INTERVIEW WITH DR. WILLIAM LUCAS
INTERVIEWED BY A. DUNAR AND S. WARING
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1. DUNAR: Why did you come to Huntsville? Did you know about the activities of the von Braun team before you came here or did you come because of a connection with the Army? What drew you here to Huntsville?

2. LUCAS: I didn't have any connection with the Army. When I came here I did know something about the activity that was going on. I was in graduate school at Vanderbilt University and one of my professors there was a consultant down here. I think it was to the Thiokol corporation, but I am not positive of that. He was a consultant and was aware of what was going on down here and suggested that I ought to look into the situation down here. It appeared to him to be an exciting new prospect of research and activity. That was my introduction to [Huntsville].

3. DUNAR: When you arrived here it seems, (just from an outsiders perspective) like an unusual thing, you had three different groups here: civilians, Army and the von Braun Team. Would you describe how that interrelationship worked?

4. LUCAS: You did have the Army. I don't know if I would divide them quite as you did here. There was the Army program here, the rocket program that was being placed here. The Arsenal as we now know it, the Redstone Arsenal, early on was two arsenals, the Redstone Arsenal and the Huntsville Arsenal. By the time we came here, it was all called Redstone Arsenal. The Army had a program of some sort here. The von Braun group came in here and established what was know as the Guided Missile Development Division of Redstone Arsenal. So it was made up essentially of civilian people. So there was a military component on the Arsenal. There was a civilian component not involved directly

in the rocket work that von Braun and his team became involved in and then the von Braun group itself which was known originally, (as I recall) the Guided Missile Development Division.

5. DUNAR: Initially then, you were not associated with the Guided Missile Development Division?

6. LUCAS: Yes I was. I came directly into the Guided Missile Development Division. That was where most of the in-house technical work was being done, although there was some work being done by Thiokol and Roman-Haus and a civilian component of the Redstone Arsenal. Primarily, in solid rockets.

7. DUNAR: We wondered also about the management techniques and the style, and where the direction came from for your group, for example. Did it come from von Braun and his group directly, from Medaris, or from the Army side?

8. LUCAS: Medaris hadn't arrived on the scene at that time. He came later on, around '55 or '56. There was a military person, I think a Major, who was (so-called) head of the Guided Missile Development Division. Von Braun was the technical Director. The direction for all the work that I was involved came from von Braun. Some of the administrative functions were handled by the Army and came down that way. It was just like any other division or organization, where there is a top organization and then there are suborniate elements. I don't recall what all the names of the other elements were. But Guided Missile Development Division was the one that I knew and which was headed effectively, essentially by von Braun as the Technical Director. Then that grew and the technical component of that was transferred into the Army Ballistic Missile Agency and then ultimately into the Marshall Space Flight Center.

9. DUNAR: Before Medaris, or before it became ABMA, your directions came from [051]...from White Sands. There was also the Army's push. How were those two projects integrated? In other words, if you have the Arsenal here the work had been done at White Sands. How was that brought together and what was the mission as was seen by the supply group that was formed from this?

10. LUCAS: Are you referring to the von Braun group?

11. DUNAR: The von Braun group and the interrelationship with the Army and the Army direction from Redstone.

12. LUCAS: Well, the Army direction, I believe was not significantly different than what it had been at White Sands, that was an Army post also. They came in from out there and they had test facilities, very good test facilities. They had put together and test flown some V-2 rockets out there, and V-2 rockets with upper stages on them around 1949, gained a new altitude record by launching a V-2 and a "Wac-Corporal" as a second stage on the V-2 rocket. That was the large liquid rocket work that was being done. That was the only liquid rocket work that came to be done here. The people who were here at that time were working on solid rockets and small solid-rockets.

13. DUNAR: Your work was primarily in liquid rocket?

14. LUCAS: That's right. At that time that is what we were working on. The von Braun group came here and they did a number of things. They were working on the RAM jets, but principally rockets and the Redstone Missile was the thrust of all of that. All of this was under the Army's Department of Ordnance. Ordnance had the oversight for the White

Sands Range and then of course, for the range and what was done here. For the von Braun group it was a matter of changing locations and changing facilities and adding a large number of people to their small group.

15. DUNAR: We have read some things that talk about management style and so forth, some of this being the legacy of the way things were done at Peenemunde, but also again some that the Army system shaping those too, to a certain extent. Could you describe from your perspective how those two integrated?

16. LUCAS: At the beginning the practice was what is called the "Arsenal Concept". You know what we mean by that? It means we do everything in-house. That was what was done and that concept was very consistent with what the von Braun team liked to do. They liked to do the whole thing. That's the way it started. There was no real problem there. The concept of doing it all in-house was pretty much what their experience had been before and what they liked to do. So there was no real problem there. We developed some unique capabilities for doing the work in that fashion. You did it less formally, that is with less documentation and in a sense more efficiently that way. Of course, when you get to producing large numbers, that would not be a proper thing for an Arsenal to do, I think. It had been, building guns and so forth for the war. But it was soon recognized that would require a greater number of people and it was not consistent with the national administration at that time in and in particular the Eisenhower administration. I remember when it became apparent that we would produce a large number of Redstone Rockets that we went out on a request for proposal for industry to build those rockets and we didn't get any proposals. Then, the Secretary of Defense, who had formerly been the head of Chrysler, and so he had a talk with his friends back at Chrysler, I guess, and they did submit a bid and produced the Redstone Missile and ultimately the Jupiter Missile. I think the beginnings were consistent with the Army Arsenal Concept and consistent with

the desires of the von Braun group. But we outgrew that and then adopted, what I believe to be, a more reasonable approach to doing what had to be done.

17. WARING: As an American, was there anything about the management system of the Germans that seemed to you foreign, or odd? Or coming out of graduate school, did this all seem new and reasonable?

18. LUCAS: I should qualify it by saying I was coming out of graduate school, but I was not coming out as a raw recruit, because I had had work experience and military experience prior to going into graduate school. Between the time I got my undergraduate degree and my graduate degrees. The concept I think, that one saw among the German approach to things would not be greatly different than the slave labor aspect of graduate school. You probably are not far enough out of graduate school to recognize, at least in the physical sciences at least, or the graduate student is laboring pretty hard and under pretty strict tutelage of their major professors. The German approach to the thing was pretty much that. There was a pretty tight management in terms of the head of the sub-level of the organization that was responsible for the job. It was pretty strictly managed. That didn't cause me a problem, because I was experienced in the discipline organization of the U.S. Navy and in graduate school. I am usually pretty much a discipline person anyway, so that didn't bother me. It did bother some people. Of course there was some perhaps resentment on the parts of some who had come not too long before out of the war situation and they had been shooting at Germans and vice-versa that bothered some people.

19. WARING: We found some evidence that many American engineers perceived the German scientists as being clannish and not very open to outsiders.

20. LUCAS: That is true and I think that is understandable and predictable. I think they

were clannish, but I translated myself into their situation and assumed I would have been the same way had the situation been reversed and I were in their country. They did come here as a group. Of course, they were brought into the United States through Mexico. They came in here and most of them didn't have their families initially, and they lived in on post there without very much freedom. If they went out they had to have an escort. That probably promoted the clannishness to a degree. Then eventually their families came over and they didn't speak the language as well. It's not surprising to me that they would be clannish in their social affairs. They did buy a block of land up on the mountain and many of them built houses adjacent to each other. Again, I think that's not at all difficult to understand. I would say that in some cases, and some people reacted to this, that on the job they were not as willing to discuss their expertise or to explain things as they wanted them done as one might like or expect from supervisors. On the other hand there were others that were very open and entirely willing to share what they do with the people coming in.

21. WARING: Were American engineers excluded from decision-making, in the earlier days?

22. LUCAS: In the earliest days I don't know that excluded would be the way to put it, in fact they were, but I don't know that was any different than the fact that the decision-making is higher up in the management chain, than most Americans were at that time. So, the early decision-making was largely a German situation.

23. DUNAR: Were there differences in approaches to engineering as well? For example we have talked to some of the members of the rocket team in town and they have mentioned the stress for example, the idea of the reliability testing as something they had brought to the situation. But that didn't sound too different from the engineers trained

here. Did you see any differences in the engineering approaches?

24. LUCAS: No, I didn't see any difference in that. I think there were really two levels of engineering (so-called engineering). You know in the German scheme of things there were diploma engineers and non-diploma engineers. They usually think of the diploma engineers as being somewhat equivalent to the master's degree level in this country. Whereas the non-diploma engineers may nearly approximate the bachelor level of training. Those people at the first level of engineering had more training in shop techniques and things of that nature.

25. DUNAR: That was another thing they play out that there was more hands-on experience that they had in their background.

26. LUCAS: Well, I think that is right, because I think most American trained engineers expect technicians to do the hands-on experience. So there was that difference. I don't think there was any difference in philosophy in terms of reliability testing and things of that nature. You might say more of a cut and try approach rather than an analytical approach. I think that it would be improper to interpret that as reliability testing.

27. DUNAR: Because of the limitations were placed on the ABMA (for example) in terms of the distance the rockets could be fired and the possible conflicts of the Air Force programs, did that limit the way in which you could evaluate the program that you were developing? Or to what extent, how did it?

28. LUCAS: Do you mean the range of the rocket. Of course there was the Redstone Missile was a short-range, as we think of it now, 75 nautical miles. The assignment of responsibility did affect what was done to a degree. As you know the Redstone Rocket had

been developed for a military purpose, to carry an atomic bomb. The desire of the team, von Braun team and many of us associated with it or who became known as members of the team also, had certainly not the ultimate goal of building a bigger bomb or rocket. The idea was to go into space. Von Braun in 1952 published a series of articles on space station. (You have probably seen those, the Colliers Articles) [207] That had been a goal for a long time. When we developed the Redstone Rocket, then the next step was the Jupiter missile. Which was to be a joint program for the army and navy, at least serve the interest of the army and navy. The Air Force at the same time was given the green light to go with the THOR rocket, which also was an intermediate range 1500 kilometer rocket. Those went along with that kind of development. That's when ABMA was formed to do the Jupiter Rocket. It was a competitive situation with the Air Force. One of things that changed the course of the activity and was guarded at the time as a setback, but turned out to be a favorable thing, was that around 1955 or so, when they were talking about a demonstration rocket for the international geophysical year was around July 1957-58. Von Braun had made a proposal on behalf of the team to launch another 15 pound satellite. The Navy came along and recommended to launch a 40 pound satellite, the VANGUARD. Some work had already been started. As a matter of fact, a proposal to launch the satellite was based upon the availability of the Redstone Rocket. The projection of putting two upper stages on it, (you've seen this? Do you know this story?) they put on the 11 solid rockets as a second stage and then the three solid rockets inside that. We had all this hardware and we could do that, so that was turned off. So the hardware we used, we went ahead and developed it anyway to use for the purposes of the Jupiter Missile. The Jupiter Missile of 1500 nautical miles, had to be launched out of the atmosphere and had to re-enter, and of course, generated a substantial amount of heat as it re-entered the atmosphere. So we wanted a test vehicle to test for the materials being used to insulate the nosecone coming back in. So we said, "Well we will develop this rocket for that purpose." We will fire that thing up and instead of firing it another time into space, we will fire it

back down into the atmosphere. We did that as a matter of fact, but that was the reason that when the Russians finally launched in October of 1957, their Sputnik I, we were in a position to say that in 90 days we could launch one also. People have often marvelled at how we could do that. The reason we could do that was the thing was already developed. In August of that year, we had launched that nosecone, which was the first nosecone or first body that had gone into space that had been recovered. If that's the kind of thing you are talking about, the competition was there. The Jupiter Missile was developed and a number were deployed over in Italy, but were eventually taken away. The THOR eventually won the toss as far as long-term development was concerned.

29. DUNAR: The resistance to heat upon re-entry is one of the things that was specifically worked on, right? How did you test that before it was actually being fired? How did you develop testing procedures, something that would be very difficult to test without actually have launched it?

30. LUCAS: Well, the test procedures that we used were subject to some criticism. What we did was use a rocket motor itself. We took the rocket exhaust, a supersonic rocket exhaust, and then placed the nosecone in that exhaust. [This] simulates the velocity as well as the heat-flux. We calculated the amount of heat flux we would get. Then we developed the technique in that way. It was rather an empirical approach to it and other people were using the same techniques because that was all we had at the time. We developed the abalation concept which was derided by a lot of folks at the time. The Air Force (some of the contractors for) was developing what is called the "Heat-Sink Concept", and we did that also. [This] was where you simply take the heat that you are generating and simply absorb it in the mass of material that has a high heat capacity. We tested some of those things. But we projected that the abalation would be better. The first real test, that I know, (as a matter of fact I think it is what gave us the idea) we were trying to replace the jet veins on

the Redstone Rocket. Most of the rockets today are guided by a "gimbling" [385]...motor. It moved it around like a rudder on a boat or ship. But in the case of the Redstone, we had to jet veins. [There] were four veins that stuck into the flame. Straight up if you wanted to go that way... We were using graphite for that. Graphite that we could get was not a sufficient quality to last very well. We had a lot of breaking veins. As a matter of fact, if we bought a 100 pieces of graphite to make jet veins, we might get 25 out of them. So we were trying to get a replacement material for jet veins. We tested some fiberglass reinforcement plastic, or micarta kind of thing that was being used to make table-tops and things like that at the time. We tested those for jet veins at White Sands and found that they didn't have very attractive properties. Jim Keats published a paper, a few years after the fact (because it was classified at the time) showing how the heat was carried away, or bladed off and didn't penetrate in a perpendicular direction, which is what you would really want in a nosecone. We tested a lot of materials like that. We tested the fiberglass reinforced plastic. One of the difficulties in using that as a jet vein was that this glass and stuff would run off and then solidify and interfere with the machinery that would cause the thing to turn. It didn't make any difference in the nosecone, but it did in the jet veins. So we tried even wet wood, so that you would take wood, fibrous wood, and impregnate it with water and put on a test cone. Then when it was heated the water would come out changed to steam. Steam absorbs a lot of heat. That was tested also. It would work as far as blocking heat, but the trouble was we never worked out the practicality of keeping the steam coming in one direction. It would go in the other direction eventually and blow the whole thing off. We also tested some water containing salts, like the alums. Those are crystalline substances that have in each molecule of chemical a great number of molecules of water crystallization (we call it). That is a way to concentrate the amount of water you have there so that you would have to break down the molecule and convert it to water and then raise the temperature of the water and convert the water to steam. All of those processes take up heat. It turned out that the abalation concept was the concept. It is the

concept that is most used today. We were made fun of at the time, nobody is laughing anymore! That's the concept that was used on the Apollo nosecone coming back from the moon. That was a lot of empirical kind of research and some analytical.

31. DUNAR: In terms of the way in which (I talked to Mr. Heimburg) and he talked about the way in which he had hanadled the test stands and the arrangements between the labs. It seemed to me to be an example in the way in which the von Braun team worked and kept in contact with each other. Did you test these things in your own lab or did you have to coordinate with other offices in testing?

32. LUCAS: Well, we did some "bench"-type of testing. But the kind of rocket motor we are talking about would have to have experts to do. These people did nothing other than that.

33. DUNAR: So, for example, if you were using testing the exhaust from the rocket engine at the time they were testing an engine, would you then give this as part of this test, or would that interfere with one of their test?

34. LUCAS: No, some of that was done. But mainly these test fixtures were set up and used only for that. We had the four HT and the 160 4HT. I remember those numbers. That was the heat transfer test with that many square inches of surface available depending on the nozzle itself. The test people operated the test. For example if we wanted to test, we would schedule the test and say, "On a Saturday or Friday at 10:00, we would like to test this." So they set up and run the test. We would evaluate the results. There were some cases where we would piggy-back the test on a larger rocket test. But there weren't so many of those, but ultimately we did that when we got large scale. When we took essentially a large scale item. That was the only way we had to do it. So that did require a

lot of coordination. But the running of the test rocket was mostly where it was done was purely a service. Much like a machine shop. We didn't run the machine shops either. We would say we need this built, they would build it. If we said we needed this test done, they would do it. Heimburg was the Director of the Test Laboratory at that time. Those little rockets, Eric Ball, who is now deceased, ran those small rocket test. It was really a conversion of the RAM jet facility. They had been doing some RAM jet work there and that work was no longer supported. So we transferred those same rocket engines to test. If you have been down there, that area was east of the tennis courts.

35. DUNAR: I am interested in the period of transtition from ABMA to Marshall and von Braun's attitude at that point. It seems for a time he was very much opposed to the idea of a civilian agency. Then both he and Medaris switched gears. I think it was Medaris that said when it came to the point that it was a "fait accompli" that there was no sense in fighting it any longer. Then he went to the point of endorsing it or at least supporting this idea and saying let's make it work. Did von Braun was as opposed to it as some of his public statements in 1958 or was he simply demonstrating loyalty to Medaris.

36. LUCAS: The latter. I don't think von Braun cared at all. Von Braun wanted to go to the moon. He wanted to build a space station. That had been his dream since a little boy. When he was building the V-2 rockets for the Germans, that was his idea. He wanted eventually to do something more that. That was his means to support. Here he had gotten that support from the Army. I don't think there was any resistance on the part of von Braun, personally, in transferring this. This was a great thing for him. It was the next step in the dream. Medaris was greatly opposed to it. He is to this day and makes a lot of statements to that effect, I think. Von Braun was loyal to Medaris, at least in his public statements. I am sure he did have a higher regard and respect for Medaris as did many people, including myself. I think von Braun saw this as the golden opportunity.

37. DUNAR: That is what I suspected. Some of his public statements are very much opposed to it at first. That he thought there would be more freedom of action through the Army. But this seemed to be that he was loyal to Medaris.

38. LUCAS: There were some things he probably visualized that in the case of the Army, the Army did provide a lot of the administrative support. In the Army, most of the work done is classified and that has its advantages and many disadvantages. So he probably saw that he wouldn't have that umbrella or shield over him when he went into the civilian sector. But of course, the great thing was that in the civil side of it. We didn't deal with the classified material. It was open. So what you do would be more like what you do at the university. It wasn't like something you had to worry about covering every sheet of paper. I think that was a great deal of freedom. The decision making process was greatly shortened. The distance between where you are and where the decision eventually gets made. With the Army, of course von Braun had to convince the local people. Then they had to go to Ordnance and convince them. Then Ordnance had to go to the Department of Army. Army then had to go to DOD. DOD eventually had to get to the President. Whereas in the civil side, von Braun reported to the administrator of NASA, who then reported to the President. That's a great advantage. Of course it also has some disadvantage because the visibility goes both ways. But as long as you are doing what you are [suppose to do] I think it is a great situation.

39. DUNAR: Were there problems during the ABMA days in terms of getting adequate support. I know there were in the early days. They had to really scrap and get things any way they could, in terms of materials resource. Is that true during the period of 1956-60?

40. LUCAS: That is true since time begin! In a discretionary project, resources are

always a problem. In the Redstone Missile, for example, you have probably seen the test stand out there, the original Redstone test stand is now on the historical register. We call that "The Poor Man's Test Stand" because we had to built it out of scraps and stuff like that. Through the development of the Redstone Missile and the beginning of the Jupiter Missile it was the same way. With the coming of Medaris and his influence, the resource situation was improved, but it was still tough. That was true of the Explorer I satellite. When we went into NASA, the situation was much better during the decade of the sixties, when the national commitment by the President was very popular. The commitment was honored. We had far more resources. Then when you got to the decade of the seventies, we got back into the situation we were in before. Now, we have gone to the moon, what do you want to do next kind of thing. I think that is an interesting thing, interesting difference that happened over the years. The contrast before and after. I wrote a paper one time on the differences of the decade of the sixties and the seventies in terms of resources. In producing a product and executing an assignment as we had, you have performance and schedule, and resources. So performance, when you are going to the moon, you can't give up on. When you have to go and it is just one place, you have to do whatever it is to get there. The schedule also was fixed. Performance was fixed, the schedule was fixed because the President had said, "We will go to the moon in this decade and with a safe return." The only variable among those three was the resources. Whereas resources were not wasted, they were far more abundant than they are today. Consequently, we made the mission. We could buy a lot of hardware. We could do a lot of testing. Every piece of hardware that flew, there were several pieces that were used an testing all aspects of the program. That was a technique you used if you wanted to make sure, absolutely sure, of your performance. When we came along with the shuttle, it was a different philosophy altogether. The same factors were involved. Performance was not as critical. You had to go in orbit, but there are a lot of ways you can go into orbit and a lot of orbits you can get into. You didn't have any commitment on time, but you did have a strain on budget. It was

underfunded, reminiscent of the earliest days of rocketry. Then, schedule became critical and when you slip schedule, you generate additional costs. The cost per year is not as great. But the total cost is great. I think the contrast is there, but as far as resources are concerned, they are always tight and will be tight. But they should be tight in a discretionary program. But they have been unreasonably tight in the seventies and eighties. So we could not afford to do what we knew needed to be done and had to be done to assure complete success. I think it is remarkably that we have had the success we have had under the circumstances. People who didn't come up in early rocketry do not understand, perhaps, the risky situation it is. We blew up a lot of rockets in the early days. On the test stands, in flight and otherwise. So when we talk about reliability and a difference approach in the early days of the Germans vs. American engineers, before one would make such a comparison, one must compare the results.

41. DUNAR: In that same line of thinking, the comments about what von Braun being a innovator and yet, a very conservative engineer. In a way those seem contradictory. I wonder if you could say a little bit about this.

42. LUCAS: I think that is consistent and that it could be done. Being an innovator doesn't mean taking undue risks. Innovator in terms of new things to be done, but conservative in one's approach to it. That is, doing a lot of testing, building a lot of hardware. Stress the hardware in every way you can envision prior to liftoff. That is always important in rockets. In the case of an airplane, there are not a lot of things that can go wrong with an airplane in flight that an alert crew can't correct and a least bring things back down safely. In rocketry, everything. When you lift off the pad you are dependent on the systems that are on board, that have been designed and put in it. Our mistake I think in the shuttle program was assuming it was more like an airplane than a rocket. I think the innovation and conservatism came about by saying, Yes, we will test this thing. We will test

its structure, we will test its mechanical working parts, its components. We will tie the thing down and test it in every way we can before we release it. That is what I would call conservative engineer on innovative projects.

43. DUNAR: Going on into the sixties, once Marshall was established, there must have been a great deal of friction in terms of giving way to the Arsenal concept and bringing in contractors. How was that adjustment made?

44. LUCAS: There was a lot of people that like to do things the way it was done before. An engineer on the drawing board could walk over to the shop and talk to the people who were building the hardware and vice versa. You could be more involved in the hardware. You could see what was happening, you could see if what you put on the paper could be easily converted into something hardware-wise. So for conservative engineers that is a good feeling to go over and test and see. Talk to the experts all along the line. That is always important in a small organization where you know everybody and you can talk to the test people, manufacturing people, the quality people, the analyst, the designers, everybody together, it makes a different situation. That was not to be when we got into a large program for two or three reasons, not the least of which is we never got a resource. It shouldn't have been that way, because by that time, we had people that were anxious to work in that field. Whereas we didn't get any bid the first time on the Redstone, we got several bids when we got down to the shuttle hardware. For a program that large you have to spread it around. There are some pretty smart people everywhere I have ever been. You want to use them all. There was some probably grumbling and resistance, but the change was inevitable and it happened. There was a compromise that wasn't made that would have been more desirable to been made. That is in the early days, when our people were fresh out of the shops themselves, had been building the hardware and knew what was possible and practical and what wasn't, then you could work with the contractors and work

with the interest of the government very well. You could go into a contractor plant and say, "Hey, that's not going to work. That's not the best way to do this thing. In our shop we did so and so." People would listen to you. Now you don't have a shop and you don't have people who are up to speed on manufacturing or any other aspect that you want to discuss. You go into a contractor's plant and say, "well this is not going to work." They reply "Says who? Why won't it work?" We say, "Well I don't think it will." They say, "Well, I think it will." That's probably a little exaggeration, but hopefully illustrates the point. We started two or three years before I retired to recover some of that in the Productivity Center. (You might have seen some of this) Coming from that area myself, and the materials, from the aspect of material engineering and processing. I pressed that and some other people responded quite well. Today that does provide a facility where you can test things out and generate a basis of judging what been done in the contract organization. I think that it is entirely understandable and reasonable that people would be reluctant to move away from the Arsenal Concept. It was necessary and was done.

45. WARING: How did the materials lab evolve through the Saturn Project. Were there significant changes when Marshall moved away from in-house to monitoring contracts. .....[661] was it relatively isolated or monitoring contractors.

46. LUCAS: On the contrary, I think it became more involved. The materials organization. (Of course you have to recognize there is a certain amount of prejudice, because that is my own discipline, my own love, originally) People working in the materials Laboratory became involved in virtually every aspect of the program. You can seen from that laboratory have come some of the leaders at the Center level, including myself. One of the reasons you can do that is, as a materials engineer, everything is made out of materials. So you get involved with it and whereas, you aren't the expert, you are conversant with what goes on in every aspect of the program. So you do become involved

in it. If you are talking about electronics, that's made out of materials. Semi-conductors, or if you are talking about large structural items, or engine components, everything you are involved in. So the materials people have evolved over the years, I believe, to occupy a broader role than originally. Where they still have the function of materials selectivity, evaluation of the performance of materials, they now have filled in the gap of processing that you would have formerly gotten entirely out of the manufacturing organization. Since we don't have a manufacturing organization anymore, the materials people have had to fill that gap in processing. In fact is now called Materials and Processing Laboratory. It was not called Processing Laboratory when I headed it. (As a matter of fact, I organized it) We at one time had a materials laboratory that was primarily metallurgy and another one called, Chemistry. I was given the task of integrating those two into a Materials Engineering Laboratory. I think in those early days, although we were involved in the practical aspect of the program, it was more of a research flavor perhaps because of the times and probably my own particular interest. Over the years, they have had to evolve, not only from the materials side, but from the processing side. So I think if any difference, they probably occupy a more significant role today then they would have twenty years ago.

47. WARING: During the sixties, during the time of the construction of the Saturn project, the technical labs and the project offices, could you describe the relationship between those two types of organization. Were there difficulty coordinating these managerial functions with the more technical side. We found some evidence that there was some resentment between engineers and these various offices. For instance, it was easier to get promoted sometimes in the Project Office. Could you comment on that?

48. LUCAS: Yes, I suppose that is right. That was a perception on the part of engineers that those project people could get promoted faster. I think that's primarily because the laboratories were there first. Then when we got a broader program, bigger programs

where everybody wasn't working on the same thing. Back in the Redstone days everybody was working on the Redstone Missiles, so you might say that von Braun might have been the project leader. But when you have larger projects and multi-faceted projects and multiply projects as we now have today, then today one person at the top can't do that. You have to have some project leaders. When you go to get your project people, you go into the laboratories, which we had. You pull people out and they get promotions. Then project people get a lot of publicity and attention, [therefore] the perception was that they got promoted. Its also conceivable that those same people who were selected to these leadership roles, might have been promoted faster if they had stayed in the laboratory.

49. WARING: As you were?

50. LUCAS: Yes, I could have been. I think that there was naturally some, I hesitate to use the word "friction" between them, I don't think that is accurate, but I do think there is conflicting interest. That [is a thing] you want to build into an organization. If you have your project manager over here who has a deadline. He has a project to complete, a schedule and resource problem and he has to get it done. Here's a person over at the laboratory, his professional reputation is dependent on how thoroughly he does his engineering or science job. To heck with the schedule, I want to do it right. So there is always that pull between them. Which I think is healthy. I think it is unhealthy when the laboratory people become too submissive for with what the project person wants. We always thought that was the check and balance we wanted built in. The laboratory people have to be hardnose and be sure that the thing is done right. The project person has to be hardnose and be sure he gets it when he needs it. So naturally,, there's a built-in conflict there. If you went to the principal project managers at Marshall today, or anytime in the past, and asked them "What do you consider the principal success of Marshall is due in producing hardware and it would be due to the relationship between the projects and the

laboratory. They would draw a contrast between what the Marshall Center has and NASA in general, but Marshall to a greater degree than others, as opposed to the Air Force. The Air Force manages a lot of projects, but they don't have what we call an "in-house confidence" to the extent that NASA does. There were frictions and are frictions along the way. But they are healthy and in the end it is the way to do the job.

51. WARING: When Marshall for a time was running Centaur Project of the early 60's, was originally an Air Force project and the Air Force ran into some real trouble and Marshall had to come in and bail them out.

52. LUCAS: That's right. Then Marshall got so busy with the Saturn Program they transferred the Centaur to the Lewis Research.

53. WARING: How did von Braun tie together the labs and the project offices? How would you characterize his management style. How did he keep the lab chiefs and the project managers working together?

54. LUCAS: We had frequent program reviews in which he participated in personally. His role was to make sure, as I tried to do, to make sure the technical people were heard and yet they maintained their position within the bounds of reason in terms of scheduling and cost. Von Braun was a great integrator. I think one of his strongest attributes was his understanding of so many different things. I have seen cases where he would have his lab directors and project managers all around a big table in ....[787] conference room and they would be talking almost like in unknown tongues or something. The expert in one particular field would be talking past the expert in the other field. They would be going at each other and not understanding really what each other were saying. Finally von Braun would take over and explain what was being said in terms that everybody could understand.

He had a facility for doing that. I think that is why he was so effective in dealing with the general public. He could do that do a very rare degree. I think the techniques, first of all, the fact that they were not working on a great many different projects. That was the easy management aspect of it. Then the fact that he would get the people around the table and make sure that everybody understood what was going on.

55. WARING: Were the weekly notes that von Braun introduced a helpful device? This has been described as an informal channel of communication?

56. LUCAS: Yes, I think it was. I don't know if they still do them or not. They did them as long as I was there. But that was a good technique. That was a technique that encouraged communication. There would not be the risk of put-down of somebody, just because he happened to be rocking the boat or something. People in the laboratories could introduce these notes. They were read and annotated and sent back to the people. I think it was an extraordinary technique. I think every management survey we ever had done pointed to the weekly notes as a good technique.

57. WARING: Did Petrone and Reece continue use of this as you did?

58. LUCAS: Yes. They were not a part of the system of records. They did not supplant any other thing in terms of communication, any of the more formal things. It was an information exchange, to help the top management understand other views. In top management, it is pretty easy to get isolated. You are totally dependent upon what other people tell, you can't be everywhere. It gave you a little better feel for what the disagreements were. Sometimes, there was no intent to disagree or uncommunicative, but simply two different organizations might have a different view on the same subject and weren't getting together. By the same token for those people down in the organization, it

gave them a feeling that they could get right to the top with their information and it would be listened to. I always read the notes, even if I had to leave off something else, I would do that.

59. DUNAR: Were the weekly notes used as basis for discussion when the meetings were held (that you described before)? Or was this merely a paper exchange.

60. LUCAS: Well, it could be, what you did was figure out problems. Usually they would take care of themselves. For example, you would send your weekly note in and I would read it and then just initial it. It would just be information. But if there was something on there that created a concern, you would just write on the margin (you had to leave a margin for that purpose. The format of writing the notes was prescribed.) then sent it back. So you might say, "Well, talk to so and so and see his note" then pin a copy of his note to it. Or, "please get together and send me a note on how you are going to solve this problem." If it didn't get resolved, it would come up during one of these meetings. But usually that's all it took to get people together.

61. DUNAR: Another issue that we want to get into is the one about Center rivalry. This becomes quite apparent after the end of Apollo. Were there problems in the sixties as well with the rivalry between Marshall and the other centers?

62. LUCAS: Yes. I think that's been over exaggerated. There was and I suppose, still is rivalry among the Centers. There are also rivalries among various elements of one aerospace industry. Even I suspect there may be some between departments of the university! I think its there, its been there. Some of it has been healthy. Some of it hasn't been healthy. But I believe in any case, it has been overstated. For example, if you are building some hardware, here's Marshall responsible for the launch vehicle of the Apollo,

another Center is responsible for the capsule, and we have a performance limitation, these engines can only lift so much. We know what the total weight is. So there is a tendency for all hardware to grow and weigh so that a hollow capsule gets larger and the pressure is to cut down on the weight of your structure, you have it to big. The people responsible for the structure say cut down on the weight of the capsule, you have it too big. That kind of thing went on. I don't know if that was totally bad. For example, the Apollo capsule as I recall, was suppose to weigh 78,000 pounds. I think a 105,000 pounds went to the moon. That kind of competition I am sure was there.

63. WARING: In the case of the Saturn program wasn't Marshall in a pretty strong position since they knew the technological limitations of the rocket and imposed real limits on what Houston could try to do?

64. LUCAS: Well, it did except that they always expected to upgrade. We have always been able to do that. You can increase the performance level of the rocket itself. For example, we did upgrade the thrust of the J-2 engine. We were prepared to upgrade the thrust of the F-1 engine. It was designed for 1,500,000 pounds of thrust for the F-1 engine. We never did this but we were prepared to do the testing so that we could upgrade that to about 1.65. So that was always expected. The difference in the shuttle program is, because of the combustion cycle of that shuttle engine, we told them we were upgrading it to start with. Do not make this any heavier. Of course they did. We did upgrade it by running it over 100% of thrust which compromises the engine. I think the difference of the Saturn Program and subsequent programs is the interface was very clean on the Saturn Program. The first stack through the S-IVB stage and the instrument unit was totally Marshall. That above, was Houston's. It was a very clean interface. Its a weird interface on the shuttle. There are just a lot of things different about the shuttle that make it remarkable that we have done as well as we have.

65. DUNAR: In terms of dividing responsibilities, when you have situations (some attribute) it to the change from having control from headquarters of having the lead center concept and that it brought out more center rivalry as well and competition for pieces of that program within that concept. Do you think that the lead center concept brought on more rivalry?

66. LUCAS: I think it did. I think the lead center concept flawed it from the beginning. I think I proved that when I was in the laboratory. After I was a graduate materialist, I was Director of Propulsion and Engineering Laboratory, which the materials organization was a part. In the early days before we got to the degree of this projection that we are talking about earlier. We had the lead laboratory concept. My laboratory was the lead lab on Skylab. We proved then that the concept wouldn't work. That concept will only work when everything is running smoothly. But that's not when you need it. You need it when things are not running smoothly. My philosophy is that you can not manage from a peer position. There is an old Chinese proverb that says, "If two guys are going to ride on a horse, one has to ride in front." That's kind of the way I think it is in project management. I think in a multi-component project, spread around the country, somebody has to be in charge. That person has to be in more than a peer position.

67. WARING: Because otherwise it results in all sorts of negotiations and negotiations have to bog down?

68. LUCAS: Yes, because no one can make a decision. If everything is a committee, it is a bad scene. This can be misunderstood. I am one of those persons that believe from a management standpoint, you ought to work things out at the lowest possible level. You ought to get people, the experts that have the knowledge to get together and work out the

trade-offs and come to a conclusion. But at the same time you have to have someone in the position to say, "Ok, I have heard from your side, and from your side, this is the way I am going to have it done. I am responsible and this is the decision." Then have someone make an uncontested decision. On the lead concept, you can't do that. A lead concept is even further made a nightmare if the lead person doesn't control the resources. Which is the case in the early shuttle. That's the golden rule: He who has the gold, rules. You have to control the resources if you are going to manage.

69. WARING: Could you describe what things were like as the Saturn/Apollo programs began to phase down? Was there a real struggle to develop new projects, new ideas, new ways to keep Marshall busy?

70. LUCAS: Yes, there was a struggle to do that and it began much too late. As you may know, I was right in the middle of that, because, an a hide-a-way of the top management in Marshall. I believe it was late in 1968, we came to confront what's next. We had been so busy working on the Saturn Program, we hadn't given much attention to what was left. It was concluded at that hide-away meeting,(the reason that is so firm in my mind, because I got saddled with the job) that we would develop a new business organization. Von Braun called it his vice-president of new business and I was tapped to head that organization, starting from scratch. That is how what is now called Program Development began. Organized program development and pulled in some very good people, small in number, to set out to develop the new business for the Center, for its succeeding generations. Every project in which the Center is now working had it genesis in that organization. I am really proud of the achievement of that very fine group of aggressive people that got busy and got in a lot of new business. We diversified the Center and amplified the management problem, but we kept alive. We might not have. The Center went down in strength from about 7,300 people at its peak which was around 1965 or 66 to its present size which is

maybe 3,500. Back in that time we also had a great number of mission support contractors. Due to some court cases in 1968 that involved the Goddard Space Flight Center first, but then reflected to Marshall, we had to get rid of those. They are building back up now, but our resource situation changed very markedly. There was a good, strong possibility that the Center could have been closed before the end of the 70's had we not gotten these new missions. I think it would have happened. We came very near to it, nearer than most people know.

71. WARING: We found a memo last week in Washington, D.C. this was from the mid-70's that described plans to close down Marshall. They decided that wasn't such a good idea. But it must have been on administrative minds.

72. LUCAS: There are very few people who know how serious that was. Very few people at Marshall. There were very few of us in that small group that made the plans and turned that around.

73. WARING: Why do you think that Marshall was slow in adjusting to this in the late 60's? Was it because people were so preoccupied with Saturn?

74. LUCAS: It wasn't Marshall that was slower than anyone else. It was NASA that was slow. NASA was preoccupied with going to the moon. NASA was slow. Marshall was by virtue of doing what we did in Christmas of 1968, were heavy to gain. I think if you look at the track record through the 70's, you find that Marshall has gotten involved, but in extraordinary amount of the new possibilities available to NASA. Marshall I think was leading, but NASA was the one that didn't give enough attention to its future.

75. WARING: And they have the money through Congress?

76. LUCAS: Through Congress they have the reach to. I think their money was used. That's been one of the problems of NASA even until this day, is the mission. Where are we going and stuff like that. It isn't clear today. There's not a real long-range plan today. That's one of the problems.

77. DUNAR: Even at that time, though, wasn't Marshall in a more precarious position than even some of the other centers? I know there were, what some people called at the time, The Marshall Problem, where it was top-heavy in terms of senior as opposed to more junior people as compared to some of the other centers?

78. LUCAS: I don't know that would be necessarily true. I think there are reasons that Marshall was more vulnerable, but I don't think that was one of them. The average age of the Marshall Center was greater than the other Centers. I think maybe the Lewis Research Center was up pretty close to it. Because we had some people who had been in rocketry longer than some others and we had a lot of people coming up for retirement. The average-age situation made us stand out. The principal reason was Marshall was primarily a propulsion center. Since we don't need so much propulsion anymore, we proved that we can get into space. The decade of the 1950's I usually characterize by saying, "we had to prove we could get into space." In 1958, we put up a 30 pound payload. Less than 10 years later, we put up one 10,000 times that much. We had the propulsion system, so the problem now is in space and that is not where Marshall strong forte is. That was probably the most plausable reason for it. There are other reasons for it if you want to cut back. There were certain jealousies of Marshall. Marshall had always been pretty independent, particularly under von Braun and had gotten a lot of attention at headquarters that headquarter would like to have gotten for itself. The second thing is that the political delegation of Texas was much stronger than Alabama in terms of numbers, their interests.

I don't want to speculate on other reasons, but I don't think it because of being top-heavy.

79. DUNAR: Could you describe the way you went about developing the ideas for these programs from the Program Development Office? Did you bring people from other departments and brainstorming?

TO BE CONTINUED AT ANOTHER TIME.