, and tried to simulate this thing.

NEUFELD: Okay, you have to explain that. What simulator had you developed?

HOELZER: Okay, we had now analogue components to do computations with. This means addition, subtraction, multiplication, division came later, but I mention this too, integration, differentiation. With those things we could solve differential equations. Okay. Now, to simulate the behavior of a physical system, is to set up its differential equations, and replace then the mathematical operations in this equation by hardware, so that you have now not only a differential equation on paper, but the differential equation in hardware, and no wonder this hardware behaves as the original system. So in other words, it simulates it. And this can be done electronically very nicely if you have the components. Those things we call nowadays analogue computers. At that time we called them electronic models.

NEUFELD: So with the simulator you would have created the apparent control signals--

HOELZER: --right--

NEUFELD: --of a missile --

HOELZER: --ja. We had a little tilting table, which carried the gyro, that the gyro was mounted on. We needed this table anyway for the simulation of the guidance system. And we had the rate gyros also on this little platform, and well, the rest of it was electronic simulation. And all we did now was, we took off the rate gyros of this platform and replaced them by condensors and an amplifier, one of those AC amplifiers I told you about. Okay, and then we used our thump and made a fast thump approach, later on to this business, and we found out that with certain adjustments, the thing was stable, could be stabilized easily, no matter what company furnished the gyro, who furnished the servo

motor, and so on. This could be a servo motor from Siemens and a stabilized platform from Kreiselgeraete. Or some servo motor from Anschuetz and somebody else's gyro. So all this we had plucked then into this simulator, and the simulator then computed what was necessary, and we could show von Braun at 5 o'clock that the business works.

NEUFELD: Let me ask you one detail question. The simulator assumed three gyros for position? Or two?

HOELZER: This is only a stimulation for one axis.

NEUFELD: So you had one gyro for one axis, no accelerometers.

HOELZER: No accelerometers.

NEUFELD: So it's a demonstration then that in one axis, you could compute the correct control values.

HOELZER: Right.

NEUFELD: Through, you integrate the control values and --

HOELZER:--instead of measuring-

NEUFELD:--and differentiate the, and use electronic differentiation instead of rate gyros.

HOELZER: Right. Instead of measuring the angular velocity you computed it, including the angular acceleration, because this was the problem the industry had. They had no accelerometers that could measure the angular acceleration. See, there's a difference between straightforward level acceleration, and angular acceleration. You shouldn't mix them up. And this was one of the problems, and this was why they needed more time and more money. Okay, and we could do away with this whole business by computing those values. Okay, at 5 o'clock, no von Braun. We finally found him in the Officers Club having dinner or a drink or something, and we told him, "Here, this works." And this was the end of all of the industry control systems. The industry furnished the gyros and the servo motors. The servo motors which tilted the vanes. Everything else was replaced by a computer, the mixing computer, Mischgeraet. A Tarnname.

NEUFELD: Okay, so Mischgeraet was a cover name.

HOELZER: Mischgeraet was a cover name for this onboard computer. Okay, now later on, after the war, Siemens came out with a control system for airplanes, and they used now also a mixing "computer", but the computer didn't compute anything. They used rate gyros and called this thing a Mischgeraet. And it really was

only a mixing device, namely, to mix the gyros' signals and feed them into the servo motor.

NEUFELD: The position.

HOELZER: Yes, the position and the rate.

NEUFELD: The position and the rate.

HOELZER: This was after the war. Later. This was much later.

NEUFELD: And considerably behind what you had accomplished, behind what you had already accomplished.

HOELZER: Oh, sure. This was --ours was during the war. I mention this for the following reason. Dr. Petzold whom you know wrote me a letter one day and said he found some documents where Siemens also had a Mischgeraet. And I found out that this was something after the war. It was however no computer. Okay, fine.

NEUFELD: Let me just show you this as a contrast. This is a diagram I got from Dr. Mueller showing a sort of electromechanical A-4 system that they were talking about, as an alternative, so this is what they were working on. It's a rather complicated thing, with rings, these rings are around the bottom of the nozzle, and a mechanical device --

HOELZER: Right. And this thing was unstable. This worked because of friction in these rods here, and Steuding claimed it was not stable, and after they greased everything, it was unstable. It was later on improved for the A-5. So then it worked, they put some damping in or something.

NEUFELD: Apparently this is actually the version for A-4. There were only three examples built or something. That's what he said.

HOELZER: For the A-4?

NEUFELD: This was an A-4 system that obviously never entered production.

HOELZER: This never entered production because --

NEUFELD:--because they had yours.

HOELZER: We could do that much cheaper. See, a rate gyro cost about a couple of thousand dollars, and a condensor cost two dollars fifty. And there was quite a difference there, in this business.

NEUFELD: So that the electromechanical systems that

Kreiselgeraete had, and I assume others had been playing with the same idea, were very expensive and didn't work very well.

HOELZER: Very expensive and didn't work very well, for one reason. See, the motor, when the rocket motor burns, this creates all kinds of heavy vibrations of all frequencies, and some of those frequencies are the resonance frequencies of the rate gyros. The rate gyro is a spring mass damping system which has a natural frequency. And when you feed this thing with a mechanical vibration, it starts to follow. So it clogged up its own signal, and therefore we had many failures before, for this reason. From the moment when the rate gyros were thrown out, the A-4 flew.

NEUFELD: Did you launch A-4s then with the rate gyros?

HOELZER: There were some fired with the rate gyros which were failures, for this reason. Okay, now, but this wasn't the end of the story. This was only some demonstration in the laboratory. And von Braun said, "Go ahead and build such a thing." Okay, we did. But in the meantime, the industry got told, no more time, no more money, forget it, you manufacture the gyros and you manufacture the servo motors and wait for the rest. Okay. This leads then to a conference in Berlin at Siemens. And at this conference was Dornberger, von Braun, Steinhoff, I was there, people from Siemens.

NEUFELD: About what time was this? Some time in '41?

HOELZER: Ja, some time in '41, I forget when exactly. It was right in the middle. It was after Siemens got told, you don't get any more money.

NEUFELD: For the mechanical system?

HOELZER: For the development of the angular accelerometer. They needed the accelerometer, the acceleration, for stabilization, not only the angular velocity, they also needed the angular acceleration. But nobody had this except Moeller from Askania.

NEUFELD: Not the same Mueller as Fritz Mueller.

HOELZER: No, Moeller. The other one is Fritz Mueller.

NEUFELD: The pronunciation difference is too subtle for my English ear, but anyway --

HOELZER: Okay, fine. So this was in Berlin, and Siemens wanted to know what happened and why don't they get this contract? Therefore the conference. And now I have to get technical again. I have to tell you something before I do this. Can I have the "rocket" once more?

NEUFELD: Okay. Can I ask you one question before we get to that? That is, by this time it's clear that Siemens had won the contest for the gyro system that was going to be applied in the short run, because that was the main gyro system --

HOELZER: For the gyros yes.

NEUFELD: Because Kreiselgeraete's more accurate but much more complicated platform hadn't, was taking too long, I assume, or something. Is that true? And Kreiselgeraete was still developing their stabilized platform, so Siemens won out with their simple two gyros rocket fixed to the body of the rocket.

HOELZER: Ja, but only the Richtgeber, which are the free gyros to measure the direction. The gyros, the free gyros. No rate gyros.

NEUFELD: Right, just two gyros that gave direction and one for pitch and one for combined roll and yaw.

HOELZER: Right.

NEUFELD: So they just had that system, and they were using the Wolman radio system for cutoff mostly.

HOELZER: For cutoff, ja.

NEUFELD: Okay, so they had cut out the rate gyros because you said, "We can do it better this way."

HOELZER: Yes, we can do it better this way. We have no natural frequency, no resonance, and we have no $2,500 apiece, but $2.50.

NEUFELD: Do you remember when Anschuetz and Askania dropped out?

HOELZER: Same time.

NEUFELD: All together.

HOELZER: At the same time, all together. And they also had the rings. I guess Fritz Mueller told you about that. They all had to use different mounting rings at the Heck.

NEUFELD: At the tail.

HOELZER: At the tail of the thing, and they were all different. There were four different ones, so if you want to ever manufacture this rocket, you had to manufacture four fuselages which had a different tail. This was an impossibility. This was also one of the reasons why we switched over to the analogue technology solution.

NEUFELD: So all of these systems originally proposed had rings around the tail --

HOELZER:--right--

NEUFELD: --to transmit the mechanical force, from some kind of rods or something --

HOELZER:--right--

NEUFELD: Which would then move the vanes.

HOELZER: Ja.

NEUFELD: The four jet vanes.

HOELZER: They were servo motors, hydraulic servo systems which moved the vanes. Okay, now, I come to this conference with Siemens. And before I come to Siemens, before I can make this plausible, I have to say something about the Nyquist method of determining stability. Nyquist was from Bell Laboratories and developed a method to determine the stability of feedback systems. I give you now a lecture about the Nyquist method so that you can understand what happened at the Siemens meeting. This is important. Nyquist's method is general, but it came no doubt from the electronic technology side and most of the control system "specialists" were mechanical engineers. Here is Nyquist's recipe in a nutshell. First an explanation.

A feedback control system consists of a straightforward path from the system input to the system output and a feedback part from the system output back to the system input. I call this system input and system output because later on we also have a Nyquist input and Nyquist output.

Nyquist says: Cut open the feedback line. This creates now two new terminals which I call the Nyquist input and the Nyquist output. Connect a sine wave generator to the Nyquist input and feed this input with sine wave oscillstions from zero Hz to very high frequencies with amplitude "one." Now observe the Nyquist output which is of the same frequency as the input but of different amplitude and phase.

Plot the output in vector form in a coordinate system and obtain a "locus curve" marked with the frequency values. Follow this curve from frequency zero to frequency infinite and see whether the point X = -1 Y = 0 lies to the left or to the right of the curve. The curve divides the plotting plane into a stable part (the left) and into an instable part (the right). The point X = -1, Y = 0 contains all the roots of the system when it is closed up again. The mathematical proof is somewhat complicated and I

leave this out. You will find a new proof in an appendix to my doctor's dissertation. A locus curve is a vector curve, ja. Okay. Now, we don't want to go any further in the mathematics of this thing, because it can get quite complicated. But the most important thing is the phase shift of your signals. What comes out versus what you get in, whether there's a phase advance or delay or whatever. So people who use the Nyquist system to determine stability or instability very often use the word phase, because of the phase of the oscillation you sent in versus what comes out. But this oscillation you sent in is an arbitrary enforced oscillation you got from an oscillation generator, not from the system, and after you get through, you take your frequency generator, your oscillation generator, away, and you close what you have cut open before, and the system is now in its original condition and it's now either stable or unstable, but now you have measured it and you know now what it is. And you don't talk about phase shift any more. The oscillations which were used in our test have nothing to do with the final behavior of the system.

HOELZER: Okay, so now what happens at the conference? The conference, Siemens said, "Well, you want to use condensors and amplifiers and all those things now. How does this work?" Okay. Dornberger said, "Hoelzer, explain it." Okay. I explained how it worked. And then the Siemens guy got up and said, "Mr. Hoelzer," (at that time) "I want to ask you a question. You always speak about phase shift and phase." I said, "Yes." He said, "So you agree that the word phase, to talk about the word phase or to shift the phase in this system makes only sense if you have an oscillation." I said, "Now, wait a minute, now we come to the forced oscillation or the artificial oscillation we needed for measuring purposes." But he cut me short, he said, "You see, this is the problem. You need an oscillation to make your thing work, ja, because otherwise you couldn't shift your phase. And this oscillation is exactly what we want to avoid. So your system will never work." Dornberger said, "All right. Hoelzer quit working." This was the end of the onboard computer. So for Dornberger this was plausible. He said, "Well, if he needs oscillation to shift his phase, and we don't want an oscillation, his system cannot work." Okay, in the mind of a general, this was absolutely logical. And he (Dornberger) said, "Well, out, we go back to Peenemünde, and I want to make now absolutely sure that I understand why things don't work." And Siemens says, "But ours works." Dornberger said, "Okay, come back to Peenemünde, we'll have a series of tests, and then we'll see what works, all right?"

NEUFELD: So you're talking about a static test with a complete rocket.

HOELZER: A complete rocket.

NEUFELD: In gimbals.

HOELZER: And gimbals. The fuel was in, the liquid oxygen was in, everything was in, Siemens control system was in with gyros and rare? gyros and everything. They deflected it. Main stage.

NEUFELD: So it oscillated back and forth.

HOELZER: It oscillated back and forth. It was unstable. Dornberger said, "Out. Next." Okay, here came Anschuetz. Wham-Wham Wham out. "Askania." Same thing. "Kreiselgeraete." Same thing. Dornberger got mad and shouted, "That's enough, I have enough now, and I'm going to close this outfit and you go to the front and learn shooting." So, at that point, I went to Dornberger and said, "General Dornberger, we want to have one more test." "You can have as many tests as you want, you will learn shooting, I'm going to close this outfit." Okay, I said, "Well, thank you, sir, one more test." "Okay." So we went back to the mixing computer, put it in, left what ever gyro happened to be used and which servomotors happened to be mounted because the mixing computer could work with everybody's gyro and everybody's servomotor. So we left in whatever was in. We just disconnected what we did not need and connected what we needed to the the mixing computer, set the right positions on the knobs, pushed the button. They deflected the rocket, and here it was, bop, no more motion.

NEUFELD: Jaws dropped.

HOELZER: Ja, jaws dropped.

NEUFELD: Dornberger was there, he saw that too?

HOELZER: He was there, ja. "What was that?" I told him, "This is what you said we shouldn't do. But we had it before you said we shouldn't do it. We had it already. We didn't do this after you forbid it, we had this before." Okay, this was then the final version of the control system.

NEUFELD: Okay, let me just, so for accuracy purposes here, when these comparative tests were made with the A-4, all these other systems were electromechanical systems?

HOELZER: A hodgepodge of heavy current technology, mechanical technology, about everything you can think of.

NEUFELD: But the integration of, you know, the creation of the control signals by integrating, was it rate gyros, were they integrating rate gyros and regular position gyros?

HOELZER: No, this was only differentiating.

NEUFELD: So you had electronic differentiation.

HOELZER: Right. Twice. Twice. Position and --

NEUFELD: In place of the rate gyros, for the different gyroscopes.

HOELZER: Plus the signal was then differentiated once more, to compute the acceleration, the angular acceleration. Instead of an accelerometer. So this was twice computed, ja.

NEUFELD: So that they were using partially electronic systems that you had developed, but not the full --

HOELZER: Well, the measuring of the position must be done mechanically with a gyro.

NEUFELD: These competing systems that failed had no rate gyros, right? Is that true?

HOELZER: The competing systems, they had rate gyros.

NEUFELD: Okay, so all the ones, these examples that failed--

HOELZER: --had rate gyros.

NEUFELD: They all had rate gyros.

HOELZER: Ja.

NEUFELD: And other gyros.

HOELZER: And the controls system we put in had no rate gyros. But it also had one more component which computed the acceleration, the angular acceleration also, which the others didn't have, and this was what made them instable.

NEUFELD: And the control signals for the --on the competing systems had all failed.

HOELZER: Ja.

NEUFELD: They had servo motors for the vanes that were powered by mechanical systems, like these rings, or?

HOELZER: Mechanical, ja. Or hydraulic systems like Siemens. Or, well, the others were hydraulic, the three others were all hydraulic.

NEUFELD: Okay. And this was some time in '41?

HOELZER: This was beginning of '41. Beginning or middle of '41.

NEUFELD: Probably more like middle, because it would have taken some months to get all this together. Probably at some point documents will turn up which show the meeting, what happened or something.

HOELZER: Okay, it was around this time, ja. Into this mixing computer naturally was also fed the guide plane signal. I have this here in the diagram, which --what finally came then out was the mixing computer, and you find the diagram in Dr. Tomayko's article, the one I explained to you, at the end of Dr. Tomayko's article in, where was this, published THE ANNALS OF THE HISTORY OF COMPUTATION.

NEUFELD: COMPUTING, yes.

HOELZER: COMPUTING. Okay, so this is the way this whole business worked now. Now comes --the manufacturing phase, which was done by Lorenz Co.

NEUFELD: It combined with the guide beam?

HOELZER: With the guide plane system. Lorenz had two contracts. One was the manufacturing of the guide beam system, and the other the production of the Mischgeraet (the mixing computer).

NEUFELD: I have a question that comes out of this. Did you ever combine the guide beam and Siemens gyro system? Because the Siemens gyro system does not give you lateral deviation from the trajectory dispersion from the trajectory. It only gave the attitude control, essentially.

HOELZER: Okay. Let me have this thing back from Tomayko and I show you what this is. Here is the combination. It is always a combination, because -- where are we? Here. See?

NEUFELD: This is, which page is this, 231?

HOELZER: This is page number 231.

NEUFELD: Of Tomayko.

HOELZER: Picture I, there's Roman I, there's 19, diagram of the three axis control system. Okay. This here is Siemens, including the potentiometer.

NEUFELD: That gives inputs from two gyros --

HOELZER: Two gyros, ja, with three potentiometers.

NEUFELD: Potentiometers, right, so you're using one gyro for two axes.

HOELZER: Right. This is as far as they went. Then came the mixing computer, and then came Siemens again, the rudder machine, this one, this one, this one and this one.

NEUFELD: Right, four vanes.

HOELZER: And Lorenz went in here.

NEUFELD: So that you can feed the guide plane, you could feed the guide plane value into --

HOELZER: --ja, through here.

NEUFELD: --into the, that would be a yaw axis.

HOELZER: Yes, right.

NEUFELD: Right, the guide beam would give you a yaw value, in order to steer you back to the center of the trajectory, as well as keeping the attitude straight. But I gather that in practice--well, how much did you use the guide beam with combination with the Mischgeraet for the actual production missiles? I gather, only part of the time.

HOELZER: No, this was always in. This transformer was always in. And all you had to do was plug the guidance signal in. The diagram of this thing, you find in my letter to Mr. Winter. Ja.

NEUFELD: Of the guide beam. All I'm saying is that many missiles flew without the guide beam.

HOELZER: Ja, most of them.

NEUFELD: But you could put it into this system, merely by including the guide beam equipment inside the Geraeteraum, inside the nose of the missile, put it into the system.

HOELZER: Right, plug it into the mixing computer.

NEUFELD: Plug it into the mixing computer.

HOELZER: That's all you had to do. Just put in a plug. If you have --this little publication here can show you what this looked like. A letter to Mr. Winter, here. Let me see. It should be in here some place. Well, it doesn't specify it, it just says "input to mixing computer." That's all it says here.

NEUFELD: Figure 3 on the --

HOELZER: Differentiating and integrating, ja.

NEUFELD: So that, what was the reason why it was not incorporated in all vehicles? Was it fear of jamming? Was it problems with getting it into production?

HOELZER: Yes, it was, ja, the production, the main thing. See, you needed, for this business, you needed a ground station, with ground antennas. They had to be surveyed very exactly to produce this tight beam very exactly. And this was a little bit complicated, because you couldn't show up in an open field without being shot at. It was done in the northern portion of France, and as soon as some guy with a theodolite showed up, he was shot at. So to speak. And this was one of the complications. And on the other hand, the production, the mass production was not far enough advanced.

NEUFELD: This is the mass production of the equipment inside the missile, or the mass production of the ground antenna?

HOELZER: Both. Both.

NEUFELD: I gather that you ran into severe shortages in the electronics capacity of German industry.

HOELZER: Ja. We used a certain tube? here, EF 14 number I remember, and the EF 14 was not available any more all of a sudden. And then, since this was needed, Dornberger got his industrial outfit going and grabbed them out of the warehouses, to make sure that they were available. Okay, now what comes next?

TAPE 2, SIDE 2

HOELZER: All right, here's the little article back.

NEUFELD: Okay.

HOELZER: We come now to the next thing in Peenemünde, and this was the simulator. This simulator consisted of components which were necessary for the mixing computer. Once you can differentiate, integrate, multiply, add and subtract, all you need to have a complete analogue computer is, you also have to be able to divide, and you have to be able to extract square roots or higher roots and you have to be able to produce functions of functions, and that's about it. So with the exception of the last three I mentioned, division, square root, cube root and so on, and functions of functions, we had everything already. Now, to divide was done very easily. If you remember, if you take a first order linear differential equation, the normal first order linear differential equation has a steady state solution which is the quotient --well, it's the quotient of

two coefficients of the equation. This is hard to explain without diagrams, but you find this in either Dr. Tomayko's article or in a little write-up of a little speech I gave a couple of places where I explained this or in my dissertation. This is hard to do without pencil and paper. Okay, anyway, we could do all those operations in our analogue computer at that time. We just couldn't call it analogue computer because the word analogue computer was not invented yet. We called it electronic modeling. To create an electronic model of a differential equation or of some system whose differential equation you know and whose solution you want to have. Okay, now, what happened further was this. We computed with this, I call it now analogue computer, with this analogue computer we computed now transients of how the missile behaved in the guide plane field. Well, we simulated everything, which was no problem whatsoever. And we had the computer trajectories. The computation of trajectories using desk computers and pencil and paper, Rung-Kutta. methods and so on, took at that time, oh, a couple of weeks, sometimes months to compute step by step. And we had a contract with the technical university in Darmstadt, with Professor Walther's Institute for Practical Mathematics which I knew from my time as a student at Darmstadt, and Professor Walther, and one of his assistants, Dr. Thiel, who is now vice president at Ramo-Wooldridge, or was until he retired. He worked under --

NEUFELD: --this is Adolph Thiel.

HOELZER: Adolph Thiel.

NEUFELD: Not, obviously, Walter Thiel.

HOELZER: Not Walter Thiel, this is Adolph Thiel, and Adolph Thiel and Walther and others came up to Peenemünde to present their latest computations. Okay. Now, I knew Alwin Walther personally very well, and I could dare to make a couple of nasty remarks occasionally. So I asked him, "Professor Walther, are you really sure you didn't make a mistake in computing?" And he said, "Well, why don't you just repeat the computations? It took us only a couple of weeks to do it, and then you will find out." I said, "Well, we can do this faster. Where are the differential equations? And let's see what comes out." And so I took the differential equations, went back to the laboratory, and two of my assistants in the laboratory then wired the equations in the computer, and sixty seconds later, this is as long as trajectory did, the computer computed in real time. ... Okay, so after a couple of more minutes, the output was plotted. I took this result which was still wet from photographic developing and went up to the conference room and rolled it up on the table, and Alwin Walther's jaw dropped down. "How did you do that?" Okay, so I told him, "I am not at liberty to tell you. Why don't you go to General Zanssen, he sits there, and ask him?" He was Dornberger's

successor in Peenemünde. Naturally Alwin found out what this all was about, and Alwin was mighty fast in thinking, and he said, "Mr. Hoelzer, isn't this a wonderful doctor's dissertation for you?" He wanted to get this done "under the supervision of Alwin Walther", you know. Okay, I said, "Yes. Maybe that's a good idea." So I wrote this whole business up, made some more experiments, solved some more complicated differential equations, about that kind of thing, about statics, about loaded beams, and other things in other areas of technology. I wrote this all up and I was writing the numbers of the pages on the pages at night, 12 o'clock, when the attack came on Peenemünde, the air attack.

NEUFELD: The first air raid of August 17/18, 1943.

HOELZER: Yes, 1943, the whole thing burned up, and this was the end of my dissertation, period. The house burned down too and there was nothing left. All right, so Alwin said, "Well, do it again." And I tried to do this again, until end of '45, I had completed the second version. Now, this second version was a hodge podge of control system development and computer development. It was more chronological than according to the technology here. So it contained rocket development and computer development, and the next thing which happened was that the American Army got hold of the thing, after the collapse of Germany. So they had this thing now, and I didn't have it any more.

NEUFELD: What's "this thing" you're talking about?

HOELZER: My dissertation. The second version of my dissertation. Okay, so this was then in Garmisch-Partenkirchen in the meantime. You know, the whole group moved down to Garmisch, and there were Colonel O'Hara I believe was his name and some others, and I talked sweet to them, and he gave the paper back to me. He said, "Okay, here's your dissertation." All right now, after we were through in Garmisch, and after Operation Backfire was over, Backfire was the British operation in Altenwalde there, after this I went to the University in Darmstadt and presented my dissertation, in 1945, fall, '45. Okay. Now, I got told, "We have now a commissioner for the university. This is an American captain. And you have to go to him, present this to him first, because without his say-so you can't do a thing here." All right, I went to him. He was in the railway station office building, had his office upstairs. Downstairs at the entrance was an MP. I went in and said, "I want to see Captain so and so," I forget his name, and "I come from the university." Okay, fine, I ended up in his office. And he said, "So you want to get your doctor's degree?" I said, "Yes, sir." "Okay, now, where's your dissertation?" "Here's the dissertation." He looked at the dissertation, thumbed it through slowly, and said, "This is about

rocket development, is it?" I said, "Yes, and control systems and computers and so on." He said, "Young man, I want to tell you one thing. You know, this was the last war, and we don't need any rockets any more, and as long as I have to say something on this university, and this will be a long time, there will be no dissertation about any weapons system development whatsoever, rockets or no rockets, and that's final." I said, "Well, now, wait a minute, rockets can be used for transportation of mail and for space flight." He said, "For what?" I said, "For space flight." He said, "Brother, you have lots of phantasy!" Anyway, I was out. "Get out." All right, so I went out, I slammed the door, ja, I was mad too, and then I went back in and said, "Excuse me for slamming the door." And then I really slammed it. And when I came down, the MP said, "He wants you to come back up." I said, "Oh boy, now I blew it!" Okay, I went back up. And he said, "You said something about a contract you had." I said, "Yes, here it is. Here's the contract with your government about rockets." He said, "Lemme have this." I said, "No, I'm not at liberty to give this to anybody, not even to you. You can look at it from there. You see, this is my signature, this is the signature of the representative of the Army, here, and that's as far as it goes, good-bye. And I go now to G-2, to Wiesbaden, and complain about you." Okay, so I went to Wiesbaden, to G-2. They talked to the guy, and when I came back, he told me--now, I came first back to Walther, Professor Walther and told him the story, and Walther said, "Well, I tell you what. You do this over again." I said, "No, I'm not going to do this over again." He said, "Why not? All you have to do is make two volumes out of it. One is the computer development, and number 2 is the application of the computer development to the development of guidance and control systems and rockets, and when we finally have your examination, I put number 2 under the table, and after he left, I pull it back out." I said, "All right," so I took this whole business apart again, made one computer development volume, the other volume was the application to rocket development, and we showed this first volume to him and he Okay'd this, he said, with a little bit of help from G-2 and Wiesbaden and finally, in February 1946, he Okay'd it. Finally came then the examination. I had to talk about what this was about, about the new proof for the Nyquist system, about control and conformal mappings, about everybody and his brother and what else you can do with computers, and about it's influence on statics and structures and what have you, and finally he said, "Okay. I have to leave you now. It's all right with me." And then after he had left Alwin said, "Okay, let's go to the next one." Okay, then this was, the application to control systems and guidance systems. So I had to do this whole business all over again from different angles. This was in February 1946, and in May '46 I finally was put on a ship and sent to the United States, first to some fort on the Potomac. I never found out which one. They didn't tell us. On letters we sent home to Germany, everything was cut out. We said, "it must be close to

Washington, it was close to, " and then came a hole in the paper. "There is a river which is called" ... blank, The name of the fort I never knew. Okay, later on I came then to Fort Bliss and to White Sands, where we fired the remaining V-2's the Army brought over, and well, you know the story from there on. Originally we were supposed to stay only for about half a year or maybe one year to fire those missiles. And after this half a year was over, there was the Korean War, and people from Washington who had seen the tests and the firings at White Sands came and said, "Can't you build a missile we can use in Korea?" "Sure we can." Okay, so we need a place, and then they discovered two arsenals in Huntsville, closed, reduced for quick sale by the Army. This was Redstone Arsenal and Huntsville Arsenal. And well, you know the rest of the business.

NEUFELD: Yes. Now --

HOELZER: Okay, so what's the last thing we have on the table?

NEUFELD: Okay, you gave a summary of the simulator development which sort of became the fullfledged analogue computer, when you added, when you could do other functions, development. And then of course you talked about how that developed through your dissertation. It sounds like it was really a tortured problem, to get that dissertation out.

HOELZER: It really was, yes.

NEUFELD: I've heard many problems, and of course I've been through one myself, but I never heard of one that had as many problems as that, including being bombed out in a raid.

HOELZER: Yes, this was a little bit complicated, yes. Well, when we finally had the final examination, this was in one of the leftover classrooms in Darmstadt, with one big corner blue sky, because there fell a bomb in. And finally when I was through with the whole business, the professors could not go back into the conference room to talk about it. They had to stay there because the conference room was utterly destroyed, and they had to tell me to get out. So they asked me, "Would you leave us now and sit on the stairs outside for a little while?" So they talked about things, and I was sitting in the dust on the stairs. This was the situation in 1945.

NEUFELD: Yes. Now, as far as Peenemünde was concerned, to backtrack and try to reconstruct where we were here, so that as I recall we were saying, some time in '41 probably, your system, the Mischgeraet was accepted as the only system that was going to be done for the combining of the control signals from various aspects.

HOELZER: Yes.

NEUFELD: You beat all of these other electromechanical, or hydraulic alternatives. And that decision was made some time, you figure, in '41, for A-4. And then you were involved in the development of the analogue computer for simulation. Was that your main task after that point? Or was that just one thing that was going on on the side?

HOELZER: We had other things to do. For instance, Messina 2. This was a remote measuring telemetry.

NEUFELD: Was it a radar system?

HOELZER: No. It was the following system. We had Messina 1. This was the one Dr. Reisig told you about, probably, because he was in charge of this. Now, from my side came another suggestion. I wanted to, not to have the various measuring values modulated on certain frequencies. I wanted to do this digitally, so to speak. One value, switch over to the next value, to the next value, and do this very fast, the correct word is multiplexing.

NEUFELD: He gave me the impression that he also had the commutation on the first telemetry system as well, that is --

HOELZER: I don't know of any.

NEUFELD: That is, that you would sample different values. Perhaps you did it in a different way.

HOELZER: This may be, but it was not the circular sampling, from 1, 2, 3, 4, 5 until 12 or whatever and then start over again from 1. This probably was done because he didn't have enough channels. But ours was done with one channel only.

NEUFELD: One channel.

HOELZER: Yes, and was done then by sampling. And the sampling, the switching was done by an electronic tube, 12 electrodes soldered in through the glass, and the electronic beam went around in a circle and touched peg 1, peg 2, peg 3 and so on, this was the switching. Okay, and the guy who had the main work in development was Dr. Otto Mueller, from my group, and it was called Messina 2. But Messina 2 I believe also was coming too late to be used later on. It was used a couple of times.

NEUFELD: On test launches of A-4's?

HOELZER: Yes, something like this, yes.

NEUFELD: Late, at the end of the war?

HOELZER: Late, very late, end of the war. The late firings might have had Messina 2 in. I don't recall exactly how many we used there. Again, it couldn't have been many, because I remember we had only a couple of them in the first place.

NEUFELD: So this development, this second telemetry system, did that come largely after Reisig was gone? I know that he told me he had a blow-up with Steinhoff early '43 and was gone after that, and later came back but he was on Dornberger's staff mostly later on, so this is mostly '43 and later that that telemetry system?

HOELZER: Probably, yes.

NEUFELD: Was it delayed because of low priority or pressing demands from other issues?

HOELZER: It was just started too late. It was started too late, a couple of years too late. You know, if you want to expedite a development, and have it two years earlier the best way to do this is to start two years earlier. And this wasn't done there.

NEUFELD: So that it only started in '43 or something and never really was finished?

HOELZER: Around this time. It was finished and it worked. But in general it was a little bit late.

NEUFELD: Was this because the existing telemetry system didn't have enough channels?

HOELZER: It didn't have enough channels and not enough capability, and it wasn't accurate enough. It needed something more accurate in the meantime. This is what Messina 2 was supposed to do. And as far as I remember, it worked.

NEUFELD: Did you notice delays as a result of that, because of problems in the electronics industry, not being able to produce your components on time?

HOELZER: Not directly, because we didn't need many. We only needed a couple of them.

NEUFELD: Just a small little experimental system.

HOELZER: Yes, small thing. It never went into mass production. And neither did the analogue computer. This didn't go into mass production either. We had built a couple of those things, and one of them was brought over to the United States here, but this was the one that was used exclusively for the rocket development, for the guidance development, and not for general purposes. For

instance, the nonlinear functions were left out, and --but they are documented in my dissertation.

NEUFELD: Yes, so that the analogue computer only existed in two or three examples?

HOELZER: Two or three examples, yes.

NEUFELD: And one of them was at Darmstadt with Walther?

HOELZER: No. No.

NEUFELD: He didn't even get one there.

HOELZER: He got some integrators. As it turned out, Alwin Walther told me that he had a contract with some governmeny research outfit in Berlin to produce a machine for the automatic solution of differential equations. But his didn't work, because his integrators were running away, and so this never worked fine, and he asked, can he have some integrators. So we sent a guy, I even remember his name, Mr. Gattermayer, down to Darmstadt with integrators for Walther. And he never came back, because he ran into the air attack on Darmstadt, and this is probably where the integrators went too. What happened to Gattermayer, I haven't found out, whether he survived the attack or whether he just went home, I don't know.

NEUFELD: So the analogue computer, you had a couple of sort of laboratory test models you'd put together.

HOELZER: Yes.

NEUFELD: And they were only at Peenemünde, nowhere else.

HOELZER: Nowhere else, no. Only at Peenemünde.

NEUFELD: Okay, so you were involved in developing that say from '42 onwards approximately, the analogue computer.

HOELZER: Ja, actually, component wise, earlier.

NEUFELD: In the laboratory from 1942 onwards. You were involved in the telemetry system, Messina 2, the second telemetry system from that time on.

HOELZER: Messina 2, yes.

NEUFELD: What other things were keeping you busy?

HOELZER: One more thing was the Schlitt accelerometer and its use for inertial guidance systems. Schlitt finally transferred from

the technical university, from Professor Hueter's outfit, to Peenemünde, joined my department, and his job was to use his integrator, his accelerometer and my integrators to make an inertial system out of this, which he did, and this was mounted, his accelerometer was mounted then on this stabilized platform of Kreiselgeraete, and we had, I don't know how many firings. I remember one which was very successful, and the others, I don't know. There must have been some more.

NEUFELD: Okay, so in that test, how many accelerometers did you put on the platform?

HOELZER: One. This was the one for the lateral guidance, ja, inertial guidance in the lateral, in the pitch, no in the yaw, in the yaw plane.

NEUFELD: Did you use a Mueller integrating accelerometer for the cutoff velocity, or was that a different experiment?

HOELZER: This was different. Dr. Kirschstein was Reisig's successor. Dr. Kirschstein came from the university in Berlin, and he was in charge of the accuracy, hitting accuracy distance-wise.

NEUFELD: Range.

HOELZER: Range, yes. Okay. And he managed the Wolman system, the Kreiselgeraete accelerometer for this range cutoff,

NEUFELD:--cutoff velocity --

HOELZER:--cutoff velocity. And the other one was Professor, what was his name? Karl Wagner, the chemical thing.

NEUFELD: Wagner, what was his first name?

HOELZER: Karl. Karl Wagner.

NEUFELD: Yes, the chemical one, I think there's a description of that in a couple of places.

HOELZER: And Buchhold. Buchhold also came up with a system.

NEUFELD: The chemical one I've heard of, I know that it goes from a deposition of silver on electrode onto another electrode.

HOELZER: Yes, as soon as the silver has been taken off, the thing jumps in voltage. and when it jumps in voltage, this was used to cut off the thrust. This was Wagner. And then there was Buchhold for the same purpose, and what was the other one?

NEUFELD: There's Mueller's.

HOELZER: Yes, those three. They were under Kirchstein's bailiwick. Schlitt's was used for lateral guidance.

NEUFELD: These were all intended essentially to replace the Wolman system, so that you didn't have dependence on the radio?

HOELZER: Yes, four inertial systems, yes.

NEUFELD: How far along? As I understand it, a fully inertial system would have a stabilized platform and three accelerometers for all three axes, an ability to compute both attitude and position relative to the --

HOELZER:--yes, right.

NEUFELD: How close did you come to doing that, in the tests toward the end of the war? Where you had--

HOELZER: We did, we had here the, what's his name, Wagner or Buchhold, I don't know about Mueller. I can't remember that we ever used Mueller's gyro integrator for this purpose. But we used the other two, I know exactly, yes, and we had those two in. We had them on the stabilized platform and we had Schlitt's on the stabilized platform, and this was a complete --

NEUFELD: So as far as you remember, in the last, in 1944, you launched with three accelerometers in three dimensions, three axes?

HOELZER: Yes.

NEUFELD: Full three axis measurement of acceleration. Because I mean, that's not complete --

HOELZER: --no, not --only one axis. Only one axis.

NEUFELD: One axis, lateral acceleration.

HOELZER: No, wait a minute, wait a minute, lateral acceleration, yes, for Schlitt but there were two more accelerators.

NEUFELD: Accelerometers?

HOELZER: One more accelerometer, ja, no, two more, for --

NEUFELD: --tangent to --

HOELZER: --for range, ja. For range.

NEUFELD: --tangent to the trajectory?

HOELZER: Ja. So we had a complete inertial system at that time.

NEUFELD: That was only in test launches at the very end of the war, I gather. Because I know that's basically where SG, these Kreiselgeraete platforms were used, SG 66, SG 70, right in the second half of '44.

HOELZER: Ja. And then the ones without --well, Okay, ja, and then the accelerometer was mounted on the rocket itself, on the frame, for integration, for --

NEUFELD:--yes, I gather that in actual production models that were made, some of them had, instead of the Wolman system, they had the Siemens 2 gyros rocket-fixed and Mueller integrating accelerometer rocket-fixed for final velocity instead of Wolman. He showed me a photograph of the system.

HOELZER: Can be.

NEUFELD: You don't remember that.

HOELZER: I don't remember too much.

NEUFELD: You didn't have anything much to do with the gyroscopic systems themselves. I mean, obviously you had to do with the outputs.

HOELZER: Yes, we had. Yes, but the gyros, the gyros development was so much advanced in industry that we had not much input from Peenemünde. Only specifications, what should be done. But they were available. But development-wise we didn't do much with them. The man who did this was Dr. Rosenthal. He is also in the United States, worked for the Navy in Orlando. And I don't know what happened to him.

NEUFELD: You mean he was the person responsible in your office for dealing with the gyroscopes.

HOELZER: Ja, for dealing with --

NEUFELD: --later in the war.

HOELZER: But there was no development really going on from our side.

NEUFELD: Okay, so you were involved --I'm just trying to picture, you stayed with the BSM or whatever it was called, in differing changeovers, all the way through the end of the war.

HOELZER: All the way through.

NEUFELD: You were entirely focussed on control systems and so forth.

HOELZER: Ja.

NEUFELD: And the telemetry system. Did you do much more with the guide beam system or was that settled?

HOELZER: Yes. There was another development. We talked about so far, about the guide plane system, yes.

NEUFELD: Right.

HOELZER: All right, now, we also had the idea to use a guide beam. And for this guide beam, a king-sized dish should be used, radar style, from Telefunken, which should be movable in pitch, so when the missile went up, it would then follow the motion of the trajectory from behind.

NEUFELD: Right.

HOELZER: And keep it in guide beam. And this thing was also experimentally done but never used in practice, or not in practice in final application. We had one of those transmitters and radar dishes, it was not radar, it was different, it looked like one, in a portion of the country behind the launch site, about 12 miles back, and this then produced this beam and the rocket should go in. The guy who was responsible for the ground station there was Otto Hoberg. He lives up here, up the hill. Hoberg. And the place was Lubmin, where this thing was located.

NEUFELD: Lubmin.

HOELZER: Lubmin. In charge was Otto Hoberg. ...

NEUFELD: Was this near Peenemünde, or are you talking about on the Polish range?

HOELZER: No, Peenemünde.

NEUFELD: Okay, Lubmin.

HOELZER: I never was at the Polish range. It was Lubmin. Okay, and we did some tests, but we never, we never really applied it. There was no receiver built. Well, an experimental model, maybe, but this was all. If it was ever completed. I don't know. It was not test fired either.

NEUFELD: But this would give you, I mean, the first guide beam,

just a guide plane, only gave you lateral deviation, left and right of the trajectory, not above and below the trajectory.

HOELZER: No. This was the idea with the guide beam system, but this was too late. We could have used another couple of years of war.

NEUFELD: So you don't recall ever flying that on a test vehicle --

HOELZER: --no--

NEUFELD: --towards the end of the war?

HOELZER: No.

NEUFELD: Were you involved in Wasserfall?

HOELZER: Yes, I was. I was. I had a deputy with the name, after Otto Mueller?, no before Otto Mueller became my deputy, Lange, Oswald Lange was my deputy, and when the Wasserfall project came along, Oswald Lange was then made in charge from our side, and he moved then with the Wasserfall group away from Peenemünde to Wollin. Wollin is another island. And this is where the Wasserfall group was, and from our side, from the guidance control side, Oswald Lange did this. But Wasserfall came never into use and deployment.

NEUFELD: I gather there were quite a few test launches, though, 20 or 30 or 40 of them.

HOELZER: I remember one of them especially. We had Mr. what was his name? Well, anyway, the photographer, he was there.

NEUFELD: Making a movie.

HOELZER: Filmed the whole thing, took a movie, and the Wasserfall vehicle, it's the same as the Hermes 2, same thing, only this is in centimeters and the other one in inches. Lifted off but the plugs didn't separate, so here came the launch stand and the whole business. Went up, and the photographer stood there, and filmed, and all of a sudden, he looked up and here came this thing and he took off. Okay, this is the one I remember. Wasserfall.

NEUFELD: But you had little or no direct involvement in Wasserfall control.

HOELZER: Well, all problems were solved, practically. We had the components, and to put them together finally was then not too much work, ja. Oswald then took care of this, and I don't know

what companies they worked with. I don't know.

NEUFELD: But in terms of creating a control system for Wasserfall, you saw it basically as a problem of just straightforwardly applying what you had from before, with the Mischgeraet and so forth, and just send it over there, apply it.

HOELZER: Not quite. They had to take the components, the integrators, differentiators, whatever was used, because the Wasserfall had another range. See, this V-2 only needed a couple of degrees here and here. If you had more deviation the thing would go haywire anyway. But the Wasserfall

NEUFELD:--had to maneuver --

HOELZER: And this was different, so the ranges of motion were different. But basically the philosophy was analogue philosophy again. You know, once you have those parts and you have the components, it's not hard to glue something like this together.

NEUFELD: Yes. I gather though that Wasserfall never really solved the other parts of its problems, which were, --

HOELZER:--no--

NEUFELD:--originally they wanted to have a, you know, sort of control based on a radar beam, which would locate the aircraft, and then --

HOELZER: --follow this one and follow this one and finally get them together. This was the idea.

NEUFELD: Right, a guide beam for the missile.

HOELZER: And a guide beam for the airplane, and finally get them together. This never got this far.

NEUFELD: It was too complex?

HOELZER: Too late.

NEUFELD: I'm assuming they ran into the problem of radar development and competition for resources for radar.

HOELZER: Probably, yes.

NEUFELD: But organizationally, did the Wasserfall people, more or less be somewhere else altogether? They were separate?

HOELZER: Yes, they moved to this other portion, to --see, we have

two islands there. One is Usedom that Peenemünde is on, the other one was Wollin, and they moved to Wollin, for space reasons. There was no space in Peenemünde for them.

TAPE 3, SIDE 1

NEUFELD: I just wanted to finish this up. The Wasserfall group was effectively separated from the rest of the group.

HOELZER: Right.

NEUFELD: It was heavily staffed by Luftwaffe.

HOELZER: Yes.

NEUFELD: Officers who had been assigned to it?

HOELZER: Right. See, they had two arrangements. In Peenemünde, the following thing happened. Right in the middle of the war, the German army found out that they should not have drafted all the engineers and scientists to join into the army, because they needed them at home. So there was a guy by the name Osenberg, the Osenberg-Aktion, came to get all those people back out of the troops and back into the industry. And some of them also came to Peenemünde, and they called this the Versuchskommando Nord, VKN. And in the Versuchskommando Nord, these were all engineers, ja. And scientists.

NEUFELD: That was the army unit. I gather there was an equivalent air force unit.

HOELZER: The equivalent thing happened in the air force.

NEUFELD: They had a Flakversuchsstelle.

HOELZER: Right, and they got the air force people to work on the Wasserfall. That was basically it.

NEUFELD: From the standpoint of cooperation, you didn't notice any difficulties from your side in Luftwaffe-Army cooperation?

HOELZER: No. I had no problems there. I had one problem. This was, when one of the officers in the VKN, the Versuchskommando Nord, came into the laboratory and required that the soldiers got up and saluted when he came in. And I put a stop to this business right away, by ordering white laboratory overcoats. Everybody had to wear them, closed up to here, so you couldn't see any more who was what, and when the gentleman came in and required people get up, nobody responded, because, he couldn't do a thing.

NEUFELD: So the VKN people were still dressed in uniforms, up to

I gather some time in '43 or later, they were told that they could take the uniforms off.

HOELZER: Very late. Very late, before they were out of uniform.

NEUFELD: But rank was irrelevant, in the sense that they were assigned jobs on the basis of technical specialties.

HOELZER: Ja. Well, there was also a little bit--you know how this is. You have a PFC, Professor Dr. so and so --and the lieutenant, school dropout so and so, to go to the extreme. And this doesn't work occasionally.

NEUFELD: There was friction that was caused by that.

HOELZER: A little bit, yes. Those things happen too, and this is why I ordered the white laboratory coats. I just stopped it. I had no problems any more afterwards.

NEUFELD: I gather that you had very good relations with Dr. Steinhoff. Did you?

HOELZER: I knew him from Griesheim, and I knew him from Darmstadt. From my time as a student at Darmstadt. Yes. Who else was in Griesheim? Roth. Ludwig Roth, he was in Griesheim. Well, when I came to Peenemünde, I found a whole group from there, I found Roth, I found Steinhoff, I found Steuding, I found, oh --

NEUFELD: Darmstadt was probably the most heavily represented of all the universities.

HOELZER: Ja. Ja. For this reason, because some of us, quite a few of us had graduated in Darmstadt, and then came the Institute for Motorless Flight, which was close to Darmstadt, which also again it ties with Darmstadt. And those guys left the Motorless Flight outfit and went over to Peenemünde. So there were two ties, and consequently, we had lots of ties too.

NEUFELD: They went out essentially through a kind of an old boys' network, essentially went out and said, "We know so and so is out there, we should get him," as in your case, "Let's draft Hoelzer because he would be useful to us in this," or they came to other people and said,"Why don't you come over? We have a good job for you."

HOELZER: Basically it was like this. When Steinhoff, Steuding and von Braun were in Berlin at that time, I told you about them, von Braun was interested. Steinhoff must have said, "I know a guy in Berlin we should look up because he works on those problems." ...Okay, and this was the reason for my drafting, I'm sure.

NEUFELD: So there was kind of a Darmstadt connection which went out, as they brought more people in, there would also be more --I mean, I'm sure, I know you hired a lot of other people from other areas.

HOELZER: Wolman for instance brought in a couple of his assistants in from Dresden.

NEUFELD: Dresden.

HOELZER: Woerdemann, Muehlner.

NEUFELD: What was the first name?

HOELZER: Woerdemann, Hugo Woerdemann. He was one of Wolman's guys.

NEUFELD: And Muehlner, did you say?

HOELZER: Muehlner. He was also from there. And then from the university in Berlin, we had Kirschstein, Lange, a couple more.

NEUFELD: How important would you rate the university research connection at Peenemünde? Not just in terms of bringing people in, but in terms of contracts done by them.

HOELZER: Let me tell you a story. Comes to my mind. There was a, Kulturminister, the secretary of culture, Mr. Rust.

NEUFELD: Yes, the education minister later on.

HOELZER: And as education minister, he owned the universities, so to speak, and he found out that Peenemünde, Dornberger, just bypassed everything and got his contracts with the universities, without going through Berlin and the secretary of --

NEUFELD: Yes, Erziehungministerium.

HOELZER: Erziehungministerium, right. And Mr. Rust finally got this in the wrong channel and came up to Peenemünde and complained, and told Dornberger, "You go through my office."

NEUFELD: About when?

HOELZER: When was this? This was in winter time, I remember, for a special reason, I believe '43. Something like this. Anyway, he showed up, and well, the contracts had been going on for years already, ja, and he came, complained, and Dornberger said, "Well, what do I do with this guy?" And one of us had the bright idea, invite him, roll the red carpet, invite him to the Officers Club and get him drunk. And Dornberger organized this, and this worked

beautifully. Mr. Rust came with another guy or two, his driver, and the very first thing we did was, we were in the Officers Club, and Dornberger got all his young engineers together there and said, "Okay, you know what you have to do, ja." So we--"Secretary, to your health!" Okay. So after around 12 o'clock at night he was stone drunk, went out in the, out of the Officers Club, fell down the stairs in the snow. This is why I said I know exactly it was winter. And from there they picked him up and drove him back to Berlin, and he never came back.

NEUFELD: You didn't have any dealings with the Reichsforschungsrat either. That was the body originally under Rust. Until '42.

HOELZER: I don't know how this business worked.

NEUFELD: Basically you bypassed all of that stuff.

HOELZER: All of it. Dornberger had no respect for anything of that sort.

NEUFELD: Ordnance just, Heereswaffenamt just had direct contracts.

HOELZER: Direct. Direct, ja. And if Dornberger wasn't pleased with the Heereswaffenamt, he did it himself.

NEUFELD: So he was extremely independent of the bureaucracy, army bureaucracy?

HOELZER: Oh yes. For instance, Dornberger--now, this goes a little bit into politics, but it's an amusing story and I think I ought to tell it. You probably have heard it. There was Himmler, you know Himmler?

NEUFELD: Oh, of course.

HOELZER: Himmler, Staatsicherheitsdienst, plus S.S. combined, and Himmler had the bright idea to organize the whole country according to the Russian commissar system. In every industry you should have a commissar with a gun in his hip pocket shooting at everybody who didn't salute or something, ja. Or didn't produce. That's the way the system worked in Russia at that time. Okay, Himmler's idea was to do just this, and he naturally ran head on into Speer, because Speer was in charge of armament, and Speer had the say so in the industry. But Himmler tried to sneak his people in, through this infiltration. And Speer describes this in his book called INFILTRATION. The German edition is DER SKLAVENSTAAT. The slave state, where Himmler tried to make a slave state out of the war business, ja. DER SKLAVENSTAAT. And the American version is called INFILTRATION. And there Speer

describes how Himmler tried to bypass him and to get a hold of the German industry, commissar style. Okay, so Himmler tried not only to get a hold of the German industry but also Peenemünde. But in Peenemünde was Dornberger. And the relations between the old army officers and the SS officers was not too good. So Dornberger had a great distaste of this whole business. First, he didn't like him. Secondly, he was convinced, what they would do, would not work. And secondly it was competition. So Dornberger was in Berlin in charge of the Heereswaffenamt department so and so, and in charge in Peenemünde was at that time Colonel Zanssen, later General Zanssen. Zanssen was a Catholic, and very devout Catholic, and he disliked the treatment of the Catholic Church by the Nazis. And so always when Zanssen was in the Officers Club and had one too many, he started to give his opinion about the Nazi regime, which wasn't too healthy for him. Himmler got a hold of this business, and had some evidence about what Zanssen said and what he did when he was a little bit drunk, and he tried to use this to pick him up and put his own man in. So one day, some SS people, not SS but this time Staatsicherheitsdienst, --

NEUFELD: SD or Gestapo?

HOELZER: Gestapo. For all practical purposes it was the same thing. These people came, tried to pick up Zanssen, because they had this evidence. They came to Zanssen's office, and opened the door, but there was no Zanssen, there was Dornberger. He had found this out through his channels, transferred Zanssen to his place in the Heereswaffenamt, made him his own boss, and he took over Peenemünde.

NEUFELD: I think Zanssen was actually arrested, for a day, because his children, I met his daughters in Hamburg and they told me that he had sat in jail for one day.

HOELZER: This was a little bit later. At this period I'm talking about now --

NEUFELD: --no, he was arrested at this time, right at this time, for this thing.

HOELZER: But he was in Berlin.

NEUFELD: I don't know where he was at that point.

HOELZER: He was in Berlin. When the SS people came to Zanssen's office, there was Dornberger.

NEUFELD: But Zanssen was effectively pushed out of Peenemünde in the long run, because he ended up in a field command, rather than coming back.

HOELZER: But Himmler still didn't get hold of Peenemünde at that time. And now I come to the next point. So Himmler tried again, and this time he got von Braun, because von Braun made the remark, when he was congratulated that the first missile went to London, he said, "Yes, but it landed on the wrong planet," and said more things, and then he got in jail, for saying this. They were derogatory remarks, egotistic remarks, and they put him in jail.

NEUFELD: Actually that occurred months before the first launch against London, because the arrest was in March, 1944.

HOELZER: Okay, I don't exactly --

NEUFELD: I'll tell you what I think was behind it, although certainly they put him in jail officially for saying these things. According to von Braun, he was called to Himmler's headquarters in February, 1944, and Himmler said, "Why don't you come over to us? We've got much more money." And he said, "No, I'm loyal to General Dornberger." It strikes me that, there's no way to prove this, but it seems that he was arrested for the remarks, less for the remarks than as a punishment for not going over to the SS.

HOELZER: Right. Right.

NEUFELD: Or switching loyalties.

HOELZER: Himmler tried desperately to get a hold of Peenemünde. Okay, whatever the reasons and whatever his remarks were, what finally happened to us, that Dornberger went to Hitler and told Hitler, "No von Braun, no A-4." And then Hitler said, "Well, what happened?" "Von Braun's in jail." "Who put him in?" "Himmler." "Get him back out." Okay, Dornberger came with orders to Himmler, get von Braun back out of the jail.

NEUFELD: Well, actually Dornberger didn't see Hitler directly. Speer saw Hitler. Dornberger went to Speer, among other people, after, according to Dornberger's account, he was in the head offices of Gestapo in Berlin and complained to them, and they said, "We've got a file on you too." But eventually Dornberger enlisted Speer and Speer was crucial in talking directly to Hitler.

HOELZER: Okay, maybe it was this way. Anyway, Hitler ordered that he had to get out of jail. And the version I got told, as far as I remember, was now that when von Braun was out, Himmler even apologized. I don't know who told me this business but this is what stuck in my mind, my memory. And Himmler even made him honorary SS officer.

NEUFELD: Well, that actually came long before that time.

HOELZER: This came before this time?

NEUFELD: Yes.

HOELZER: I think this came --

NEUFELD: Von Braun was an SS officer since 1940. It's in the record. He even told the same thing to US Army authorities.

HOELZER: When was this?

NEUFELD: Oh, it's in the security records in various places.

HOELZER: I mean, when?

NEUFELD: He received the rank in 1940. And was promoted a few times, promoted three times, between '40 and '43. It's interesting however that after '43 he was never promoted again, that after this whole arrest and everything, it was obviously --he had an honorary rank in the sense that von Braun did absolutely nothing for the SS.

HOELZER: No, on the contrary.

NEUFELD: He had his name on paper as a member of the SS.

HOELZER: And I remember one thing, von Braun, this was a story which von Braun told in the Officers Club. When Himmler came and said, "I have much more resources, why don't you join our side?" And von Braun said, "Well, first, I can't, and secondly," and now comes the story he told in the Officers Club, he said, "I told him, Mr. Reichsfuehrer, see, the V-2 can be compared with a little plant." Do you know the story? With the cow?

NEUFELD: Yes. With the manure.

HOELZER: Yes, with the manure. "It needs a little bit first fertilizer. It does not need what you want to give, the whole bullshit on it." And through all this business, I deducted that von Braun's standing with SS was not too good.

NEUFELD: Now, I want to ask you a question about those people. I don't know if I ever asked you personality-wise how you felt about Dornberger. You know, what you thought of Dornberger when you met him.

HOELZER: Dornberger was a rocket and space flight enthusiast. When we had the first V-2 which really flew perfectly, out of the atmosphere, and the full range, Dornberger will be on top of the

test stand, not of the test stand, of the building, close to the test stand, and Dornberger said, "Do you know what happened today?" He said, "Today the first Raumschiff flew." Spacecraft flew. This was Dornberger. He joined, the thing was this--you know, after the Treaty of Versailles, Germany was not allowed to produce long range guns. Okay, so the army looked for other means, and Dornberger was the one who suggested to develop rockets, and they grabbed the Raketenverein fuer Raumfahrt in Berlin, in Kummersdorf, to --well, to produce rockets. Dornberger got the money from the army, and fed it into the Raumschiffraketenverein where von Braun still was a student, and Rudolph was there and Max Valier was there.

NEUFELD: Well, they were separate but that's another story.

HOELZER: One that came later.

NEUFELD: I mean, it's a long story, I'd have to interrupt you to tell you, but anyway, Rudolph and Papa Riedel both worked at Heylandt when Valier was there, then they worked separately afterwards, and they actually had nothing to do with the von Braun group.

HOELZER: No.

NEUFELD: And Klaus Riedel, they came together at Kummersdorf later on, yes.

HOELZER: In Kummersdorf Dornberger--you know, this whole business fell apart because they had no money, and Dornberger got people who in the meantime were in industry like Hueter, Hueter in the meantime ended up at Siemens, He got a big offer at Siemens and various others also. And I don't know where Rudolph was at that time. But I guess afterwards, Dornberger brought them back.

NEUFELD: Dornberger as a personality, did he strike you like a Prussian officer type?

HOELZER: Well, I wouldn't say Prussian officer, if you think of the Prussian officer with a monocle and eh, eh, this was not Dornberger.

NEUFELD: So he wasn't quite that type.

HOELZER: No, not even quite. Dornberger was basically--Dornberger was an engineer. He had a master's degree in engineering and an honorary, I believe his doctor's degree was honorary, I don't know exactly, but anyway both degrees were in engineering, and Dornberger was an engineer. Dornberger basically used the army money to build rockets. This is what he wanted to do.

NEUFELD: So for him the first loyalty was the engineering.

HOELZER: In my opinion, yes.

NEUFELD: And not the army, and he was good at keeping army bureaucracy away when he didn't agree with it, Army Ordnance?

HOELZER: No, not all of them. Becker, for instance, was--I don't know too much about this business, but I know that he was friendly with General Becker, and, well, I believe among the Heereswaffenamt he was well liked, because he was successful.

NEUFELD: So by and large he didn't raise too many problems inside Heereswaffenamt.

HOELZER: No. No. He bypassed them a couple of times. And I remember one meeting where the Heereswaffenamt people came to Peenemünde, and they didn't behave the way Dornberger wanted, and he said,after a couple of minutes "Meeting adjourned." And sent them all back to Berlin. He did things like this.

NEUFELD: He had the support I suppose at the top.

HOELZER: Ja. Ja.

NEUFELD: So he could get away with it.

HOELZER: Right.

NEUFELD: From Becker, from his successor Leeb and so forth.

HOELZER: Right. Right.

NEUFELD: And apparently Dornberger also had connection to von Brauchitsch, the commander-in-chief of the army.

HOELZER: Right. Right. My Diensverpflichtung was number 1 and it was signed by von Brauchitsch. I should have saved that thing.

NEUFELD: Zanssen you said, I know Zanssen was a close friend--

HOELZER: --of Dornberger.

NEUFELD: Of Dornberger, yes. Was he careless about speaking out too much, foolish?

HOELZER: Not when he was sober. But when he had a couple or more drinks, then he talked a little bit too much, ja, for their taste.

NEUFELD: Did he drink too much?

HOELZER: I wouldn't say too much, but he was no abstinenzler either. So Zanssen was not as good an engineer as Dornberger was. Dornberger had a good feeling, engineering feeling. Zanssen did not. I remember, once the voltage broke down at the test stand, and Zanssen got a report, voltage was broken down. He took a red pencil and wrote "has to be reinforced immediately." That also happened.

NEUFELD: Did you have any problems with him or did you know people who had problems with Zanssen?

HOELZER: Zanssen? No.

NEUFELD: I get different opinions when I ask this question. Some people liked him, some people didn't like him.

HOELZER: I liked him. He was --otherwise he was pretty harmless, I believe. Not like Dornberger. Dornberger was a fox. For instance, we had a measuring trailer, and in this measuring trailer we had a thing which looked like a typewriter, and recorded test results. And we ordered another typewriter then later on, and there was an order out, "No more typewriters in Peenemünde," signed by Dornberger. And we ordered, we needed another typewriter so we ordered one. It came to Dornberger's desk. He called me in and said, "Didn't you read my order so and so?" I said, "Yes, sir." "How come you ordered a typewriter?" And I said, " General Dornberger, we need a typewriter." Okay, so he chewed me out, sent me back home, next day he showed up in the laboratory, wanted to be shown around, and he came to the trailer, saw this thing which looked like a typewriter, came in and said, "You idiot! I will tell you how to order typewriters. You order this thing here and give the other stock number. That's the way to do business." So I did, I got my typewriter.

NEUFELD: So if there was a way to get around official regulations--

HOELZER: Dornberger wouldn't hesitate. Zanssen, Zanssen I think would.

NEUFELD: More old school, do everything correctly.

HOELZER: Ja.

NEUFELD: Under Zanssen before he left was Stegmaier. Do you remember him?

HOELZER: Stegmaier, ja. In my last days. Major Stegmaier, one officer, I don't think he had engineering background.

NEUFELD: He was lieutenant colonel, maybe later on. You don't

remember much about him at all?

HOELZER: Oh yes, I remember. He was peaceful and he didn't impress me very much. He was harmless. He was a lieutenant colonel, signed contracts.

NEUFELD: He was just an administrator as far as you were concerned.

HOELZER: An administator, yes.

NEUFELD: Do you remember his first name? I keep asking people this. Nobody remembers it.

HOELZER: No.

NEUFELD: Nobody remembers. So I can't even get that piece of information. Did he have any reputation of being a real Nazi? This is an accusation made in a book.

HOELZER: No, I couldn't say he was. But I know one thing. Dornberger for sure was not. And von Braun wasn't either. Dornberger was not.

NEUFELD: The reason why I asked about Stegmaier is, I know for a fact from documents that Stegmaier had high connections in the SS.

HOELZER: This could have been but I wasn't aware of this. I don't know. I remember him as being a harmless bureaucrat, more or less.

NEUFELD: Then there was one final ordnance person. After Dornberger was sort of moved out of the organization, became General zur besonderen Verwendung and had this independent position, Zanssen was gone, Stegmaier was gone, and Rossmann came in. Did he make any impression on you?

HOELZER: I don't remember him at all.

NEUFELD: He was the head of the army--

HOELZER: I remember the name.

NEUFELD: No impression. The reason I ask is only because some people have said there was a deterioration of relations with the army officers under Rossmann, but other people don't remember anything like that.

HOELZER: I don't know.

NEUFELD: Nothing at all. And then the impact of this change into the Elektromechanische Werke as a company in August, '44.

HOELZER: Ja.

NEUFELD: Did it make much of a difference?

HOELZER: It did not. We had Mr. Storch as director then. Well, there wasn't much change. No.

NEUFELD: You were officially a government company. Did salaries change?

HOELZER: It was a GOCO. Salaries, I don't remember that anything changed.

NEUFELD: Not much of anything.

HOELZER: No.

NEUFELD: Officially on paper, too, von Braun was pushed down in the organization to the same level as a few other people. But it didn't make any difference at all to the way things really operated. More or less the same as before, huh?

HOELZER: Same as before. Von Braun really was not eager to have high position or so. Von Braun just wanted to be doing what he wanted. With which position, he didn't care. Von Braun had a one track mind. This was his rocket business, that was it, in that period. Anything else was of no consequence. He used this whenever this was good for him, ja, but otherwise--

NEUFELD: But he was very good at getting along with people, I gather.

HOELZER: Oh yes, he could persuade people, convince people about all kinds of things, ja, he was very good. Even here, here in the United States, in the Congress he had the reputation of being absolutely honest. And if people wanted to know what goes on in space flight, they went to von Braun, and most of the time he was honest, ja.

NEUFELD: But would you say that he had a particular management role that you noted at Peenemünde in terms of science and technology? How did he stimulate ideas? What was the impact of his personality? His creativity?

HOELZER: Oh, everything ended up with von Braun. He was the focal point at Peenemünde. Not administratively. This was the commanding officer, there was Zanssen, there was Stegmaier or Dornberger. But technically, this was von Braun. In all fields. I

tried to give him a snow job once. I never tried to do it again.

NEUFELD: On what was the snow job?

HOELZER: I don't know anymore exactly. This was on this differentiation businss, yes. I tried to make it simple. I don't know exactly why it was a snow job. But he threw me out of his office. He said, "Don't tell me any stories here, get out."

NEUFELD: So he kept all the different technical problems and developments going, in his head, he had them all straight.

HOELZER: He had them all in his head. Von Braun would call a meeting, for instance with Roth, Mrazek, Steinhoff, people with different fields, and he would say, "Well, we have some problems with accuracy here. The question is now, do we improve the control system? Do we control the thrust? Or what else do we do, what else? Here is the problem. Okay, Roth. Tell your story." If the guy could not tell his story and was not prepared, he could get quite mean, ja. And then he asked the others, "Steinhoff, what do you think?" Then as soon as somebody said, "I don't know, I have to get somebody in here," then this was the end of him. Von Braun didn't like this. Von Braun wanted that everybody who was in charge of something was fully aware of what happened in his department. So he would then make a decision, "Okay, control system cannot be improved, we have to control the thrust," or vice versa. I just use this as an example. But he made the decision, based on what the others told him. And there was no coming back. With one exception, with Ernst Steinhoff. Ernst Steinhoff, when Ernst Steinhoff wanted to do something, he came back and talked to you until the night at 3 o'clock and you finally said, "Do whatever you want but leave me alone, I want to go to bed."

NEUFELD: Would he do that to von Braun as well?

HOELZER: Ja. Ja. He did this till von Braun left him alone and went to bed, said, "Okay, keep talking, I'm going to sleep."

NEUFELD: But--so I gather that von Braun could really chew people out.

HOELZER: Oh yes. Oh yes.

NEUFELD: It didn't produce any long run resentment among people?

HOELZER: No. No, no. Next day he came and said, "Well, any remnants from yesterday? Let's get them out, let's get them out, what is it?" Then he would just talk to you. Or he would grab you in the Officers Club and have a drink with you or so, things like

this. So I don't remember any, anything which had a lasting effect on me, negative, from von Braun.

NEUFELD: So very few people ended up--I mean, I'm sure there were exceptions, but few people ended up holding grudges or any--

HOELZER:--I don't know of any --

NEUFELD: --or feeling resentment about these kinds of conflicts.

HOELZER: I don't know of any.

NEUFELD: Because in understanding the management side of the accomplishment of Peenemünde, you have to understand among other things how it was that he sort of managed to keep a lot of people going all in the same direction and solving problems competently all the time, which means that --

HOELZER: --well, he could, he did this.

NEUFELD: Well, I think we've just about covered everything.

HOELZER: Yes, all right.

NEUFELD: That I wanted to cover, and I've almost held you late, if you had wanted to eat lunch--

HOELZER:--oh, that's Okay. That's Okay. I have a kind of a little spare tire.

NEUFELD: I appreciate very much your giving me the interview and the information, the technological information as well as all the documents--

HOELZER: Well, I have some more.