798

INTERVIEW WITH DR. WALTER HAEUSSERMANN
INTERVIEWED BY STEPHEN WARING
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1. Waring: . . . May 28, 1992. I'm speaking with Dr. Haeussermann. We're going to talk about the Lunar Rover and the ATM among other things. Let's just start out with the Rover. Could you describe the origins of the Lunar Roving Vehicle, particularly how Marshall would have gotten involved in a manned vehicle like that? Through the early history of NASA that had been Houston's responsibility.

2. Haeussermann: I could not trace back who actually had the first idea about this, but [?8] we discussed our conceptions to move here from place to place. The Lunar Roving Vehicle I think was conceived relatively early, much earlier than the actual design saw it. You see residents in the Space Museum. Quite some different types of vehicles had been conceived. Finally, we solved it. Finally we know the Lunar Roving Vehicle only had to be designed then under the conditions that it was possible to bring this mass to the moon, to bring back several satisfactory note of first [16?] and so on. This was solved in compromises in several respects: compromises with distance, manned super vehicle, operation of time, and lunar distance. I have described all this very completely in this report which I have for you here. This report will give you some more information in detail.

3. Waring: When you say it was conceived early, it seems to me that one of the earliest expressions at Marshall was in 1962 when Von Braun proposed a Lunar Logistical Support System. One possibility we have encountered is that Marshall's involvement in a manned vehicle like that was . . . this was sort of a bonus given to

the Center as a result for going along with the Lunar Orbital Rendezvous Mode. Do you recall anything about that?

4. Haeussermann: I could not say [30?] was technical situation or not. All I can say is we had the capability more than Marshall Space Flight Center to make the total design, to review it for a contractor.

5. Waring: As I say that story is a little fuzzy, and we are not quite sure about it. How . . .?

6. Haeussermann: [35?]

7. Waring: How much of the Lunar Roving Vehicle that ended up being built, that particular design, how much of that design came from in-house and how much was through the contractors?

8. Haeussermann: I would say at least 90 percent came from in-house. We had to agree to show the changes by the contractor in all [39?], reasonable cost figures and in order also to develop the system entirely.

9. Waring: Was the Lunar Roving Vehicle primarily designed in your laboratory, or was there a special task team put together that put the design together? Who would say in terms of the labs or individuals were most involved?

10. Haeussermann: My laboratory had a major part of it as you will see from this report. We had responsibility for the controlling, for the steeling, and for the whole electronic system. The mechanical part was to a wide extent left to the contractor.

The wheels, the configuration of the wheels, were done in common work for the Space Science Laboratory in order to find out what is an optimum configuration for the mesh wire. The Space Science Laboratory, in cooperation with other laboratories, did a lot of work to find out the friction coefficents and the necessary coverage of the mesh wire.

11. Waring: So it would not slip or sink?

12. Haeussermann: Right.

13. Waring: Could you in general fashion talk about some of the major technical challenges that had to be overcome to build a Lunar Rover?

14. Haeussermann: I do think it was a relatively straight forward design with compromises. You could not go with the newest technological advance for cost and other reasons, weight reasons. Once it would have been nice. This probably will be done in the future, to have a track motor which does not use brushes. It does the switching by semi-conductors. We could not do this because development time did not allow it.

15. Waring: Because they had about 17 months or so?

16. Haeussermann: Right. We had to use as far as possible components that were available or available with minor modifications.

17. Waring: The Lunar Roving Vehicle had a very rushed schedule. Can you think of some other ways, other sorts of compromises that had to be made because of that rushed schedule?

18. Haeussermann: I would not right now. The test schedule of course was forcing on us that we develop, as quickly as possible, and have models of which we could pull the system of work on the lunar surface. That gave us additional pressure to develop the system very quickly and to test it.

19. Waring: Were some of the previous studies that Marshall had done about lunar vehicles, for instance the study of the Mobile Laboratory, the MOLAB, were those studies helpful in designing the Lunar Roving Vehicle or was that really for a very different sort of vehicle, heavier?

20. Haeussermann: I do not think it was any help in this respect.

21. Waring: Because the MOLAB was a very heavy vehicle?

22. Haeussermann: Right.

23. Waring: In the summer of 1970, the Roving Vehicle ran into some contractual problems, problems with General Motors and its Delco division. The contractor got behind schedule.

24. Haeussermann: Right. This was one of our problems at that time. That is why we had to end [84-85?]. We had to use therefore components which were more or less available.

25. Waring: Was it in part that General Motors and its Delco division were inexperienced in building space hardware or do you recall?

26. Haeussermann: No, I do not think . . . they performed very well in this respect, but obviously their cost.

27. Waring: The cost went up.

28. Haeussermann: Much higher, unexpected.

29. Waring: In working on the Roving Vehicle, Marshall for the first time had perhaps rather more intimate relations with Houston and the astronauts. It seems to me in reading that during the Saturn era, the division of labor was pretty clear cut. Marshall built the rocket. Houston built the command and service module, and they sat on the top of the rockets. With the Lunar Roving Vehicle, there were in effect two centers working on a manned vehicle. Did that cause any friction?

30. Haeussermann: I do not recall any friction. I recall very well that we wanted to please the astronauts in every respect. An example is the [101?] the hand controller, the sector to the hands of the driving astronaut so that he had it as convienent and as elegant to work with that nothing should disturb him in the operation and be accepted, all of the systems for a smooth operations of the vehicle.

31. Waring: Was there always an astronaut or a team of astronauts who were on some of the design teams?

32. Haeussermann: Oh yes. We had regular reviews before a design went fully through. We discussed the matter with them. Not only this, we made models like on the hand controller for instance and as soon check it out, compile its convenience for the operation.

33. Waring: Were there other ways in which there was cooperation between Houston and Marshall on the design of the Rover? Did Houston engineers play any significant role?

34. Haeussermann: Not that I recall.

35. Waring: It was primarily the astronauts?

36. Haeussermann: I think yes.

37. Waring: Can you think of some ways in which it was different or the same working on the Roving Vehicle as opposed to working on the Saturn rockets?

38. Haeussermann: No. I would say it went in the same way as [118?]. They were willing to see us about the work and just wanted to do the best for a reliability you would find and the safety you would find for the astronauts.

39. Waring: Were there any differences in management or organization working on . . . ?

40. Haeussermann: No. We had done it directly our Marshall Space Flight Center administer, also in the reviews of Boeing clients or with Delco. I had been there

very often at Delco and also at Boeing to head our reviews. Also our quality continues to improve the manufacturing of the component.

41. Waring: It was a very smooth transition? Another engineering project using the same methods?

42. Haeussermann: Yes, it was just the same method carried out as other things on Saturn V.

43. Waring: Very good. Another thing that was a little unusual about the Lunar Rover as opposed to perhaps the Saturn era was that Marshall maintained an involvement in mission operations longer. Marshall was involved in flight operations during the launch obviously, but with the Lunar Roving Vehicle they maintained an involvement with operations through the manned, well the lunar phase of the mission.

44. Haeussermann: Through the lunar phase because we wanted to give advice if any problem would come up. For instance, we were not at in the beginning because we had sometimes, in the very early stage here on earth, failures to unfold the vehicle out after it [138?]. We wanted really to be sure if anything would not work properly can give the necessary advice. That was the main reason [141?]. Also during the operation, if any information would have shown up that the instrumentation, distance, velocity, and covering the [144?] would not be correct, we wanted to be involved because we had the engineers to right away give information and to some extent also to simulate the situation here in our laboratories.

45. Waring: In what ways were simulations done during the missions? Can you think of some examples or could you describe the system that was set up for that?

46. Haeussermann: I do not remember that it was necessary. We had here all the components put together, actually same as the Lunar Roving Vehicle and would have been able to check this out. You might know that we had Lunar Roving Vehicle simulator that completely simulated the terain on which the astronauts had to drive. We went through all these exercises here, and depending on what would have gone wrong, we would have of our system to simulate.

47. Waring: Very good. Did the people at Houston welcome this role on the part of Marshall's involvement?

48. Haeussermann: I never found any contradictions. There are slight indications of friction.

49. Waring: Very good.

50. Haeussermann: Actually we were invited afterwards at debriefings of the astronauts at Houston and participated like the people from Houston themselves.

51. Waring: That's interesting. I think that is about all my questions on the Roving Vehicle. Perhaps a general question to conclude this part. What would you say were some of the main lessons from the Roving Vehicle? What perhaps was its greatest legacy to the Center or to NASA?

52. Haeussermann: First of all, it was very satisfactory that the system worked as predicted. Actually, we believe if you were to go up today to the moon and just replace the batteries you can use the system!

53. Waring: Because it is not going to deteriorate up there, that's right.

54. Haeussermann: I would not know if it actually deteriorated in the meantime, except the batteries.

55. Waring: A lot of people have told me that working on the Roving Vehicle was a very pleasurable project.

56. Haeussermann: Yes, in every respect.

57. Waring: Why do you think people have such fond memories of that?

58. Haeussermann: Well, because it was something different. It was challenging to develop the components for it, completely different from a missile or from a space vehicle. It was challenging in this respect to do something different with a higher performance we wanted.

59. Waring: Very good. Well, let us shift gears if we can and talk about the Skylab and Telescope Mount.

60. Haeussermann: May I interfere?

61. Waring: Sure.

62. Haeussermann: This might be of interest to you. When we got the green light to develop the Lunar Roving Vehicle, many specifications were absolutely open: what speed should we design it for, what should be the biggest slope to handle it, the crevices, the caps, and so on. All this, we have assumptions or based on assumptions, and information we had from early visits [193?]. There was still some quite some uncertainty whether we could cover the large distances which actually a round trip was figured out should be about 30 kilometers. We had to make a lot of assumptions. This was the demand of my laboratory and ideally was [190?]. We have got some knowledge there. We have got our information from other laboratories like Space Science Laboratory, from Houston. Based on this we designed then our [201?] specifications. I just say afterwards, our assumptions were absolutely right. We did not have to make any changes on these pure assumptions. It was a straight forward development in this respect.

63. Waring: Right. That is very interesting. It was not like building a car. It was building a vehicle for a whole different environment.

64. Haeussermann: Exactly. We tried, for instance, to keep the center of mass as low as possible because if the astronauts would run too fast into a right or left turn, the vehicle could tilt over. They would have to reerect it, and we know that this was once very close to have one astronaut [213?].

65. Waring: Did your laboratory do any preliminary design studies on a one-manned-flyer for use?

66. Haeussermann: No.

67. Waring: I know that was another alternative to building the Lunar Rover. Very good.

68. Haeussermann: You find many more details in this report and you might have more questions afterwards.

69. Waring: OK. I might call you back after that. There are some good things I have gotten from Sonny Morea, the project manager, but a lot of his stuff is more organizational rather than technical.

70. Haeussermann: This is purely technical. I gave this presentation at the Space Science Conference in [225?] an International Federation of Control Symposium. I had to give a purely technical side then which was actually a responsibility of at that time my Central Systems Engineering Officr or laboratory. [230?].

71. Waring: That was Center-wide wasn't it?

72. Haeussermann: Yes.

73. Waring: Was it Ludey Richards?

74. Haeussermann: No. In 1969, Wehrner Von Braun found it necessary because he foresaw many new projects would come up in the future which he could not handle as personally anymore as just the Saturn class vehicles. He requested that we give some more thought to this, and then he established the system, Central System Engineering Office. It was equivalent to a laboratory, but it went Center-

wide. He assigned me this position through the years. This way I was not responsible for such new designs.

75. Waring: It was not as head of your laboratory?

76. Haeussermann: Of the Astrionics labortory no.

77. Waring: Were you holding then two offices? You were head of Central Systems Engineering Office and the Astrionics Lab?

78. Haeussermann: No, Mr. Bruce Moore had taken over. I had proposed to Dr. Von Braun that he . . .

79. Waring: . . . M-o-o-r-e?

80. Haeussermann: Right. . . . that he heads SL laboratory.

81. Waring: This was done in 1969?

82. Haeussermann: I think it became effective the first of May 1969.

83. Waring: That was just before the project started?

84. Haeussermann: Right.

85. Waring: Was this . . . .

86. Haeussermann: In the [254?] laboratory, the specifications then had developed for the Lunar Roving Vehicle.

87. Waring: That was done before there was a contractor?

88. Haeussermann: Yes, or it might have been at the same time. At that time I could not say for sure. It was before the specifications which were the more detailed work could be given to the contractor.

89. Waring: Now the Central Systems Office must have been in place after Program Development had been established, or were they at the same time?

90. Haeussermann: No, Program Development had been established much much earlier. It was already necessary for the Saturn V system. It just got an office for the Lunar Roving Vehicle. Their assignment was mainly to see that schedules be kept, that funding was properly provided, and so on. My side was the engineering aspect. Of course we had lots to condiser that we did not exceed the cost so we had to close it together.

91. Waring: That is something I did not know about. I have not read anything about that division of labor.

92. Haeussermann: You should try to obtain an organizational chart from that time.

93. Waring: We have organization charts, but it is not always clear because historical documents are often missing. It is not always clear who is doing what.

94. Haeussermann: If you need some answers, I might be able to give you some because I know the official organization charts are often not properly dated, or some got even lost.

95. Waring: There are some gaps here. In a minute I want to ask you about the Lead Laboratory Concept which it seems a lot of documentation about that has been lost. Let's turn now to the Telescope Mount. Could you tell me how you got involved in the ATM project and perhaps a little bit about the origins of the Telescope Mount project at the Center?

96. Haeussermann: Yes. In my laboratory, we had done quite some telescope work before and especially fine point controlling of instrumentation. This was a requirement that was very demanding from the astronomers who asked for pointing accuracy to a lunar spot, particularly a sun spot, after one accent. This requirement was relative of the [300?] because it had to stay for some time, for long exposure times, and so on. We had to do some of this development work here and prove that this can be done. This was an in-house laboratory assignment.

97. Waring: How had the Astrionics Lab developed that expertise?

98. Haeussermann: My earlier work with telescopes together with the Space Science Laboratory, and also by mainly work for gyroscopes that had to have a very low drift rate and the capability to control through CMGs. The whole control system for the Apollo Telescope Mount and the one for the Skylab itself was our responsibility so it was logical to give this to the same laboratory.

99. Waring: It was assigned to the Astrionics Lab working with the Space Sciences Lab?

100. Haeussermann: Right.

101. Waring: In one early design of the ATM, it was going to be mounted on a Modified Lunar Module rather than to an orbital workshop itself.

102. Haeussermann: I do not recall that.

103. Waring: Well there was an earlier design in which they were going to, well at first they were going to put the telescope mount on the service module. Then it was to go into a lunar module. Then, as the cluster configuration for the orbital workshop was developed, it would be attached to the Multiple Docking Adapter.

104. Haeussermann: You mean on instruments which were later indicated into the Apollo Telescope Mount?

105. Waring: I do not know if all of them, but many of them were designed for the Lunar Module. I think the contractor primarily was doing work modifying the Lunar Module so that may not have affected what was going on in your lab very much. I read recently that early work on the instrument panel for the Telescope Mount was largely done when they were conceiving of the ATM as being mounted to the Lunar Module. Do you know anything about that? Were you involved in the controls?

106. Haeussermann: Not directly. The control of the instrumentation of the telescope instruments was not the responsibility of my laboratory, of the Astrionics Laboratory afterwards.

107. Waring: Just to explore this, the expertise that the Astrionics Lab had on this equipment, were there direct legacies for this sort of technology from the Saturn program or was this pointing control and use of gyroscopes, was this primarily done for other smaller projects?

108. Haeussermann: No, there were no other similar projects. We had done on phases to intricate ourselves. For instance, we started work on CMGs [365?] components. We had some very gifted engineers who were established controllers for this purpose. It was clear that the best place to bring it would be to the Astrionics Laboratory.

109. Waring: Once again, could you talk about in some more detail the technical challenges that were involved in trying to create this pointing system? For instance, this was going to be mounted on a manned space craft. How did you try to compensate for the motions of the astronauts or other similar problems could you talk about?

110. Haeussermann: We had a simulator, one for [379?], in the laboratory in a huge room. On this we simulated the control of the ATM in its suspension. This was very difficult for us because on earth you have the mass, the weight, and the elastic defamation of the system which you do not experience in orbit. We used, for instance, mercury float or mercury baths to float the simulated ATM in such a way so the weight to one gravitational force would be compensated. This is still of

course not satisfactory because individual elements still showed defamation. The challenge was to have an accuracy of 1arc seconds [399?] as dictated by sensors from the astronomers. Whether this could be done with [403?] or with a tolerance and sensors we had proposed, that was a tough question. The best it could prove in the laboratory was that we had an accuracy guaranteed to about 6arc seconds. We had no way to improve the situation. Thus we were or course anxious to find out what did we actually have in space and later had to operate. We asked the astronomers, "Can you tell us what was [413?], how far [414?]?" The answer was very pleasing. They said "We can't tell you because our instruments went deeper than they are supposed and could not find any deviations larger than what our instrumentation was on the [419?] 1arc second. Which was of course very satisfactory. I have not yet answered one of your questions. That was how we compensated or took care of the motions of the astronauts in the Skylab. Of course we had in the simulation bring in torques corresponding to such a disturbance [427?]. We did this, and from this we derived how far they have to be careful in pushing themselves off of a wall and so on and to sensibily measure [431?] how far to restrain themselves.

111. Waring: I see. You worked out some idea of how the astronauts should change their behavior during the use of the ATM?

112. Haeussermann: Right.

113. Waring: Was there much in the way of computer simulations or computer modeling during the design of the ATM?

H:: It was a combined simulation of the anti-lock devices we had built up in the laboratory and of course computers.

114. Waring: Was the simulator housed in the laboratory itself? Do you remember what building it was in?

115. Haeussermann: Well the whole [447?]? was in building 4487 which was the Astrionics Laboratory. How close its components were, I could not say.

116. Waring: That is what I wanted to know.

117. Haeussermann: There are still some people out there who might be able to answer your questions. One who was deeply involved in CMG work and I believe also the simulation was Hans Kenner. He did the basic work for the controller of the CMG. You might want to know that one of them failed and therefore we had no [461?] anymore finally. That was closed and then another one would fail. He worked out new schemes so that the stuff could stay in a reasonable operation phase.

118. Waring: What is his last name?

119. Haeussermann: Kenner, K-e-n-n-e-r. If there were other people involved in the simulation, I am sure he can tell you those. You have not talked to him yet?

120. Waring: I have not talked to him yet. Do you recall was there any change made to the ATM when Skylab was changed from being a wet workshop to a dry

ground outfitted workshop? Did that affect the work of the Astrionics Lab? That was done in 1969.

121. Haeussermann: Yes, and that was my [478?].

122. Waring: In that decision?

123. Haeussermann: In the decision and also in the design when it became possible to use the Saturn V booster for launching instead of as originally planned using the Saturn IB. I was very much involved in this. Also in its design to swivel the ATM in converse to the original rendezvous by a second transport. We had the wet workshop first up, and then in addition automatic rendezvous [493?] of the ATM to attach it to the Skylab. You know this. I do not know how far you are interested in such things. Dr. Von Braun thought in a meeting after the question of whether we would not have a final to go work the Saturn V which was more or less a kind of a surplus because additional lunar flights had been cancelled. He brought up the question, "Who fails? Who wants to see that we have a design in such a way that we bring the whole combination in one Saturn V launch up into orbit?" At that time, there was several lab directors thinking that we should not our course. We should leave it as it is.

124. Waring: They were opposed to the dry workshop idea, or where they opposed to attaching the ATM to the workshop and flying them all together?

125. Haeussermann: The latter. I believe that NASA Headquarters, but I could not give a name, had also [523?] up with Dr. Von Braun. That is why Dr. Von Braun brought it up in our staff meeting or board meeting. There was, as I said,

several laboratories in objection because it was considered a change. I personally saw only considerable simplification of this to do it this way. It went from Braun for a vote. The vote was not to do it, but Von Braun gave the assignment to me to look into this and to come to the proposal and see how feesible the situation would be, and what it would mean. I had, even in this meeting, two of my closest engineers who were excellent in this respect. They supported me fully, and we came out with a design two weeks later without help of other laboratories. This was so overwhelming in assembly, simplification, not less, not least, because we had no experience yet with an automatic rendezvous which would have been necessary. In this case we just had a design that you mechanically turned the ATM 90 degrees out of the center line. That was what we proposed, designed, and it was accepted.

126. Waring: When this decision was made, was the dry workshop decision made first, and then you decided to have a deployed ATM?

127. Haeussermann: I looked into this matter just recently. Do you know Mr. Lee Belew? He can give you much more information on this. I had discussed it with him, and he said it was a tremendous pressure from Houston to go away from the regular [568?]. Just to have good cooperation with him, the decision was then made to abandon the wet worker and to go dry workshop. How this was in sync with the decision of forgetting about an automatic rendezvous and tilting it over, I could not say.

128. Waring: I will try and check to see if I can find a chronology.

129. Haeussermann: That would be good for you to do.

130. Waring: I suspect that the wet to dry decision came first.

131. Haeussermann: As far as I know, there has always been such a desire not to go dry. I personally was never in favor it myself.

132. Waring: Of the wet workshop?

133. Haeussermann: Yes.

134. Waring: When that decision was made, was that controversial in the Center? Did Von Braun have to overcome resistance?

135. Haeussermann: I do not recall anything to suspect.

136. Waring: It seems to me that once the decision was made there was not the resentment or second guessing that there may have been about the various rendezvous techniques for the Apollo mission.

137. Haeussermann: Right. Actually I've discussed this point. The same question was brought up to me two or three months ago through the International Lunar Consortium. That is how I talked to Lee Belew. He said he is still convinced today we could have it as well done with the wet workshop as with the dry workshop and was more or less a nice schedule to heed to the pressure from Houston. It might be better that you discuss this matter with him.

138. Waring: I talked about the wet to dry decision with him, but I have not talked about the ATM rendezvous method versus the deployment method. I will check into that.

139. Haeussermann: It must have been about the same time.

140. Waring: In designing the pointing and control system of the Telescope Mount, were there scientists from outside Marshall involved in the design? It was an in-house design?

141. Haeussermann: It was an in-house design completely. Of course we used for components outside help for the development of the CMG. They had . . . . [turn tape over 632]

142. Waring: Was there anybody from Goddard involved in the design?

143. Haeussermann: No.

144. Waring: It was strictly old Marshall hands?

145. Haeussermann: Yes. Again, this was all over Marshall. For instance, the mechanical part was done in PV; Astrionics took care of the control . . . .

146. Waring: Mechanical part, you mean the actual assembly?

147. Haeussermann: That is right.

148. Waring: Propulsion and vehicle engineering?

149. Haeussermann: Right. Even the layout of the wiring and all these things was done there. We designed it for the wiring and diagrams and all this. They then designed how the wiring treated in so that we had minimum [640?]. There was a close cooperation necessary of course because if you wanted to have an accuracy of 1arc second, you can not allow any friction or restoring torques on the [642?] system. You have to work closely together to obtain an optimum design for minimum friction and to storing [644?].

150. Waring: The ATM was perhaps one of the last great in-house construction projects. Would you say that is a good assessment?

151. Haeussermann: I would say so, yes.

152. Waring: What are the advantages of doing a design and development project like that in-house?

153. Haeussermann: I consider the main advantage that you have a tremendous flexibility. When you have a new design, you very often have to make modification and changes between the electrical people, the mechanical people, and the scientists who come up with new demands. These modifications and improvements that you have made, can this be handled without going through contractual bureaucracy? That was by the way, the reason why we were so successful from the beginning with the Saturn V, all the Saturn class vehicles. When there was any improvement or risk which had to be overcome very often indeed without drawings and the drawings that were made afterwards just by going to the workshop and saying "Let's try this," and

do it. This is a minimum of cost contribution. When you ask me what is the advantage, I say the flexibility and the minimam of cost and the time element of course also because very often you just you do not know exactly which of two solutions you propose is the best. You can try both and tell very quickly the cost. You go directly into the laboratory or workshop and propose it and be done with a minimum of paperwork. That is so effective.

154. Waring: Could you describe the Lead Laboratory Concept? This is a part of Marshall's history that has been very hard to find out about. There are not a lot of pieces of paper describing this. Evidently with the ATM design, the astrionics Lab was the lead lab, right?

155. Haeussermann: Yes.

156. Waring: In effect, you were . . . .

157. Haeussermann: That means the laboratory that had the lead lab assignment had to develop the specifications, the interfaces with other laboratories in close cooperation of course to their environments or to their proposals. The final decision was then with a lead lab or the final demands that could be fulfilled was for the lead lab.

158. Waring: This was used only on in-house construction projects or was it . . . ?

159. Haeussermann: Well, of course very often we had to give them the design like in the Lunar Roving Vehicle case, we had to give the design to Boeing or the

contractor. Then this laboratory was the main spokesman leading with the contractor.

160. Waring: It was the main technical spokesman with the contractor?

161. Haeussermann: Right.

162. Waring: OK, that is something I did not know about.

163. Haeussermann: Now the Program Development group always was included in these discussions with contractors because as soon as the contractors gave their modifications which usually are very expensive. Sometimes a compromise had to be done and a different solution had to be found and was sometimes could have been proposed by the contractor. Then we had to see how far we negotiate this to everybody's satisfaction.

164. Waring: Do you remember when this Lead Laboratory System was established? It was relatively late I think. I think it was like 1968. Does that seem about right?

165. Haeussermann: I could not say.

166. Waring: Well let me ask you that question in a different way. Had it always been done that was going back to the early . . . ?

167. Haeussermann: I think it had been done more as always this way according to the chart or through the assignment of the laboratory. What Wernher Von Braun

achieved so miraculously was that the laboratories worked mostly together. For instance, a service laboratory was mainly involved in the design of the mechanical components. The requirements for these components came very often from the Aerodynamics Laboratory, Aerodynamics and Ballistics Laboratory. We had to work very closely together asking who is needy because the ones I had to say "I can't fulfill this," what do you say or "I can not fulfill it." Then they had to work together if the proper demand could not be fulfilled. Where is the compromise? You did not feel that you had a decision where you could dictate. You wanted both sides to have the optimum. We did not a program management or Dr. Von Braun to become involved in such details which we had to work out. This was just the strength we had in our system that we had the mutual understanding everyone wanted to contribute so that we had an optimum solution without any hardship to a specific laboratory.

168. Waring: When you say that there was a Lead Lab, this did not mean that it was the director of the lead laboratory, the chief engineer of the lead laboratory, was a tyrant or a dictator?

169. Haeussermann: By no means.

170. Waring: He was just the chief coordinator for a particular area of the project.

171. Haeussermann: Right. Have you ever heard of Dr. Rudolph and his problems now?

172. Waring: Yes.

173. Haeussermann: He had a wonderful habit. He was in program management [722?]. If there was a problem of technical nature he just called the lab directors and the engineers involved for a meeting and asked for their presentations from both sides. Then he just negotiated. He never dictated. He just negotiated and said, "Well, you have to get together and to see that there will be a solution." This could last until midnight until they had a solution. There was never in this respect any kind of dictatorship. It was always in such a way that all you fellows have to find out a solution that is satisfactory that both sides can stand up and say, "I can do it."

174. Waring: These Rudolph meetings were just exceptionally long versions of the sort of thing that went on normally between labs?

175. Haeussermann: Right. Especially with respect when cost view points came up and program management had to be involved.

176. Waring: Right, because when you're making a technical decision, it would effect schedule and cost relationships with the contractors.

177. Haeussermann: It was only then that Rudolph became involved, otherwise the laboratories tried to resolve this by themselves.

178. Waring: Did this Lead Laboratory System persist all the time that you were out at Marshall? Would you say it is still essentially intact or now does the Program Office have greater authority?

179. Haeussermann: Almost it ceased with the [743?] responsibilities for the technical design with the laboratories were more or less dictated by NASA

headquarters or higher ups, I think it was in the early 1970s where contacts had to even out even for research.

180. Waring: I see.

181. Haeussermann: Under Nixon, it started. Unfortunately I have never kept this paper, but there was this classified note, classified paper, that admired that per year a certain percentage of R&D work, and was a high percentage, be contracted instead of done in-house. I would kill something to have a document copy of this.

182. Waring: We will look for it.

183. Haeussermann: You should look for this.

184. Waring: Let me see if I understand this. At some point evidently during the Nixon years, NASA Headquarters or Washington . . . .

185. Haeussermann: Washington decision.

186. Waring: . . . a Washington decision was made that design projects too would be contracted out.

187. Haeussermann: Especially research work.

188. Waring: Especially research work. In effect the Lead Laboratory Concept of Research and Development was especially used in the early phase in the project.

189. Haeussermann: And lost its significance later when R&D work had to be done by contractors. Still of course, if you had specialists in the fielded area then everyone said "Here is a lead man on the lead laboratory, and we want to have his advice and want to see that he becomes involved." Just in this case, Mr. Kenner is a good example on the CMGs. Even Houston a few years ago came back and asked him. He will be able to remember.

190. Waring: That is an interesting story. I will have to ask more questions about the lead lab system. I had not quite understood what had happened to it. I had a vague idea about how it worked. Would you say that Lead Lab System was used more or less back at Peenumunde?

191. Haeussermann: No.

192. Waring: This was something developed during the ABMA years, early Marshall years?

193. Haeussermann: Yes. It grew out of just the assignments of the laboratories.

194. Waring: When new technology would be developed it would be assigned to a particular lab?

195. Haeussermann: Right. There were sometimes strange overlappings. For instance, when you build a control system, my opinion is that control system should be built by one group and not split it up into three or four groups. When Marshall Space Flight Center was reorganized under Petrone, he just disregarded this completely. The responsibility for gyros was with Astrionics. The computer system

was in another laboratory he had created. The services was again somewhere else, and so on. This more or less destroyed then the Lead Laboratory Concept.

196. Waring: The reorganization, that was 1973 wasn't it?

197. Haeussermann: Right.

198. Waring: Do you think it was intentional on his part? Do you think he was trying to weaken the lab or was this incompetence?

199. Haeussermann: He has the right word; I just want to use it! It was incompetence! Nobody knows for sure why he did it, but he had the intent maybe dictated by headquarters to equalize the manpower in the laboratories. He did it without understanding, in my opinion, the functioning of the laboratories. Mr. Moore might be able to tell you a little bit more. He was deeply involved in that.

200. Waring: Is he still working at Marshall or has he retired?

201. Haeussermann: He is director of Systems Dynamics. You will find him in the phone book.

202. Waring: Is Systems Dynamics a company?

203. Haeussermann: Just a moment. If you don't mind I'll get the phone book and find it here. [turns tape off while getting number then turns it on again]

204. Waring: Once again with the ATM and Skylab, Marshall had a prolonged role in operations. Were you involved in the operation side of the ATM or Skylab? Was that primarily done from Houston with the personnel Marshall sent to Houston?

205. Haeussermann: I personally was not involved in this. This was mainly done by personnel in Marshall Space Flight Center to my knowledge.

206. Waring: I will check into that some more.

207. Haeussermann: Again, there was a close cooperation there of necessary. For instance, you might know that gyro components had to be changed during the Skylab period.

208. Waring: For the ATM gyros?

209. Haeussermann: Yes. The astronauts had to do this. The gyros were the assignment of the Astrionics Laboratory. They had to work closely together and train them how to connect them and to check the system out.

210. Waring: Perhaps we could talk in a general way about ATM. Would you say as with the Lunar Roving Vehicle, was the ATM essentially managed in the standard way that the Saturn program had been managed and organized?

211. Haeussermann: Yes.

212. Waring: The main differences would have come after Skylab with the Shuttle program or after the reorganization of the labs?

213. Haeussermann: Right.

214. Waring: Do you remember any particular highlights from this era in Marshall with the Roving Vehicle or the ATM? Any particular stories that stand out or moments that stand out for you?

215. Haeussermann: I think Mr. Kenner can give you mainly information in this respect. For me it was interesting how we solved, that it was possible to solve with the Marshall Space Flight Center and nowhere else, how to control the system after one and possibly a second CMG failure. Again there was later a very interesting cooperation just with the specialists that there was no plans from the beginning what would happen when Skylab would come down. The time came where it is stopped. There was a very close cooperation between Houston and Marshall Space Flight Center like in the good old times despite reorganization only because the proper people there getting together and trying to solve the problem. They did very satisfactory.

216. Waring: Let's talk about the Center rivalry issue. Do you think the rivalry between the Centers primarily exists when they are competing for new programs and then during the technical phase there is a lot more cooperation?

217. Haeussermann: That is right.

218. Waring: Do you think that is it?

219. Haeussermann: That is it.

220. Waring: Did any of that competitiveness show up in the technical areas during your period?

221. Haeussermann: No. On the contrary, when I mentioned to you that reasonably there was such a problem with CMGs, unfortunately Dr. Lucas had given up this part of the assignment we always had here in Huntsville with CMG methods and had to reach a deal with Houston that this be in the future Houston's assignment. They had very soon a very tough problem and needed Mr. Kennel's help, and he gave it to him! On the operations basis, he just wanted to do the job well.

222. Waring: You mentioned when Mr. Petrone came, also during this period when Skylab was being designed and built, the ATM was being built, and the Roving Vehicle was being built, Marshall was undergoing considerable reductions in force. Did this effect morale or the way work was done?

223. Haeussermann: Yes it affected morale very much, but it came so [883?]. It was of course disturbing, but management was careful enough that the most important people could be retained.

224. Waring: So there was never a feeling that there was a crisis at . . . ?

225. Haeussermann: Therefore never a crisis would be developed. But we lost, on the other side, due to these reductions a lot of capabilities we had built up which had to be given up to industry then. This was in some part very damaging. These years till about '69 or until this directive from Nixon came out, until that time it was

possible that our own engineers and employees could gain experience in the laboratory and therefore be in a position to see whether the contractor is doing the right thing. They had this capability of technical judgement which got more and more lost and be replaced by just managing a contractor.

226. Waring: So there skills became more like the skills of an accountant rather than the skills of an engineer?

227. Haeussermann: Right.

228. Waring: Was there a smooth transition from Von Braun to Rees? Could you describe the . . . ?

229. Haeussermann: I best repeat what I heard from several people. There was practically no change in how these went just along the footsteps. Maybe due to him, maybe because of the whole inertia in the system that it worked effective for the period about Rees was in the same way as Dr. Von Braun. Of course at that time, [912?] was already that they expected. [913?] could not be received that there was a continuous reduction in personnel and manpower dictated by [916?] or higher up. This was demoralizing of course.

230. Waring: Well that is perhaps a bad note to end on!

231. Haeussermann: Well, you can not avoid it!

232. Waring: We can not avoid it! When I come back, chances are I will bring Dr. Dunar with me, and he will ask you some more questions about that. He is writing

that particular chapter. I think we are pretty well set, unless there is something else you would like to add.

233. Haeussermann: I guess you have the proper handbooks and all the books that have been published on this?

234. Waring: Yes, I think I do. One thing that we lack that we would really love to find, we have been unable to find the Weekly Notes from Von Braun era. Do you have any idea where those could be located?

235. Haeussermann: The only place I could think of is the Space Museum.

236. Waring: They are not there evidently.

237. Haeussermann: Is that right?

238. Waring: Yes. There is lots of other documentation, but I do not think there is a full collection of Weekly Notes. In your conversations, if you could perhaps ask and maybe somebody else knows.

239. Haeussermann: That is so strange. I never kept a Weekly Notes which came out after each staff meeting.

240. Waring: Sometimes the weekly notes are so . . . . [stop 939]