Haeussermann, Walter. January 24, 1990. Interviewer: Michael Neufeld: Auspices: DSH. Length: 2.25 hrs.; 49 pp. Use restriction: Permission required to quote, cite or reproduce.

Haeussermann begins by discussing his early life, education and work experience at Robert Bosch. Discusses his electrical engineering education at TH Stuttgart and Darmstadt; civilian draft to Peenemünde on December 1, 1939; initial assignment to Kreiselgeraete and Siemens Dec. 1939-March 1940; ideas for A-4 platform and one-axis simulator undertaken. Interested in rocketry in late 1920s/early 1930s; did not see "Frau im Mond." Discusses simulation of guide-beam response; A-4 and A-5; appointment to the Institute for Applied Physics; origins of the pendulum simulator; involved in Wasserfall guidance and control problems. Contacted by American intelligence after the war; offered position with the von Braun team; worked on Hermes II project; Peenemünde's in-house emphasis compatible with Army arsenal system; problems with US contractors on Redstone guidance system. Discusses Hoelzer, Steuding, Siemans, Steinhoff, von Braun and others.

TAPE 1, SIDE 1

1-2	Birthplace, early education, father occupation and work experience at Robert Bosch
2-3	Electrical engineering education at the Technische Hochschule Stuttgart and Darmstadt
4	Darmstadt and Berlin were the best TH's
4-5	Was not able to finish doctorate before the war; civilian draft to Peenemünde on Dec 1, 1939
5-6	Darmstadt had the largest role of any university because of Steinoff's role and individual recruitment
6-8	Interested in rocketry in late 1920s/early 1930s; did not see "Frau im Mond"; later assignment to Peenemünde unconnected
8-9	Initial assignment from Peenemünde to Kreiselgeraete and Siemens Dec 1939-Mar 1940
9-10	At Kreiselgeraete first ideas for an A-4 platform, and one-axis simulation being undertaken
10	No simulation at Siemens at this time; simulation with whole rocket at Peenemünde
10-11	Organization of the control laboratory at Peenemünde in 1940; roles of Hoelzer and Steuding
11-13	Simulation of guide-beam response; some data acquired from airplane flights; all simulations time-frozen
14	Primary jobs from 1940-42 were improving simulators and time response of control systems
14-15	Aware of competing Kreiselgeraete and Siemens systems; Askania-Moeller system dropped out quickly
15-16	Stable control systems produced for A-5 based on rate gyros, but not an ideal solution; Hoelzer advocated use of electronic differentiation problems with whole-rocket

simulation

16-17 Siemens system favored over Kreiselgeraete for production simplicity; but still competitors for A-4 when he left Peenemünde in May 1942

TAPE 1, SIDE 2

17-19 Steinhoff competent in building up the guidance and control laboratory

19-21 Surprised at von Braun's age but soon convinced of his leadership abilities and technical competence; von Braun was very interested in guidance and control

21-22 Von Braun played a significant role in guidance-and-control development reasons

22-23 von Braun never left the impression of being aristocratic; always an engineer to him

23-24 Had very little contact with the Army officers, even socially

24-25 Reasons for his return to Darmstadt in 1942; appointment to the Institute for Applied Physics

25-26 Contracts with Peenemünde of Professors Viewing, Walther, Buchhold and Hueter at the TH Darmstadt

26-27 Spirit of cooperation with Peenemünde and other Darmstadt institutes, as well as friendly competition

27-28 Archive reports as a form of substitute secret publication

28-29 His work also led to his doctoral dissertation, which was classified

29-30 Reports and results were necessary all the time to justify draft-exempt status; one could never be overconfident; for the same reasons viewing got involved with Navy research

30-31 Not aware of any Nazi Party or Reichsforschugsrat pressures on the university of even the Volksturm in 1944/45

31-32 Origins of the pendulum simulator and manufacturing of same

32 Dimensions of the pendulum simulator

32-34 Became involved also in Wasserfall guidance and control problems, but not well acquainted at the time about the program

33-34 Conditions for work increasing difficult, especially after the destruction of Darmstadt in a bomber raid

TAPE 2, SIDE 1

34-35 Development of ability to determine stability in nonlinear systems an important accomplishment at Peenemünde

35-36 Accelerometer development also an accomplishment

36-38 A surplus Siemens guidance system for the A-4, or an American copy, used for the Explorer I launch in 1958

38-40 Contacted immediately after the end of the war by

	American intelligence; offered a position with the von Braun team; surprised that it was voluntary; did not go immediately because wife was ill
40-41	Taught in Darmstadt until end of 1947; then went to permanent immigration
41	Contacted by the French, but refused them; the US was "the land of the future"
41-42	Convinced by Dr. Hoelzer and Steinhoff by telephone to come over; some disappointment at conditions at Fort Bliss, but also optimism
42-43	Work on the Hermes II project challenging; Redstone work at Huntsville different because of the time pressure to finish an actual missile
44-45	Organizational system more or less the same in the Army arsenal system
45	Definition of the limits of the guidance-and-control laboratory maintained from Germany to the US
45-46	One of von Braun's unique contributions was emphasizing horizontal communication between groups to solve problem; he was the primary initiator of this management concept
46-47	Fortunate that Peenemünde's in-house emphasis compatible with the Army's arsenal system; advantages of same in dealing with contractors
47-48	Problems with contractors in the United States on the Redstone guidance system; relations later with Bendix
48-49	The large in-house laboratory concept for guidance-and-control at Peenemünde largely forced by the circumstances of heavy time-pressure and lack of industrial experience; ideology unimportant
49	People selected at Peenemünde to personal acquaintance and "networking"

Interviewee: Dr. Walter Haeussermann

Interviewer: Dr. Michael Neufeld

Location: Huntsville, Alabama

Date: January 24, 1990

TAPE 1, SIDE 1

Dr. Neufeld: First, Dr. Haeussermann, I'd like to ask you to state your full name, your birth place, birth date and so forth.

Dr. Haeussermann: I'm Walter Haeussermann and I was born 2nd of March, 1914.

Neufeld: And what was your birth place?

Haeussermann: My birth place was Kuenzelsau, Wuerttemberg.

Neufeld: And that is where in Wuerttemberg?

Haeussermann: That is in the northern part, today part of Baden-Wuerttemberg. Nuremberg is only about 60 kilometers from Kuenzelsau.

Neufeld: Oh, really?

Haeussermann: Actually west of Nuremberg.

Neufeld: Is it in the area of Rothenburg?

Haeussermann: Yes.

Neufeld: So you come from near Rothenburg.

Haeussermann: Yes. I went to school in Schwaebisch Hall.

Neufeld: A name I know. Unfortunately I've never had a chance to go there.

Haeussermann: A beautiful place.

Neufeld: Your father's occupation?

Haeussermann: My father was a merchant, actually had his own big hardware store, agricultural machinery, etc.

Neufeld: Agricultural machinery and so forth. And he stayed with that pretty much throughout your career, up to the Second World War, when everything was disrupted.

Haeussermann: That's right.

Neufeld: And so you went to school in Schwaebisch Hall, and you went to gymnasium? Or Realgymnasium?

Haeussermann: It was Oberrealschule, Schwaebisch Hall.

Neufeld: And would you say your inclination was technical, scientific?

Haeussermann: I was always best in mathematics and physics--also in chemistry--and I usually volunteered to set up the equipment for my teachers in physics and in chemistry.

Neufeld: So when you went to university, you pretty much had predetermined that you wanted to be an engineer or scientist?

Haeussermann: I first thought I should become a physicist. But engineers in Germany had to go through a kind of abbreviated apprenticeship before you enter any technology, so I got this practicum, as we called it, for one and a half years at Robert Bosch Company, and that gave me then the idea I'd better go into engineering rather than into physics. I wanted to be closer to the hardware.

Neufeld: Had you planned which Technische Hochschule you wanted to attend?

Haeussermann: First, in 1933, I went to the Technische Hochschule in Stuttgart, and after I received the Vordiplom which is equal to the B.E. degree, in engineering; I went then, because I preferred the level of the Institute of Technology in Darmstadt. At Darmstadt in '35, I stayed to take my master's degree in electrical engineering, and even before I got this, I had an excellent chance to become an assistant professor at the Institute of Electrical Machinery. After I had received my master's degree I stayed there in order to write my thesis. However, the war came in between, and it was not possible to continue on my thesis, which involved a lot of buildup of electrical machinery, so I could not continue on this.

Neufeld: OK. Let me get some details from that. You first went to Stuttgart in 1933. I was wondering, had you determined that you definitely wanted to go to a Technische Hochschule because of course one could also do physics in the university.

Haeussermann: I had made up my mind by that time already, after I

had the practical experience from Robert Bosch, that I wanted to become an engineer. And I wanted to go into an engineering field. And for that reason I went to the Institute of Technology in Darmstadt. In Stuttgart first.

Neufeld: In Stuttgart, and you pretty clearly focussed on electrical engineering.

Haeussermann: Yes.

Neufeld: Right from the beginning.

Haeussermann: You have to understand, at that time, the first four semesters were almost equal whether you studied mechanical engineering or electrical engineering. There was a little more emphasis on electrical engineering, and to go specifically into electrical engineering, almost equal to electronic engineering, was then in the later semesters.

Neufeld: So that initial two years is pretty much transferable to other places. So at Darmstadt you started in '35 and become a Diplom-Ingenieur in '37, in two years? Or did it take a little longer?

Haeussermann: Since I, at that time, had a practical, or I had to work on some practical equipment, actually especially built machinery, it was delayed, and since I was at the same time already an assistant professor, I got my, actually my thesis or my work I wrote up, a little delayed, I got my degree in March, '38.

Neufeld: So you were already an Assistent.

Haeussermann: Yes.

Neufeld: Before that time, which was of course a good position. What was your field then and who were your advisors?

Haeussermann: At that time, I was at the Lehrstuehl fuer Elektromaschinenbau. That means electrical machinery. And my professor was Professor Franklin Punga. He was very famous in his field.

Neufeld: You considered Darmstadt a superior institute?

Haeussermann: At that time, yes.

Neufeld: Where would it rank among all the Technische Hochschulen, or is such a ranking possible?

Haeussermann: At that time, I considered Darmstadt and Berlin

equivalent, as the very best.

Neufeld: And you were working in heavy current electrical generation, that kind of thing, or what was your area?

Haeussermann: Yes. That is correct. But as a hobby, I had taken on also control of electrical machinery, voltage-like process control, frequency control, and so on. This was the only way to go into the field of control. So at that time, I had, as much as it was possible, already specified to go into this area.

Neufeld: Which would have brought you then into contact with the electronics, vacuum tube electronics.

Haeussermann: Right. Yes.

Neufeld: To some extent. That was unusual for someone involved on the heavy current side?

Haeussermann: Not necessarily, because it was obvious that there is a need for precision control in the future. And the specific problem was the synchronization of large networks.

Neufeld: OK. So you started as an Assistent in '37 or so.

Haeussermann: Quite.

Neufeld: While you were finishing, in the same field, or did you move?

Haeussermann: I could not finish in that field because in '39 the war broke out. I was drafted into the army. And by that time my professor learned that there is a demand for engineers, and he recommended --or he gave my name to the people at Peenemünde. Thus I was pulled out of the army before I left Germany for any war activities, I was pulled out and started with my work as a civilian in '39, actually the 1st of December, '39 in Peenemünde.

Neufeld: So that person, was that Punga again?

Haeussermann: Yes, Punga had recommended me for this. But then I, in Peenemünde, worked under Dr. Steinhoff.

Neufeld: I'm interested, I know that of all the Technische Hochschulen. Darmstadt has the largest role in Peenemünde.

Haeussermann: You might have heard this before --it's because Dr. Steinhoff and others in Peenemünde had excellent relations to the Technische Hochschule in Darmstadt, and Dr. Steinhoff requested therefore, if there are people who can be recommended for working in Peenemünde, he wanted to have the names from the professors in

Darmstadt.

Neufeld: So --I talked to who else, from Darmstadt? I'm trying to remember now.

Haeussermann: Hoelzer.

Neufeld: Hoelzer, yes. I talked to him. I talked to him about how he came into the business. Unfortunately as I said I haven't got his transcript.

Haeussermann: I think you talked also to Mr. Hoberg.

Neufeld: No, not yet. But I certainly saw from what they were describing that it had developed not so much because, it doesn't seem anyway that Darmstadt was --or Dresden, which probably had the second largest role -- were picked out for a particular reason, so much as it was a kind of network. People went back to their original Doktorvaters or supervisors, and so forth, and said, "Who have you got? Can you help me?"

Haeussermann: Yes, that's right.

Neufeld: So you think then--obviously of course Darmstadt is one of the best. That might also be a factor. But was largely a sort of network started because Steinhoff went to --at least in the electronics and guidance and control.

Haeussermann: Right.

Neufeld: This didn't come about because of the assignment of particular research projects to Darmstadt after September, '39?

Haeussermann: No, first many people from Darmstadt started to work in Peenemünde, and they knew the best place to do research work. That's why contracts were given then to Darmstadt.

Neufeld: Yes, because I know there was the so-called "Tag der Weisheit" in September '39. This is where they first started bringing people in large numbers into Peenemünde from the universities. See, I guess I was under the false impression that you had been involved in, you had come from that side, that you had been at the university and been brought in, but actually you were assigned to Peenemünde itself and were an employee of Peenemünde.

Haeussermann: Right. I don't know the exact English translation for this. I was "dienstverpflichtet." You know this term? As such, I was paid by Peenemünde, but had immediately the right at the end of the war to return to Darmstadt into my former position.

Neufeld: So technically you were kind of on loan--

Haeussermann: --yes--

Neufeld:--from the university and on loan from the army also.

Haeussermann: Right.

Neufeld: So you never, you were just in basic training or something and then almost immediately assigned into what was called "BSM."

Haeussermann: Quite.

Neufeld: Into the electronics and guidance laboratory. When you went, you had no previous knowledge of Peenemünde's existence?

Haeussermann: Never. It was a completely new situation for me.

Neufeld: Did you have any knowledge of, or had paid any attention to, the rocket?

Haeussermann: From the hobby viewpoint, yes, very much so. I had no idea about the progress that had been made in the meantime, and I was astonished about what I saw then in Peenemünde.

Neufeld: From the hobby standpoint, what do you mean? What did you observe in the late twenties?

Haeussermann: Well, I read, of course, whatever was available in this field, like the work of Valier and others.

Neufeld: Yes, because I've written an article about that period in the late twenties, where I tried to look at how it fit into Weimar, into the Weimar Republic, but--and you just would say that it was a fascination as a teenage boy with --

Haeussermann: --yes--

Neufeld: --with Oberth , did you see "Frau im Mond" at the time?

Haeussermann: No, I didn't.

Neufeld: You didn't see the movie? In effect, was it accidental then that you were assigned to Peenemünde?

Haeussermann: Yes, right, just on recommendation of my professor.

Neufeld: There would have been a considerable gap probably, because in the space flight, in the rocket thing, because you wouldn't have heard much about it, right, during the Third Reich

there wasn't much said.

Haeussermann: On the other side, don't forget, our education was very broad. I had excellent lectures received in the field of gyrodynamics, as far as that time you could get. So whatever was available at that time was not basically new to me. On the other side, there wasn't much known, and we had to start from scratch in many areas.

Neufeld: What I'm actually asking you here is not the technical side, which we'll get to, but, you probably hadn't thought much about space flight or rocketry since the late twenties?

Haeussermann: Not at all. No. I didn't think that it was so close at that time.

Neufeld: It had disappeared as an issue in many ways, under the Third Reich, had sort of gone down and disappeared out of the media. OK, so you came in 1939 and you had not known Steinhoff beforehand?

Haeussermann: No.

Neufeld: He'd been in another institute or something.

Haeussermann: That's right.

Neufeld: At Darmstadt. And you arrived, I'm trying to remember, did you say December the 1st?

Haeussermann: 1st of December.

Neufeld: 1st of December, 1939. What was your first impression of the place, of the organization of Steinhoff?

Haeussermann: Well, I was at the beginning told, we don't even have yet a laboratory where you can experiment, make experiments, where you should get familiar with the control components. So I was told, the best you shall get an assignment from us to go to one of our contractors, and this was the Kreiselgeraete in Berlin-Zehlendorf. I was assigned to Berlin-Zehlendorf for three months, till Easter, 1940, just to get familiar what Kreiselgeraete had been doing at that time. The intent was that afterwards I go in the same way to Siemens-Halske. However, since there was some pressure to continue with the building up of the laboratory in Peenemünde, I had been only for about a little over a week, in Siemens-Halske, and then I had to start building up the laboratory in Peenemünde, what later became then the control laboratory.

Neufeld: I have interviewed Dr. Fritz Mueller for a long period,

in November. So you were assigned then in the same unit that he was working in, I assume.

Haeussermann: Yes.

Neufeld: And working on looking at how the platforms that were built for A-5, I assume, right--

Haeussermann: Yes.

Neufeld: --at that, the state of, SG-52 and--

Haeussermann: --yes, and at that time, there was already some idea about SG-66. Which was later then, the second platform the only real stabilized platform for the V-2.

Neufeld: Right. This would be sort of December '39 through to March '40. You had some kind of definition of what SG-66 would look like?

Haeussermann: Yes.

Neufeld: On paper only.

Haeussermann: Not only this, but also how to test the dynamics of the missile. See, at that time we had no idea yet how to control, in a stable form, one axis of a rocket and so we had to develop analogue computers, and Kreiselgeraete had the first analogue kind of simulation of one axis of the V-2 or at that time the A-5. And this method of simulation was then used first also in Peenemünde.

Neufeld: And this you experienced at the Berlin-Zehlendorf factory?

Haeussermann: Right. Quite.

Neufeld: And that was, the simulation was a gyro being put on some kind of table, tilt table?

Haeussermann: Yes, right. And the rate gyro. For precision, you needed a positioning gyro, and for the angular rate, or to have stable behavior, you had to have a rate gyro.

Neufeld: So you would have the position gyro and the rate gyro on a table that was being moved, and the control signals being sent to produce by those gyros being sent to equipment which you could --

Haeussermann:-- to, let's say, electronic amplifiers, and from

there to the servomotor, that showed then the reaction on the simulator, and caused damping of the motions if you had given a disturbance.

Neufeld: Was the damping successful at that point?

Haeussermann: Oh yes. The only thing was, it was not very accurate, and this required large tolerances, therefore, in stability, because we could not have at that time high performance of the simulation. And this was a first step to go in Peenemünde, to improve the analogue simulation devices which produced too much friction.

Neufeld: And improving, I suppose, a servomotor response which was slow.

Haeussermann: We had a lot of problems with the servomotors, and even the servomotor controller.

Neufeld: So although you would damp the motion, successfully damp the motion of the table, I assume that you had a number of oscillations until you received that damping so that the reaction time was slow and there were errors that had to be eliminated. So you went back to --let me first ask you a question about the Siemens-Halske system. At that point, I'm not entirely clear on just when they came into consideration. I guess at some time in '38. That would have been too far before your time to have known.

Haeussermann: I could not try to give you any elaborate information on that. I only know that they were involved. But at that time I did not see any simulation equipment, to test the simulation equipment of Siemens-Halske, and that's where we started, you knew that, we tested their equipment at Peenemünde.

Neufeld: So you felt that they didn't have the capability to test their own simulation on even one axis.

Haeussermann: ...one method to test before the laboratory was established was to suspend the rocket in about the center of gravity and have it moveable about one axis, the axis to be controlled. And then we had springs attached to simulate the aerodynamic forces, the restoring forces, and to give the system its own natural frequency, and then the control was exercised through the jet vanes on the moving rocket. A deflection, initial deflection of the vanes caused a disturbance or signal, and then the system had to move back into its desired position, in a dynamically stable way. And this was the only method, when I came to Peenemünde, which was used to test the dynamic behavior of the control system. It was very expensive of course and cumbersome, because each time, you had to have the engine operating.

Neufeld: And that would have been done with an A-5, right, because the A-4 wouldn't have existed even.

Haeussermann: Yes. That's right.

Neufeld: In 1941.

Haeussermann: It was all done on the A-5.

Neufeld: So you would have these simulations, and you did not at that time have in the laboratory, at least because you were building the laboratory, did not have even what Kreiselgeraete had, in terms of a tilt table.

Haeussermann: Right.

Neufeld: So you were then brought back to build up that portion of the guidance and control laboratory?

Haeussermann: Yes.

Neufeld: So your first task then in early '40 at BSM under Steinhoff was building up this capability of simulation.

Haeussermann: Correct.

Neufeld: In one axis.

Haeussermann: Correct.

Neufeld: And so, did you have to hire new people to do that or actually organize resources?

Haeussermann: Actually several people worked on this. I reported to a Mr. Scher, and he did not come to the States after the war. He went to one of the museums in Darmstadt afterwards. He reported actually to Dr. Steinhoff. Now, working in this room were several people. Helmut Hoelzer, as you already know had always had the idea it was easier to get the angular rate from the attitude information by differentiating this, and so he had been working on this. And this was one of the schemes we tested out on that simulation equipment in the laboratory. We had to have several engineers. For instance, we had mechanical engineers and designers who worked on the equipment itself. Mr. Josef Boehm was quite involved in this.

Neufeld: So I know that Dr. Hoelzer, as he later became Dr. Hoelzer anyway, was working on the guide beam or Leitstrahl in early '40. Was he at that point doing this also as well on the side, or is this after he finished with the guide beam work?

Haeussermann: At that time, you had a man by the name of Dr. Steuding. Dr. Steuding was the one who mathematically was involved in the dynamics of control and guide beam stability, and so we developed, at that time also in the laboratory, another system, or another analogue simulator, that showed you how the missile moved back into the guide beam, when it was, through a disturbance like from wind or so, put outside the center of the beam. And this was another equipment which I had designed at that time. And when Dr. Hoelzer looked into how the beam could be generated, how the signal could be received for guidance, he gave us this information and we then simulated the behavior of the missile with respect to the guide beam.

Neufeld: And this is a simulation carried out completely in electronic form, right?

Haeussermann: It was almost mechanical, again. You had a second order differential equation which you used to move a mass, in this case a rotating mass, which was constrained and controlled by a motor.

Neufeld: So you had the rotating mass as a simulation?

Haeussermann: Yes.

Neufeld: I'm trying to imagine this, because I guess my technical training is not up to it. The rotating mass simulated the --

Haeussermann: --the mass of the system, of course a translation of the motion towards the center of the beam. Now, the mathematical basis of it is the same. You have a second order equation, and whether you simulated this by rotating or by actual transverse motion, that doesn't matter. The transverse motion was almost impossible to be used in this case, so we used the rotating.

Neufeld: And the correction, the equivalent of the correction for lateral dispersion was the rate of rotation?

Haeussermann: The distance from the center of the beam.

Neufeld: OK. Now--

Haeussermann:--and of course you had to have some damping effect, again, by generating a derivative of the system displacement.

Neufeld: So did the simulation which you developed of attitude control in one axis evolve from simulation of lateral deviation from the guide beam?

Haeussermann: We kept these two as two separate analogue systems,

one for attitude control, and one for beam control.

Neufeld: What I meant by evolved, did the equipment evolve from the simulator for lateral dispersion, or was it parallel development?

Haeussermann: No. We used the equipment by checking it on the analogue computer, and improved both at the same time, we refined the analogue computers, and then we could give better specifications again for the equipment that had to be used in actual flight.

Neufeld: What I'm saying is--well, I'm sure they evolved simultaneously to some extent. Did one come first? Did the guide beam simulation come first or did you sort of do these two things more or less in parallel?

Haeussermann: Well, we had first the idea how to do it. We had the idea how to build the equipment. Then immediately we asked, how can you test it? And of course in the case of the guide beam, you had first made some airplane flights, to measure the signal, when the airplane deviated from the center of the beam. And this gave us first the signal strength, from which then we derived mathematically the equations, and then we determined mathematically what the missile should do, what the gain factors would be, and then we tested it on analogue equipment.

Neufeld: So that you had something to go on.

Haeussermann: Right. I should add one additional point. All these simulations, like attitude control and control with respect to the flight path, of course were only time-frozen simulations, not considering the varying coefficients of the equations. See, when you have a higher speed, when you are constrained, aerodynamically constrained, it's different, your mass is different because of the fuel and all this, this changes your gain factors for the control system. So you had to consider all these viewpoints, and you could only do it step by step, test for instance at takeoff, shortly after takeoff, on the simulation equipment, and the tenth and the twentieth and the fiftieth second and so on.

Neufeld: By a series of steps, you could get an approximation.

Haeussermann: Right. You could change the coefficient at that time.

Neufeld: This might get me out of chronological order, but did you solve that problem later with the development of the simulators and Hoelzer's analogue computer?

Haeussermann: The pendulous simulator was never used with continuously changing coefficients in Peenemünde despite the fact that it did possess this feature. The pendulous simulator was mainly applied to adjust and calibrate the control (mixing) computer. Test specifications were written for certain (frozen) flight times, e.g, launch, highest natural control frequency etc., in order to simplify testing. Hoelzer's analogue computer was designed and used to simulate especially the flight path, for which the time changing coefficients were incorporated.

Neufeld: OK. So can you remember approximately when in 1940 or '41 you finished, was it in 1940 that you finished the simulators and started using the simulators for attitude control?

Haeussermann: It was a continuous development and improvement. They were conceived in '40, but the development continued practically throughout the time of WWII. For instance, for Wasserfall, another pendulum type, purely mechanical, had to be developed, which I developed in Darmstadt, and this one had the features to avoid time constants which were still permissive in the V-2 control system, or in the V-2 analogue computer.

Neufeld: So from '40 to '42, while you were still physically located at Peenemünde, that was your main task, was it? And you were involved in simulating the performance of the competing gyro systems.

Haeussermann: Yes. Improving simulators, as well as there was a problem how to reduce the time constants in the control system.

Neufeld: OK, by reducing--let me see if I can translate that into more non-technical language. You mean reduce the response time.

Haeussermann:--the time for response--

Neufeld: --a faster time response within the system. So you were constantly striving to improve the time constants for A-4 systems, both the Siemens system and the Kreiselgeraete.

Haeussermann: Yes.

Neufeld: And I know that also some time in there, and I'm not exactly sure when right now, the Askania-Moeller system came into play, and I know even less about Anschuetz.

Haeussermann: Yes, I got familiar somewhat when I started in Peenemünde. Very soon these schemes were abandoned. I think it was already in 1940, they were put aside, because the other two systems were more promising.

Neufeld: So when you returned to Peenemünde in March, 1940,

Anschuetz and Askania were already possibilities that were being investigated, that is they were already --?

Haeussermann: At that time, Askania was, only Siemens-Halske was at that time involved, and Askania was virtually followed up as a pickup in case one of the other systems, the primary systems, would not work.

Neufeld: I know that, I've just seen titles of archive reports that indicate that there were A-5 launches with Askania guidance in '40 or maybe into '41 but I'm not really sure because I haven't exactly checked how many there were.

Haeussermann: In this case, Askania--

Neufeld: --Askania--gyros?

Haeussermann: --Siemens system?

Neufeld: Askania, it appears from the titles of these archive reports that there were A-5 launches with Kreiselgeraete, that platform, with the Siemens system, which I guess is not a platform--

Haeussermann: --yes-- yes --

Neufeld: --because it's rocket fixed--

Haeussermann: --yes, right --

Neufeld:--at it appears with Askania-Moeller --

Haeussermann: --now, that's what I do not recall. I've been at most of these launches. I do not recall that there was one with Askania.

Neufeld: I'd have to check it to make sure that was true, by looking at the reports themselves. But your impression was that Askania fell out of the running fairly quickly.

Haeussermann: Yes.

Neufeld: What specific difficulties, what problems or inferiorities in terms of accuracy and so forth, do you recall?

Haeussermann: I think, and that is surely my guess, that people did not investigate to a satisfactory degree the dynamic behavior of the rocket, and did not use simulation equipment as we did. And so they used only these very expensive tests on the test stand, where the missile was suspended with one axis, and this was just too cumbersome to make a sufficient number of tests

in a short time to check out the system. Their theoretical background was in my opinion not so good.

Neufeld: And the Anschuetz name comes up. I get the impression they were involved even less or even more briefly.

Haeussermann: Yes.

Neufeld: They offered you a gyro system as a possibility?

Haeussermann: I could not say. I was not involved in that.

Neufeld: OK, so that --an anecdote that Helmut Hoelzer told me, which I need possibly more information about, is that he was telling me that all of the systems as they existed were not, did not produce stability, in part because they depended on a mechanical or hydraulic transmission of the forces to the control vanes, right? Or the signals --

Haeussermann:--I think we flew several systems which were well stabilized, with rate gyros, like the Siemens system, and also the method of Kreisalgeraete, and we had good stable flights.

Neufeld: On A-5, you mean.

Haeussermann: On A-5, right. But it was not an elegant solution. We had often because of vibrations and resonances difficulties. Helmut Hoelzer promoted the use of electronic differentiation to obtain the rate from the attitude signal, and he succeeded with this, and then of course he tested this first also in the laboratory, and then in actual flight.

Neufeld: In A-5?

Haeussermann: Right.

Neufeld: You did simulate this with A-5, where you took out the rate gyros and did it that way. See, what he told me was that at some point, there was an A-4 simulation where they had built A-4 control systems and using the whole rocket suspended in gimbals, that they had attempted to demonstrate the competing control systems without using the electronic differentiation, and that those systems were not inherently stable, as opposed to using the Mischgeraet or whatever.

Haeussermann: Well, mainly the stability problem at that time was with the method to suspend the missile, you had huge friction, and so even if you had some results of stability on your test stand, it was no guarantee you would have the same results in flight, because the simulation was too poor.

Neufeld: OK, so you were working on these competing systems. I guess another question I had about the competition between differing guidance and control systems was, why did the Siemens system become the standard system? Because Kreiselgeraete had the advantage that they'd been there from the beginning, and had built the system for A-3, which had its problems, and then had built a three-axis stabilized platform for A-5 and so forth, but the Siemens system, I guess because it's simpler, became the standard system.

Haeussermann: I recall that because we did not need the gimbal, or a large gimbal arrangement, and it was easier and faster to be developed, time- wise, and we had always tremendous time pressure. And so as far as I recall, this was the reason why the Siemens system was given first priority.

Neufeld: When you say given first priority, was that in resources?

Haeussermann: I think, no, resources were, I think, treated equally, the same way in priority, but the decision was then made to use the Siemens because they just had earlier results in production.

Neufeld: Basically they only had two gyros to produce, no servo loops--

Haeussermann:--not the complex servo loops as needed for the system--

Neufeld:--platform --

Haeussermann: --the platform.

Neufeld: And could be produced faster.

Haeussermann: Yes.

Neufeld: So that from what you're saying, it sounds as if some time, maybe in 1940, latest in '41, it was clear that Anschuetz, Askania had been eliminated, that Siemens was going to be the primary system for A-4, and that Kreiselgeraete was going to develop a platform that was going to take longer.

Haeussermann: --timewise, the competition between Siemens and Kreiselgeraete went much longer, beyond the time I had been in Peenemünde. I left Peenemünde, as I said, by the 1st of May, '42. And at that time both were still in clear competition.

Neufeld: So that Siemens was still trying to improve its system as a competitor. You considered them in a sense competitors.

Haeussermann: Oh yes.

Neufeld: They were fighting for a long term improved system over the initial basic system.

Haeussermann: Yes, right.

Neufeld: That was done for A-4, OK. ...

TAPE 1, SIDE 2

Neufeld: Now, before I go on to talking about Darmstadt, and maybe if there's anything I haven't thought about that's important about your period at Peenemünde, which was, except for a brief period, March '40 to May '42, as I understand. I want to ask you just a couple of questions about organization, personalities and so forth. So you came, the first time you met Dr. Steinhoff would have been when you got there, at the end of '39.

Haeussermann: Right.

Neufeld: What was your initial impression of him? What did you think of him as a manager, leader?

Haeussermann: Well, I considered him as a very dynamic leader, who himself had a lot of ideas, but just in the field of say electrical engineering, he needed considerable support. He had good basic background in physics, but what the practical response is, for instance, with respect to time constants on this area he needed help, and he was aware of this, and got the necessary manpower to fulfill the requirements we had, to investigate what should be done.

Neufeld: So he was energetic in building up your laboratory? Do you think he did a good job?

Haeussermann: Yes, I think so.

Neufeld: In building that up.

Haeussermann: And he also wanted others to understand completely what was going on, and gave his own inputs.

Neufeld: And as far as you're concerned, he became very competent in understanding other people's problems.

Haeussermann: Oh yes.

Neufeld: Because I know that, one person has told me that he did not get along with Steinhoff.

Haeussermann: I never had a problem with him. Of course, I had to make it clear why I had a certain opinion, and stand up to this and tell him, if he had a different opinion, why I have another opinion. And then we always went very well along.

Neufeld: Yes. I want to say here of course that I don't think anyone should shy away from occasionally talking about conflicts or whatever, because --

Haeussermann:--no, no --

Neufeld: --that's a natural part of any organization, that people disagree sometimes, and sometimes you have to change your mind or the other person has to change his mind. So arguments happen. It doesn't necessarily reflect on somebody. Did you meet Dr. von Braun?

Haeussermann: Oh yes.

Neufeld: Immediately when you got there?

Haeussermann: I think the first of things, I met him.

Neufeld: What was your impression of him then?

Haeussermann: Well, I was astonished that a man two years older than I, von Braun was at that time 27, had the full responsibility as technical director of such a tremendous development. But very soon, I understood why he was selected for this. On almost the first day he invited me, he had at that time already been a pilot, so he invited me on a flight over Peenemünde and over the Baltic Sea, where we had to observe one bomb release from a high altitude bomber, and he wanted to observe how stable it went towards the target area, and as soon as it was released and he had it in eyesight, he dived with his airplane --it was quite a new experience for me!--to follow the bomb, caught up the plane in time, and then gave the command through radio, the information to the boats, where in this area to find that same spot which was produced by the bomb.

Neufeld: So this was a bomb test conducted by Luftwaffe, or was this one of these drop tests with A-5 models?

Haeussermann: I think it was a drop test in connection with A-5.

Neufeld: So he was looking at that drop. Yes, because I know that, I just saw an article of Dr. Hermann where he mentions that they had, in order to look at the stability through the transonic region, that they had to drop iron models of A-5 to look at that.

Haeussermann: And Dr. von Braun particularly he, at that time, wanted to know exactly what everyone was doing. He dropped into your office and checked what you were doing, and you had to explain to him. For instance, I had at that time evaluated--it was of course somewhat later after I returned from Berlin-Zehlendorf--I evaluated graphs which were taken in flight about the guidance and control system, and evaluated those. It was not easy because at that time the recording was very poor, and he wanted to know how I evaluated it and how I understood the various signals.

Neufeld: This would have been recorded only on a strip chart recorder, was it?

Haeussermann: Yes. That's right. Which was of course recovered.

Neufeld: So he -- a strip chart or they also had an oscillograph in flight, right?

Haeussermann: --it involved an oscillograph with five channels at that time.

Neufeld: And you would be looking at a movie? Photographs?

Haeussermann: No, a strip chart.

Neufeld: You had strip charts on that --

Haeussermann: Also oscillograph.

Neufeld: I remember Dr. Reisig telling me about the oscillograph--

Haeussermann: -- he was involved in that--

Neufeld: --the oscillograph that was lofted with the rocket. So you had those things. When he gave you that, was he in effect also testing you to see what your command of the thing was?

Haeussermann: He wanted to see how far I'm aware of the dynamics and the response, I could explain the behavior of the signals, and well, if there was anything which could not be explained, to draw conclusions for further investigations in the laboratory. See, we had also sometimes a failure, and he wanted to know what the failure was, and so it was important to know what is normal and what is not normal, and we had to see what happened first from the chart, and if you didn't have enough channels, you had to do a lot of guesswork.

Neufeld: Yes. Now, you said that you rapidly saw why he was picked to be the head. What were the particular management or leadership or whatever skills that he had that rapidly came out?

Haeussermann: He was desiring, on his side, to be as firm in electrical engineering, in electronic engineering, as in mechanical engineering. He educated himself continuously in this respect, and he used this opportunity to learn from the various individuals employed at Peenemünde, and he wanted to grasp whatever was possible, and to explain phenomena which he could not see directly a solution to, he wanted to see how we consider this, and how we draw to conclusions from the charts, from the results of the investigation, and why we make certain recommendations afterwards, and he was not satisfied until he understood this completely.

Neufeld: So he was both extremely willing to immerse himself in the details, and able, sort of intellectually able to keep up all the time.

Haeussermann: Right.

Neufeld: To what extent was he involved in actual solutions that you produced in those years for guidance and control?

Haeussermann: Of course, he himself, just by questioning, he often said, "Why don't you do it this and that way?" To round out the picture for his own understanding, and in this way he very often generated original ideas which we worked out.

Neufeld: Part of this, part of why I'm asking this is to try to get a picture of just what, how in a sense, how close or how distant his intervention and role in this is. I mean, it's harder for you to judge the other question, how much was involved more in guidance and control issues than in other issues? I've been told essentially that he felt that Thiel was competent to deal with propulsion, he was not nearly as personally involved in that area as he was involved in the business of BSM and electrical engineering, guidance and control and so forth. Do you think that's true? I mean, it's hard to judge all this.

Haeussermann: It's hard to judge. But from later years, especially here in Huntsville, I only can say, he stayed interested in all the areas, and wanted to know why we are doing certain things, why we didn't have other solutions. He asked for other solutions and asked, "How do you compare these solutions? And why do you give preference to the one or the other?" And very often he had, just like later on the simulation for the first Explorer, he had suggestions why I, or he requested to know why we didn't have more cumbersome equipment simulating mathematical behavior better than he had assumed and that we had presented to him?

Neufeld: So did he stimulate you to produce a better simulation at that point?

Haeussermann: At least he wanted to know why--

Neufeld:--why the limits --

Haeussermann: --why we gave ourself certain limits and what assurance we could give from the limited test we made, in comparison to the more complex test he had thought would help. And he really wanted to know why were we so confident that we could do this and be successful.

Neufeld: And what were the limits as to what actually could be done? Just from the standpoint of style and so forth of von Braun, it's always an interesting question about his management style and personality, would you say that he in his appearance, in his language or whatever, appeared aristocratic? Did he have an aristocratic background?

Haeussermann: No, not at all. My feeling has always been, he was speaking as an engineer to an engineer. I never had any other feeling.

Neufeld: And in that sense then, in that sense it did not enter your mind at all that it was even an issue?

Haeussermann: No, never. Never.

Neufeld: Because I gather he could use that with army officers, it seems to me, that he had the right kind of attitude, style and so forth, he would be impressive to all different kinds of people, that he could speak to whoever he's speaking to at their level?

Haeussermann: He was for me just another engineer, in this high position, whom I admired because of his personal capabilities and wide field of interest.

Neufeld: Now, to complete these sort of management and personality kinds of questions, you had much contact with at that time Colonel Dornberger?

Haeussermann: Not at all.

Neufeld: From the standpoint of the military administration --

Haeussermann: --of course I knew him well from meeting him in the officers club and so, but from a technical viewpoint, I had no impression of him.

Neufeld: He was just a distant leader boss figure from Berlin largely, that's where he spent most of his time. Or any other officers like Colonel Zanssen, those people?

Haeussermann: I had no personal impression of those in the field in which I had been working.

Neufeld: Sometimes I'm interested to see what people are in daily contact, and on what basis does the military administration side have contact with the research and development laboratories. But as far as you were concerned, the army officers, the fact that this was a military installation was only a kind of external feature.

Haeussermann: Right.

Neufeld: Which didn't have much to do with your daily --

Haeussermann: --with our daily work.

Neufeld: Daily work.

Haeussermann: Of course, from time to time they visited our laboratory, and we showed them what we had achieved with respect to the dynamics or the simulation and so forth, but that was all.

Neufeld: From the social standpoint, when people would meet on weekends or parties and so forth, evenings, it would largely be the scientific personnel together at the officers club or other places? Or there was a very limited mixing with say Dornberger and so forth?

Haeussermann: Frankly I cannot remember that we had in this respect any contacts. It was a time of war, and we had only our development in mind at that time, and parties and so on, I don't recall a single one.

Neufeld: So it was largely an academic atmosphere, would you say, in some ways?

Haeussermann: Definitely. Definitely, yes.

Neufeld: In the laboratories.

Haeussermann: Yes.

Neufeld: And to what extent were you involved at that point also in working with people in Darmstadt or other places?

Haeussermann: Well, I had several times, as you probably know from some reports, we had meetings with people who were looking into certain problems, like the development of accelerometers, like the development of frequency control converters and inverters. And it was at that time already that the professors in

Darmstadt, with their staff remaining there, could do some work. And so Dr. von Braun and others decided we should give them contracts. Since I had no confidence, after the war with Russia broke out, that this would lead to a good end, and often I had the desire, if possible, to return to Darmstadt, early '41 a lucky situation came up. Professor Vieweg, who was the head of the Institute of Applied Physics, Angewandte Physik, he had just from Peenemünde a fellow who had his training as a pilot in the air force, and this fellow had just finished his doctor's thesis, and so the problem was that he would be called in to the air force as a pilot again. And at this time, Dr. von Braun had lost the pilot he had in Peenemünde through an airplane accident. And so he took the opportunity and requested that this student of Professor Vieweg be assigned to Peenemünde as pilot.

Neufeld: What was his name? It's not important if you can't remember.

Haeussermann: Dr. Schneller died in 1944 in an airplane accident. And so Professor Vieweg requested that the work be continued, which was started for Peenemünde, and Wernher von Braun, knowing that I would like to go back to Darmstadt, gave me the opportunity to return to Darmstadt, of course now this time to the Institute of Applied Physics, not where I had been before, electrical machinery. And in addition, there were some problems on which I had been working in Peenemünde for the control system and simulation equipment, and so I got an additional contract on that subject, for the Institute of Applied Physics in Darmstadt. The upshot was, I moved back to Darmstadt under contract, working for Peenemünde, and this gave me then the opportunity to assess the problems and to show them the solutions I had for the tests they gave me, and this way I came back to Peenemünde and stayed in contact with Peenemünde.

Neufeld: Were you an officially transferred employee at that point?

Haeussermann: Yes.

Neufeld: So you became an employee of --

Haeussermann: --the Institute of Applied Physics.

Neufeld: As Assistent or ?

Haeussermann: No. Verbally translated it would mean a "coworker." Actually the name in German was Mitarbeiter. At the Institute. And very quickly, because of the development work I did there, I then had my own group, and also some company in Aschaffenburg working on the development I had going on.

Neufeld: Now, let's get some of the specifics here. Vieweg was already working on contract.

Haeussermann: Yes.

Neufeld: And what were his tasks or contracts before you got there, do you remember?

Haeussermann: I could not say what all the contracts were. Or what kind of contracts. He had--

Neufeld: General area?

Haeussermann: He had contracts in the field of plastics, and he had also in the field of approach sensors. This was a particular field. Then he had one on the 84 minute pendulum, which was at that time academically thought to be the ideal gyro, and work was going on that. And so he had various contracts in such fields.

Neufeld: Were you aware of all of the different institutes working at Darmstadt or did you not have much contact with other activities?

Haeussermann: Yes, I had some contact. For instance, the Institute of Applied Mathematics had done quite a lot of work with trajectory calculations.

Neufeld: That's Walther.

Haeussermann: Professor Walther, yes. Alwin Walther, and I had contact with them. Actually he became my co-referent on the thesis later, and he was particularly interested, he had done before the war already some work on analogue computers, and since I continued with analogue simulators for Peenemünde in this separate contract, Professor Walther became very much interested in that.

Neufeld: So you were officially under Vieweg.

Haeussermann: Right.

Neufeld: But you were also sort of working informally or more informally with Walther on simulation.

Haeussermann: Let's say, it was more that Walther stayed interested in my work. I had not, I didn't have to report at that time to him, but he observed what I was doing, and we had sometimes discussions on this. In the same way, Professor Buchhold, who was --and Professor Hueter, who had both also contracts from Peenemünde on accelerometers and on control of inverters, frequency control and control of inverters, and I knew

about that work, and so I was very well aware what's going on. In addition, very often we had meetings,--arranged from Peenemünde, either in Darmstadt or in Peenemünde, where I was invited to participate.

Neufeld: Now, in terms of the interaction between the universities and the Technische Hochschulen and Peenemünde, is it true that it was basically a very friendly academic atmosphere between equals?

Haeussermann: Oh yes. Oh yes. Evidently. Our goal was just to achieve higher performance and solve problems, and in this respect, there was maybe sometimes a little competition, but always on a very friendly and collegial level.

Neufeld: You mean a competition between labs at Peenemünde and labs at the university?

Haeussermann: No, even in Darmstadt. For instance, if I recall right, Professor Buchhold worked on integration methods and accelerometers, and in the same way did Professor Hueter. Both had proposals, and it was thought to be wise, because both could not assure from the beginning the desired accuracy, and they wanted to see, which one is more promising of these proposals, and so contracts were given out from Peenemünde in a competitive way but still, constrained to the original idea, and so the work was going on.

Neufeld: So you felt a sense of --

Haeussermann: A very healthy atmosphere, I think.

Neufeld: In the university you felt that there was a kind of friendly competition between different institutes as well as some interchange of ideas and interaction.

Haeussermann: Right.

Neufeld: Of course you had a very high density of people working on Peenemünde contracts there, compared to other institutions.

Haeussermann: Oh yes.

Neufeld: Maybe Dresden was the only one that came close.

Haeussermann: That's true.

Neufeld: I know of course that there were individual contracts at Berlin, at Stuttgart.

Haeussermann: Also, yes, but not to the same extent as at

Darmstadt.

Neufeld: You know, one thing that struck me the other day, when I was looking through the massive list of documents accessioned at Aberdeen, was the archive reports. It struck me as almost a kind of form of publication papers which might be a substitute for academic publication when everything is secret.

Haeussermann: Quite. Quite. Yes.

Neufeld: And that is indeed how it functioned, in the sense that people were able to say, look, I published --from an academic standpoint it's obviously, as I know from personal experience, important to say I've done this work and this is my paper or report --

Haeussermann: And it was of course also desired to disseminate among those who were within the group to work on problems, that they got familiar with this, and possibly criticize and generate different ideas. That was the intent.

Neufeld: But it struck me in that, however it evolved, that it was a very -- maybe I'm guessing here, so you can correct me if I'm wrong, but it struck me as an important instrument for getting academic scientists and engineers to be involved in those problems and so forth, because in effect they had an outlet for reporting their work, showing what they'd accomplished and getting interaction, in a scientific way that you couldn't do it in open publications.

Haeussermann: Not only this. For instance, in my own case, after I had done enough development work, my professor suggested to write this work of about one and a half years up in a thesis.

Neufeld: Right.

Haeussermann: And so I could get the PhD from this work. Of course the material remained classified. And still I had some personal benefit from this too. And so this was very challenging, to write it up and disseminate the information.

Neufeld: Did you defend during the war your dissertation, your thesis, did you finish it during the war?

Haeussermann: Yes.

Neufeld: And it was possible then, obviously I guess it was, to write a classified thesis, in effect.

Haeussermann: Oh yes.

Neufeld: You write a thesis that was kept secret effectively.

Haeussermann: Yes.

Neufeld: And could only be read by people within the university or at Peenemünde or a couple of qualified people?

Haeussermann: Well, others in this field.

Neufeld: Who had the appropriate security classification.

Haeussermann: Correct. Don't forget another point here. You wanted of course to be in some way protected, that you were generating something, that you were important, that you had contributed to the matter, and so you wanted to write up it all to have your position more or less strengthened. Because there was always the problem, people were needed at the front. And people who were not worthwhile to be kept and could not generate and contribute were --just thought, well, I'll live my own way and I'm protected because I'm in a classified project--they were not protected. Often people were pulled out. In my own case, after I returned to Darmstadt, I got one day my orders to come to a company and become a soldier. And only because just by that time, it was in fall '43, I think, no, it was earlier, it was in the summer '43, and by that time I had developed this pendulum simulator. And this was so important for Peenemünde that Dr. von Braun said that it is a must that I continue the development and the production and the checking and testing and calibration that I achieved, that I would not be pulled as a soldier into the army.

Neufeld: You had to maintain that so-called UK or draftexempt status?

Haeussermann: Right. Yes. My professor was the same way and of course, at the research institute, the professors always wanted to show how valuable their own contribution is, and so he one day asked me to present my simulators to the navy. They had quite some serious problems in the dynamics of torpedoes, and so I got an additional contract from there to use and apply my simulation equipment to their torpedo motions. And this further solidified the position I had.

Neufeld: So in terms of your position, I have to ask a specific question about that --at that time, did you feel that there was a serious chance that you were going to be drafted, or were you confident that you could get the backing of?

Haeussermann: You never could be confident.

Neufeld: So you were never sure that you might not go back into a

control what research is done at universities." But von Braun and so forth did not want to have the direct connection between you, the university contracts, and them disturbed by --but you weren't aware of any kind of battle like that?

Haeussermann: No, not at all, no. Even towards the end of the war, when people were drafted into the Volkssturm, you know the term?

Neufeld: Yes.

Haeussermann: We were never bothered in any way.

Neufeld: That's good. OK, now so far as the specifics of your research go, you went back on the simulation again of guidance and control and improving simulation, including Wasserfall, I guess.

Haeussermann: Right. This came somewhat later then. Since I was familiar with the problems in Peenemünde with the analogue computers, one day, I had been working on other equipment for directly for the rocket, for instance, what we called Tochterplatform, that means a slave platform following the basic platform to carry an accelerometer or so. And that was, for instance, when I went back to Darmstadt, my first assignment. And I developed that. And then just by a pure coincidence, I participated in a meeting and heard about problems in the simulation, with these test tables which were very inaccurate. Then from the knowledge I had, and the simulation of mathematical equations, I had the idea, why not do something like this here with the pendulum?

Neufeld: The pendulum to replace the tilt table.

Haeussermann: And even without my telling anybody, in our workshop, we had still so much freedom, I made a very simple setup of this, and took it, told my professor about it, what he didn't know and so I wanted to think about it yet, and then I said, "I have to show this to the people in Peenemünde." And then I brought this up to Peenemünde and showed it. It was immediately convincing that this is an ideal solution for that time. And so I got a contract on this for the Institute of Applied Physics, and from there on this was my main project. I had to find a company, in Aschaffenburg, that built, in the first series, about 50 of these components, and then of course I had to calibrate them. I had to check them out 100 percent, and had to maintain their performance then through the rest of the war, so that they could be used as calibration equipment for the control system.

Neufeld: So they were used to calibrate. Were these --

Haeussermann: The mixing computer?

Neufeld: Yes.

Haeussermann: And the servo system.

Neufeld: They were used at the manufacturers? Did you have them sent to Siemens and Kreiselgeraete?

Haeussermann: And Peenemünde. Actually they had engineers who took care of this, and they brought me the equipment, regularly to have it checked out and also to build new ones.

Neufeld: So in the production then of A-4, V -2, when it went into mass production, really at the end of '43 through '44, --

Haeussermann: I didn't even know where the simulators went, because this was again classified, and there was no need for me to know about this, because messengers brought the equipment and picked it up again.

Neufeld: So all you knew was that it was going to Berlin or Peenemünde.

Haeussermann: Some place, or maybe even this underground facility, I couldn't say, near Nordhausen?

Neufeld: It just appeared, the equipment appeared and disappeared.

Haeussermann: Right.

Neufeld: You had, you said, 50 in total built?

Haeussermann: Right.

Neufeld: So this was going to be a small scale production.

Haeussermann: For this equipment, it was a high number.

Neufeld: Yes, it's complex equipment that you're dealing with. This diagram of principle does not give the size of the equipment. Was it?

Haeussermann: I hope you can still see it here in the museum. They had it on display till about two years ago. It must be still somewhere. The pendulum had about this size.

Neufeld: That's about two feet.

Haeussermann: One and a half.

Neufeld: One and a half, a half a meter.

Haeussermann: Yes, right.

Neufeld: And the total equipment was about that high?

Haeussermann: Yes.

Neufeld: Half a meter or so, and about the same physical size --

Haeussermann: Or even narrower.

Neufeld: A foot or so.

Haeussermann: More than a foot. Yes.

Neufeld: So that was your main task, wouldn't you say?

Haeussermann: For Peenemünde.

Neufeld: Yes, for Peenemünde. And then you had the --

Haeussermann: For Gotenhafen, Torpedoversuchsanstalt. Torpedo test station. For that I had again a manufacturer. And towards the end of the war, I had a similar equipment, with less time constants, no mechanical, no electromechanical torquers to test the Wasserfall control system. See, Wasserfall worked with higher frequencies than A-4, and therefore you could not afford the time constant as an error contribution, to have in this equipment here. And so the torquer on this system had to be replaced.

Neufeld: You needed a quicker --

Haeussermann: -- faster response.

Neufeld: Faster response time.

Haeussermann: Yes.

Neufeld: Because you had a higher maneuverability of the missile.

Haeussermann: Yes, right.

Neufeld: Assumed, you had to have a more accurate maneuverability. When did you first become involved in Wasserfall guidance and control development? Do you remember?

Haeussermann: That was about summer of '44.

Neufeld: That's when they first asked you to become involved.

Haeussermann: When I proposed -- I knew about the problem, and I proposed this kind of test equipment. I think I mention it in here briefly too.

Neufeld: For the record, Dr. Haeussermann is speaking of his 1981 article, the title of which is "Developments in the Field of Automatic Guidance and Control of Rockets."

Haeussermann: I don't have a picture of it in here, but I think I mention it with some words in here. Yes.

Neufeld: You just mentioned the Wasserfall.

Haeussermann: I don't think I mention the Wasserfall in here.

Neufeld: Were you at all knowledgeable of the state of Wasserfall guidance and control development?

Haeussermann: No.

Neufeld: They were not filling you in, that was not your --

Haeussermann: --only the problems for analogue computer testing.

Neufeld: Because that's sort of a gap. It's hard to fill in on Wasserfall.

Haeussermann: I know. I found this myself later.

Neufeld: Maybe some of the literature will help. I'm also supposed to talk to Dr. Oswald Lange tomorrow, and Dahm.

Haeussermann: Werner Dahm.

Neufeld: Yes, tomorrow, about Wasserfall issues, but it seems that was the great problem with Wasserfall, was guidance and control. It was too complex, effectively, for the technology.

Haeussermann: At that time.

Neufeld: At the time. But I imagine the problem was largely one of radar development and so forth, control. The gyroscopic system of Wasserfall--or did you have any exposure to that at all?

Haeussermann: No, not at all. No.

Neufeld: Because I don't even know what system they were going --for attitude, just for attitude control alone, just what gyroscopic system they'd intended to use on Wasserfall. So then that would mean, for the last three years of the war you were in Darmstadt.

Haeussermann: In Darmstadt.

Neufeld: And you had to produce this, a large number of the pendulum system simulators.

Haeussermann: Correct.

Neufeld: For Peenemünde. You had to produce more of them for the Torpedoversuchsanstalt.

Haeussermann: Right. I also developed for that purpose, in contrast to this two degree of freedom simulator, or second order simulation of a mathematical equation, also I developed in addition single degree simulators, or one order, that means differentiating and integrating devices, which I used, had to use in the simulations for the Torpedoversuchsanstalt.

Neufeld: So you had the navy contracts and then you had Wasserfall development to do.

Haeussermann: Right.

Neufeld: And that was basically again improving simulation in order to adapt it to --

Haeussermann: -- for the functional requirements.

Neufeld: -- to a potential Wasserfall control system. And that went on until, through to the end of the war.

Haeussermann: Yes.

Neufeld: I imagine that you must have faced increasing difficulties there. I mean, that's in the western area that was heavily attacked.

Haeussermann: Especially after the destruction on the 11th of September of Darmstadt, where we were completely bombed out.

Neufeld: That was in '44.

Haeussermann: That was in '44, September, '44, and what I could save of my equipment and of work benches and so on were moved to the suburb where I lived, Jugenheim, and there in the school house I continued.

Neufeld: What was the name of the place?

Haeussermann: Jugenheim. It's about oh, I would say 12 kilometers south of Darmstadt.

Neufeld: So you had to improvise, essentially.

Haeussermann: Oh yes.

Neufeld: In a school. What was left.

TAPE 2, SIDE 1

Neufeld: You were saying about development of the mathematical equations of stability and description.

Haeussermann: At that time, we had no criteria yet to judge when a non-linear system is sufficiently stable, or to give limits for desired stability. We had our knowledge purely academically derived conditions to say when a linear system is stable, and when it is not stable, but we had not included in these mathematical considerations any nonlinearities which are connected with any practical system. Practical systems just don't behave linearly, as you usually use in your mathematics. We had to start from scratch to see what can be done to evaluate a system for its stability, and theoretical investigations were made; very cumbersome mathematical calculations were carried out, especially considering at that time we had very primitive calculators, and so our last resort was just to go into analogue computations and to simulate as closely as possible with analogue computers, still to be supported mathematically to find out what the performance of analogue computers is. Mathematics gives you ideal situations, which you don't find in reality, so our simulation equipment of course should, when in flight, behave almost ideally, but the control system which has the nonlinearities had to be tied in, into the mathematics of the flight motions, and this had to be resolved in a fashion in the laboratory so that you could repeat on a one to one time scale your flight performance and evaluate it, and determine what are the limits for your adjustments and gain factors in order to achieve the desired flight results.

Neufeld: So you would consider that to be one of the most important problems at Peenemünde?

Haeussermann: This was one of the problems we had to attack, and in my opinion we solved them satisfactorily for the requirements of the A-4.

Neufeld: And that would have been an accomplishment unique in the world.

Haeussermann: For that time, I think so, yes.

Neufeld: In terms of stability calculations, for such a demanding situation.

Haeussermann: Yes.

Neufeld: What other things in Peenemünde's technological development did you consider to be really groundbreaking in guidance and control?

Haeussermann: I think you know that we worked with various methods to determine the acceleration of a missile. We had accelerometers which had to be --in which the output had to be indicated, for instance, for thrust cutoff for A-4--and these problems were solved; this indication was solved with a chemical call, also developed in Darmstadt by Professor Wagner, in close connection with Professor Buchhold. And there were many developments like this, which were very worthwhile at that time. They are not any more in use today because we have better methods, of course.

Neufeld: Of the accelerometer systems that were developed, were you able to judge which were the best, or did you only have exposure later on in the war when --

Haeussermann: --this exposure came out only later, and actually in the States after we learned about the performance. In Darmstadt we had no access to flight results, so I didn't learn anything about this at that time.

Neufeld: You didn't really know about Mueller's integrating gyro accelerometer?

Haeussermann: I knew the principle, but I did not know what the performance was that has been achieved in flight.

Neufeld: Do you know which performed better from later knowledge in the United States?

Haeussermann: Well, of course in the United States we had to continue with the development of three axis gimbal platform because only this platform could give you the high performance results as requested by the Army. You probably know, I have described it in this report here, the first Explorer still used a Siemens inertial system, without a three axis gymbal systems, because it was available, in surplus. It was not as expensive as a stabilized platform, and it could do the job satisfactorily, to go into orbit.

Neufeld: That's interesting. You used an old German system from the Second World War?

Haeussermann: Yes. I could not say for sure whether it was a one to one copy built here. It was definitely the same scheme which was used in the A- 4 from Siemens-Halske.

Neufeld: So you did not use the platform that was developed for the Redstone missile version?

Haeussermann: No.

Neufeld: Why was that? Was that an expense reason?

Haeussermann: It was an expense reason mainly, and it was also that saved a little weight. It was of course important at that time to save weight as much as possible, to reach orbit.

Neufeld: So the Jupiter C version of Redstone had a very small payload into orbit.

Haeussermann: Right.

Neufeld: So you wanted to gain whatever --

Haeussermann: --whatever you could gain, you tried to do.

Neufeld: We should go on and at least make a brief discussion of your time in the United States and so forth, insofar as you're willing to continue in your time here. At some point in early '45 you were overrun by the American forces, so in March or April '45 --

Haeussermann: Yes, right.

Neufeld: And you were contacted at some point by American intelligence?

Haeussermann: Oh, yes. Yes. I think it was the famous Dr. von Neumann who had been working on interrogation here, he was in the interrogating team, and he wanted to know what I had achieved and done. I got an offer in May '45 to join Wernher von Braun to come to the States, and I was amazed by the way, this was voluntary, that we came over or not. And I'm the best proof, because at that time I had to decline. My wife was in such poor health that I didn't want to leave her alone. So I said I would like to come to the States but first my wife has to be in a better shape physically, she was quite a lot of time in the hospital at that time, mainly because of starvation. I got another offer in '47, and then I was able to accept, and I gladly accepted.

Neufeld: For those two years how were you living? Did you get any salary from the university? It still existed in some form?.... So you lived in Darmstadt for two years. You managed to get by. And then you were once again offered a position. It's interesting, when you look at what von Braun chose people to come over now, that you know often his choices or choices of other people who were cooperating with him were scattered all over. You know,

everyone concentrates on the group that went to Bavaria and was captured in Bavaria including von Braun and Dornberger and others, as if that was all there was, but it's apparent that he must have been looking at lists of people all over Germany and saying, one of those, one of those.

Haeussermann: Yes, I think that was the case. And this way von Braun or Dr. Steinhoff recommended that I be contacted, and that's when I was interrogated about the work that I had done. It was very interesting that the interrogators asked me also other questions about the A -4, where I had not a clear information how it worked by that time, how the control system performed.

Neufeld: You're talking immediately after the war.

Haeussermann: Correct, yes. I think it was about June or July in '45.

Neufeld: Interrogation teams came through. So they were still pretty unclear as to the different control systems and so forth.

Haeussermann: Right.

Neufeld: So your trip to the United States didn't come until the middle of '47.

Haeussermann: December, '47.

Neufeld: Not till the very end of 1947.

Haeussermann: By the way, I also lectured at that time especially in '46 and '47 at the technical college in Darmstadt.

Neufeld: OK, so there was some kind of university sort of existing?

Haeussermann: It was a somewhat lower grade. We had technical colleges and we had the Institute of Technology, and at that time there was quite a demand to give lectures at the college of technology, Darmstadt.

Neufeld: What was the name of it? Do you remember?

Haeussermann: Ingenieurschule Darmstadt.

Neufeld: So you came over then. By the time you got to Fort Bliss, it was almost the beginning of 1948.

Haeussermann: Yes, it was January '48 when I arrived.

Neufeld: When you went over, were you consciously coming as an

immigrant, that is, with a feeling that you were going to stay in the United States and move here, or you didn't know?

Haeussermann: The agreement was that I had a commitment for half a year. During this time, the commitment was mutual, I could withdraw or the United States could tell me, we don't need you any longer. And in addition was an agreement that if I accept, then my wife could join me, everything would be done that she would join me as soon as possible. So of course I accepted gladly. My wife joined me then in September, '48 in Fort Bliss.

Neufeld: But your expectations when you went were that it would be permanent or wouldn't be permanent or you'd just wait to see?

Haeussermann: For me, it was clear that I could stay. Yes. And I wanted to stay.

Neufeld: Was that because you were interested in the opportunity, working in rocketry, or --

Haeussermann: --yes--

Neufeld:--you were concerned about Germany's condition or the life or --

Haeussermann: --no, I think the main thing was, I wanted to continue in this profession, which was for me very challenging, and I didn't see any future in this respect of course in Germany, nor anywhere in Europe at that time. By the way, the French contacted me immediately after they learned that I had an interrogation for going to the States in Wiesbaden, how they learned about it I don't know. I got a phone call from the French who wanted me to join them. Several of the Peenemünde group actually went to France.

Neufeld: Your reaction when the French?

Haeussermann: I said, "Sorry, I have already made my mind up; I want to go to the States."

Neufeld: I suppose part of it always was, it seems to me, that the Germans had a better opinion of the Americans than of the French.

Haeussermann: Well, it was the land of the future.

Neufeld: So that image was still pretty strong.

Haeussermann: Yes. In my opinion, if anything can be done in the future with rocketry, then it would be in the States.

Neufeld: Yes. Was there an influence from the 1920's, early in your youth, or can you say when it was?

Haeussermann: For the Germans, as far as I knew at that time, the general opinion was that the United States is the land of the future.

Neufeld: So you came to Fort Bliss at the beginning of '48. A number of people had been there for three years, or a bit over two years.

Haeussermann: Yes. The only disappointing matter, that there was practically no contact during this time with them, and that made me a little hesitant, causing a little delay. I wanted to have first some information from them. And then Dr. Hoelzer and Dr. Steinhoff made a phone call to Germany, which as in that time in '47 almost unheard-of, and I spoke to them and got the answers I wanted to have from them on my questions, and then I made my final decision to come to the States.

Neufeld: What could they tell you?

Haeussermann: Well, I mainly asked about the development possibilities here, what they had been doing, and whether there is in their opinion a future to continue with the rocket work.

Neufeld: So they gave you the impression that they thought there was a future?

Haeussermann: Yes.

Neufeld: Because I know that there was some disappointment in the Fort Bliss period with the fact that not enough was going on.

Haeussermann: Well, the disappointment was, in my opinion and for me also at the beginning, that we had extremely primitive laboratories. See, we had practically to start from scratch, to build up even power distribution panels in the barracks and such things. Some of our fellows had to go underneath, where the black widows were, to pull the cables through. And such things happened. And of course, this was a kind of disappointment. You couldn't right away start with what you had in mind. It was a long road. But we made it, and especially one thing was nice, especially for Dr. Mueller, that ideas brought from Germany for future research could be pursued.

Neufeld: You mean in the laboratories at Fort Bliss?

Haeussermann: Yes, and in the workshops.

Neufeld: When you finally managed to accumulate enough equipment

and so forth.

Haeussermann: Right. And so far I was in a better situation than those who arrived two years earlier, because I found already a certain basis on which I could start.

Neufeld: So you immediately picked up where you had left off.

Haeussermann: Right. Yes.

Neufeld: With the simulation and so forth. At this point I'm kind of wondering just how much we can cover, it could take hours to do another thirty years in the United States, which is hardly realistic in terms of covering the whole career, but in a summary way, then, you were moved to Huntsville in 1950, and did you notice an immediate change at that point in the amount of work that you had to do?

Haeussermann: Different, because at that time we had a firm goal. In Fort Bliss we still worked to get the V-2 started again, to have scientific equipment on those V-2's and such things. To improve the guidance system for a project which was called the Hermes II and on this I worked and this was challenging. This was always on a research basis, a high priority. And then we came to here, and it became immediately an urgent project because then the Redstone missile was given to us, as a firm assignment, which had to be produced within a certain time.

Neufeld: The Hermes II was an upper stage for ?

Haeussermann: Let's say a modified V-2, some higher performance, and so especially it had some aerodynamic unfavorable features, for other reasons, which had to be controlled, which had to be taken by the control system, and so the control system and the guidance system had to be improved in comparison to the original V-2 or A-4. This was challenging.

Neufeld: This was different. I noted there was also, essentially Wasserfall was picked up again as a Hermes project.

Haeussermann: Yes. I had nothing to do with that.

Neufeld: Nothing to do with that at all. And the theoretical investigation that you made for Hermes II carried over into the Redstone guidance system?

Haeussermann: Yes.

Neufeld: You were able to begin applying those --

Haeussermann: --oh yes --

Neufeld: --in a concrete way. I'm trying to think of where I should stop here, that's the problem. To go through all of Redstone, Jupiter, Pershing, Saturn would take far too long.

Haeussermann: Oh yes. And it's not the subject of your present work?

Neufeld: Yes. I think in some ways, the problem is that I would really like to do this, but I need to prepare to do that kind of interview, as much as I need to prepare to do Peenemünde, which means a lot of research which has to be in the future.

Haeussermann: Right, yes. I have covered this also in this brochure here. You might, if you want to, come back and have your questions prepared with the help of this.

Neufeld: That would be a long term research project, because it would take me a few years just to do Peenemünde, but I don't want to wait far too long in order to do the oral history for the Redstsone period. Let me just ask you a couple of organizational questions. When you came to Fort Bliss, did you find a sort of laboratory organization that was modeled on Peenemünde?

Haeussermann: In a certain respect, yes. But since we did not have in other fields the same capability here, there were some --well, some concentrations in specific areas. And just like the guidance and control area, it was clear that there considerable improvement has to be made. The engine development was important, for more powerful motors, and such things, so work was started in these areas, yes.

Neufeld: I'm trying to figure out just how much the organization was sort of picked up and reconstructed more or less identically in the United States, and how much it had to be modified or changed. So you feel that pretty much the same skeleton structure, at least --?

Haeussermann: -- yes. Right.

Neufeld: Was reproduced in the United States, and then carried into the Redstone period.

Haeussermann: Right.

Neufeld: And you felt that it fitted very well within US Army arsenal system?

Haeussermann: Oh, definitely, yes.

Neufeld: So you had no problems in that sense reproducing Peenemünde's essential philosophy.

Haeussermann: Right. And it was more or less, I use the term, an organically grown organization. For instance, BSM, which meant, you had devices on board, the control system, here, Steuerung, that means steering, Messtechnik, was measuring what you need in order to evaluate the flight path performance, and not used in this abbreviation was the total electrical onboard equipment, which was actually included onboard equipment, and so all this was retained in the laboratory, headed by Dr. Steinhoff, later by Professor Buchhold, who succeeded him. I took it over in 1954 from Professor Buchhold. And we maintained this organization throughout the Apollo program.

Neufeld: And the definition of the limits as to what was included in your department, onboard electrical equipment, guidance and control and measurement, the definition of what that included more or less stayed the same throughout.

Haeussermann: Right.

Neufeld: That was part of the organizational philosophy. Was that re-establishment of that central organization, was that von Braun's idea in particular, or can you really say that it's one man's concept?

Haeussermann: I think, this happened before my time, I think, when Dr. Steinhoff started in Peenemünde he proposed that these things belong together. And from the viewpoint of closely related things, they belong together. Another thing was, Wernher von Braun always was anxious to see that you have a good communication between related fields. As he said, nothing should fall between the cracks. And this is a philosophy that Wernher von Braun maintained throughout the time he has been responsible, that you do not see your limits somewhere and say, "Up to here I work on this problem, and beyond that, I don't care, that is the problem of somebody else." He wanted to have an overlapping, so that both sides knew what is going on, and he made both responsible for this, and said, "This has to work so you straighten out your problems here." That was a very healthy situation. I mean, this was one of the reasons why he maintained the organization that way, because it had proved to be all right for other developments in earlier years.

Neufeld: So that philosophy was obvious from the beginning when you came to Peenemünde, that von Braun was insisting on sort of cross-communication, horizontally between different departments.

Haeussermann: Yes. Yes. Right.

Neufeld: That he insisted that you talk to each other across the laboratories, as opposed to going up through channels where there might be a superior over both.

Haeussermann: Yes, he took it so far that you could feel free to watch what was going on in other laboratories. You could get the information, what is going on there, especially in these fringe areas where you had to draw conclusions for your own work. This continued throughout the following years.

Neufeld: At Peenemünde and later you felt free to walk into somebody else's laboratory, did you?

Haeussermann: Yes.

Neufeld: Often without calling them up in advance.

Haeussermann: Without going to the boss and saying, "Can I get that information?"

Neufeld: So you always felt free to do that, in a sense tread on someone else's turf.

Haeussermann: Right, but limited to obtaining information or clarification.

Neufeld: And that remained true throughout.

Haeussermann: Oh yes.

Neufeld: Through Redstone, through Marshall Space Flight Center --

Haeussermann: And the Apollo program.

Neufeld: You always tried to --and in terms of solving problems then, you, from the management standpoint, you had committees, formally or informally?

Haeussermann: Yes, Wernher von Braun had throughout the years here in Huntsville the habit to call every week for a staff meeting, and usually the directors were included, and the directors had the freedom to bring specialists for them, when they wanted to present a special situation or problem.

Neufeld: That's communication at the top and certainly that's important.

Haeussermann: Yes.

Neufeld: But also horizontally between departments, without going through the staff meeting at the level at the top of the scale.

Haeussermann: Oh, of course. Then again we had our meetings, for instance, between the aeroballistics laboratory, Dr. Geissler,

and my laboratory, and we discussed then our problems, described to each other how our solutions are in such a way, and felt free to tell our members in the laboratories to go to each other, without formal request, to discuss the problems. And only when they had discrepancies, when the next higher echelon had to be involved for a decision, only then they came up and said, "Here we have a problem."

Neufeld: This may be hard to assess, but do you think that was first and foremost sort of intercommunication, emphasis on constant cross-communication and not going through channels necessarily, do you think that was von Braun's unique contribution?

Haeussermann: One of his unique contributions.

Neufeld: That in effect, if he hadn't been there, it might not have worked that way.

Haeussermann: Right.

Neufeld: Because that's interesting. I don't want to stray into the territory of saying von Braun did this, von Braun did that, that he did everything himself, he was the sole conceiver of the management system and how things operated, so it's hard to --you have to judge sort of how much does one person contribute and how much does another.

Haeussermann: Well, let's say, because the education we all got through him, and we found he was right and pursued it. That's how I would like to say it.

Neufeld: Yes, certainly you learned and contributed.

Haeussermann: And this gave him also the freedom to go more in depth than usually a man in his position could do, because he knew it's taking care of the problems and he wanted to see then and inform himself that all the problems are properly solved.

Neufeld: What you're saying is, he relieved himself of some burden by not having to arbitrate every conflict himself.

Haeussermann: Right.

Neufeld: By expecting others to do that.

Haeussermann: Quite.

Neufeld: Now, the other side of this is, the other side of the Peenemünde philosophy that seems to be transferred is this kind of arsenal concept, or big in-house development capability,

laboratories large enough to build things yourself, without having to do that.

Haeussermann: At least as a prototype.

Neufeld: Yes, as prototypes that you could show then to industrial management. It's clear that that's carried over into the United States, and through a fortunate series of circumstances that fitted into the US Army arsenal concept.

Haeussermann: We were lucky that our superiors in the Army agreed on these matters, because it was basically not a concept here, especially not in the Air Force.

Neufeld: And NASA took the Air Force philosophy largely as its philosophy, at least from Headquarters.

Haeussermann: Yes. See, the new point, why we were so insistent to handle the situation in this way, was, we wanted to give our own engineers the capability to learn first hand the problems, and to be able later in manufacturing to give advice. If you have an organization, as it has now been reverted today to quite an extent, those people are just managers, have to believe what they are told, don't have any more the capability to check out whether what they are told is correct or not. And so they have just to yield to the contractor. And it's very unfortunate and very costly, because for any modification proposed, there's almost no limit what the contractor will ask for.

Neufeld: In terms of money.

Haeussermann: Yes, quite. And time. Another point, why this in-house capability helped so much, as long as you are in R and D phase, generating something in a short time, you always see some shortcomings, some improvements that can be made. If you're limited by an existing R&D contract, and the contractor makes some unsatisfactory modification, you pay enormously for it. We have done it very often in such a way here that in our own workshops, we made the modification and changed the system to as higher performance, eliminated shortcomings which should have been foreseen from the beginning, but for time and other reasons it didn't work out this way. And so you are much more flexible if you have such an in-house capability.

Neufeld: Now, of course it strikes me, part of that is that when you have to send the specifications to the contractor and ask them to do something, you have to generate, especially in the current system, such an enormous amount of paperwork to accomplish something that you used to be able to just go into the laboratory and with little or no paperwork do by yourself.

Haeussermann: Right.

Neufeld: This becomes, I think this is becoming a real downfall of the Pentagon way of doing things, and NASA has to some extent adopted the same philosophy.

Haeussermann: One of my first disappointments with industry here was the following. In the first year I was here in Fort Bliss, the need came up to have a fast response servomotor, electromechanical servomotor. We had a special development. I was familiar with this already from Germany with it, how to build such a fast response motor, and we developed it here in our own laboratory. I tried to have it manufactured for the Redstone missile. The R&D development required about 200 servomotors. I went to General Electric, as recommended by our officers, to have these motors built. When they heard about the quantity they just rejected and said no, they were not interested. Then next step we went to the guidance and control company Ford Instrument Company in Long Island, New York.

Neufeld: Ford?

Haeussermann: The Ford Instrument Company had nothing to do with the Ford Motor Company. And they got a contract to build for the Redstone the whole guidance system, according to our specifications. I gave them the drawings for the motor, and requested that when they have copied the drawings and before they start to build the motor, they come back to discuss it with me once more. When I saw the new drawings, it was a different motor. Couldn't fulfill the requirements. It was just modified without knowledge, what was the important aspects of our modifications. I had to demand they copy what I had given them, and that remained from then on one of our clear statements with a new company, later also with Bendix: You have to agree that you copy first what we give you. We will be open-minded when you have something better, but first you learn why we are doing it, and then we discuss it, and definitely Bendix had such highly qualified people that we agreed on a lot of items to change something.

Neufeld: Bendix became the main contractor for guidance systems.

Haeussermann: On Pershing and on parts of the Apollo project.

Neufeld: I want to backtrack on the relationship between industry corporations and the center, to the Peenemünde period, and the sort of development of this in-house philosophy. As far as you could observe it, just having come in at sort of ground floor of BSM, you built up your own laboratory because of what?

Haeussermann: To obtain for yourself experience, and to be able to test what you propose should go as equipment into the rocket.

Neufeld: Was it with a clear sense that no existing company could do what you needed to do?

Haeussermann: Not only this, but you could not write out the specifications at that time so clearly that you had specified what has to be done. So you had, because the development made such fast progress, you had to be as flexible as possible, and since industry was at that time learning as much as we had, it was new land in which to work. We said, it's better to do it ourselves. Let's have industry participate in it, just like in the case of Kreiselgeraete, where they had their own test equipment, but as soon as we had a better one, they had to accept ours. And so on. And we accepted from them what they had, of course.

Neufeld: Yes, it's interesting to see how that emerged, the philosophy of large in-house capability.

Haeussermann: Yes.

Neufeld: A kind of German equivalent of the arsenal model that seemingly emerged over time. Somebody didn't just say, "That's the way we're going to do things." I'm trying to --at least there's a number of factors.

Haeussermann: See, we had to integrate the whole system. So that everything worked well together. You had to give, at that time, to a company everything. To parcel out certain developments could only be done on a research basis, such as to see what will bring you the better performance, of an accelerometer or so. But to go all the way and say, build the whole system, this had to be retained in Peenemünde. And this has been the scheme I think we successfully pursued in later years too.

Neufeld: Yes. It strikes me about BSM's evolution that Kreiselgeraete had been given the task earlier with A-3 of developing a complete guidance system, and it was too overwhelming.

Haeussermann: Right.

Neufeld: I don't think it was their failure in the sense that they were not competent. It was, the task was too complex for the existing technology. So in effect you had to go then to build an organization in-house in order to have --

Haeussermann: I want you to understand that Kreiselgeraete had done control work for the navy, where weight was unimportant, power was almost unimportant. You could build as heavy as you

wanted to. And the main thing was, it works. This was a completely different subject, when you had to go into a missile, or into a spacecraft, because then you had to limit your performance in such a way that minimum weight and still the minimum, the absolute necessary performance. Over-design was not permitted. And so the companies had to learn this first.

Neufeld: Because it's interesting, it shows, I think, that you would have been happy to give a contract to a company to do something if they could just do what you wanted them to do. But since you couldn't either define your requirements exactly enough to specify it, or couldn't find the equipment in industry --

Haeussermann: As being responsible for the total system, you had to make very often compromises, and this is what you had to work out yourself.

Neufeld: Yes. I think that's important, because you see, part of what I'm doing here is, I'm trying to figure out, in the context, in this specific context of Germany and the specific context of the Third Reich, what the role of the government was versus the role of industry or companies, you know, and what attitudes there were. Certainly you don't have in Germany as big an opposition to having state control or state companies as you do in this country. But it seems in the case of Peenemünde, mostly it's just, you're driven by the demands of the moment, of producing.

Haeussermann: Exactly. Don't forget we were under tremendous time pressure. Very often we had indications that the whole project, during the war, could get killed and terminated. So we had to show as quickly as possible that we achieved a certain goal. And that the steps which we had said can make the development had been fulfilled.

Neufeld: The A-4 was a feasible military project that could be delivered in a couple of years, something like that.

Haeussermann: Yes.

Neufeld: So that not, since you built a larger organization at Peenemünde out of sheer necessity for getting done what you had to do in a short period of time. I think that's important. It shows that there wasn't much of an ideological issue here regarding what should be the role of the state in the Third Reich versus the role of --

Haeussermann: No, not at all...the situation was forced by the circumstances.

Neufeld: You know, personally I think that Dornberger also had a role earlier on in establishing that they'd build a big facility

and do some things there. But there's more development that was needed, of the laboratories and so forth.

Haeussermann: Yes. Right.

Neufeld: OK, well, that was very informative and interesting, I think of great value to me.

Haeussermann: I guess you do not find many who have been at the same time, like I have, at another institution and observed it from there.

Neufeld: Yes. So far it seems that I've had little contact with somebody who spent a long time at the universities. Everyone I've interviewed came to Peenemünde, either had been a student and had come fairly directly, or just through personal connections, you know, that Steinhoff knew somebody who knew somebody that brought --brought them, as opposed to actually having worked inside the university environment.

Haeussermann: Right. Yes. Actually, how the people were selected was very often through recommendation. They wanted to have somebody with specific capability, and at the same time with some hope that he would achieve what you expected to be done when necessary.

Neufeld: So people were sort of sitting around saying, "Do you remember so and so, where did he go?"

Haeussermann: "How would he fit in here?" and so on.

Neufeld: And "where is he employed now and how can we get him."

Haeussermann: Right.

Neufeld: Yes, well, that's certainly what it appears to me to a current term, networking.

Haeussermann: Yes.

Neufeld: Is how the people were drawn in to the laboratory organizations at Peenemünde.

Haeussermann: Right.

Neufeld: OK, very good, thank you. ....

Haeussermann: OK.