Transcript of Proceedings

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

Interview with

JOHN L. SLOOP

December 2, 1971

(THIS TRANSCRIPT WAS PREPARED FROM A TAPE RECORDING OF VERY POOR QUALITY.)

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PROCEEDINGS

VOICE: In Huntsville we are working on the NASA history of the Saturn launching, and it's funded through Marshall Space Flight Center under contract with the University of Alabama, and as we point out there in the little fact sheet there are several NASA histories in progress, and Tom, for example, is working on the headquarters Saturn history.

What we wanted to do with you today is talk about the propulsion systems, and I would guess especially liquid hydrogen, and talk about some of the inputs from Lewis, in particular, as to liquid hydrogen technology, not only as it applied to Centaur but also as applied to the Rocketdyne J-2 material.

I have read your paper that you just gave.

MR. SLOOP: You want another copy? I've got a pile of them over there.

VOICE: I'd be delighted to haveone.

What happened to the footnotes? Are the footnotes in here?

MR. SLOOP: Yes.

This isn't a very scholarly paper. I did it sort of as a fun thing, part time, and I wanted to get into the record a bibliography of some of our work, a lot of which does not apply to high energy propellants, but the first 177 references is a list of all the outputs, a number of which do apply to energy propellants. Well, the first 60 directly apply to high energy propellants. I refer to some of them. After 177 I started adding references for this particular paper. I hope that in your Saturn history you will be sure to talk to Silverstein.

The punchline of this paper, of course, is the fact that based on our work on hydrogen and his confidence in hydrogen/oxygen, he almost single handedly made the decision to use hydrogen/oxygen in the upper stages of Saturn.

QUESTION: Along that same line, when you say single handedly, did other members of the Silverstein committee oppose?

MR. SLOOP: No, I don't think they did. The other members were Colonel Adpole, Abe Hyatt, Cal Muse, George Sutton, Eldon Hall, and Wernher von Braun, and at the luncheon prior to giving this paper, Wernher told me that Abe almost single handedly made the decision. It didn't take too much persuasion because, as I said in the paper, he was chairman of the committee. He was also Director of Space Flight Development, and so he was in the top NASA position to make the decision, and the fact that he was convinced that hydrogen/oxygen was the right way to go all contributed.

Now, he had a strong ally, that is Eldon Hall, who was the secretary of that panel. He made a number of calculations on the performance of various propellants in upper stages of Saturn, and Hall was convinced that hydrogen/oxygen was the way to go, and he in turn had an influence on Silverstein. Abe came by to see me after I gave this paper, in fact it was last week, and suggested that if I wanted to follow up and dig a little deeper I ought to get some more information from Hall and some others.

Incidentally, Emme wants me to revise this paper or enlarge it and perhaps get Silverstein to help contribute and give it next October to the IAF session in Vienna. I talked to Abe Silverstein about that when he came by last week, and he felt he would not have much time to contribute personally, but he would be interested in doing what he did have time to do and suggested I go ahead and do a little research and see what I could dig up.

QUESTION: Would you happen to have the appendices to the Silverstein report? We have a copy of the report that I think you sent over to Gene's shop but we don't have --

MR. SLOOP: No, it's the other way around. Gene sent me a copy. What I've got is a copy of the report that Emme got from Von Braun, and it's in two parts. One is a summary part that was to the Administrator, and the other is the full report, I thought.

QUESTION: That's what we're trying to find.

MR. SLOOP: And Emme sent me a copy of that so he should have it.

QUESTION: That's just the summary. Do you have it here?

MR. SLOOP: No, I've got it at home, I think. I'll have to dig that out for you. But Emme had them both because that's where I got mine. He let me see his copy sometime several years ago. I don't know where it is.

QUESTION: Silverstein mentioned something else about this -- let me see.

MR. SLOOP: Abe mentioned that he had chaired a study for the Department of Defense on rocket systems in this '56 or '57 period, and I'm going to look that up and try to track it down. He didn't know much more than that.

QUESTION: It was concerned with liquid hydrogen?

MR. SLOOP: Yes. He also chaired a panel that led to Centaur, but that's another story.

QUESTION: That's interesting.

MR. SLOOP: But to pursue the hydrogen story to any greater extent, they ought to check out those two as well as the paper that you are referring to.

QUESTION: In the process of the work at Lewis, I'm very much interested in the injector design. You mentioned a coaxial injector.

MR. SLOOP: Now you've hit an Achilles' heel in my memory. First I wrote it from memory as coaxial and then I changed it before I gave it as showerhead, and this one shows it as showerhead. That's on page 12. And I called over and told Emme before I distributed this I wanted to check that one word as to whether it was coaxial or showerhead and I'm going to do that by checking the references. I did check one reference. In fact, I've checked three since then. I'm pretty sure it was showerhead. It was a very fine showerhead. By that I mean we used a hell of a lot of holes. The references that will tell you that are, the ones that I'm going to look up, are numbers --

QUESTION: 48 and 56, according to your note here.

MR. SLOOP: 48, right, and --

QUESTION: And 56, the one over here by Douglas and Hennies and Price.

MR. SLOOP: Right. I know we used the showerhead. In fact, I think the coaxial came in much later. I'm pretty positive that it is the showerhead.

VOICE: Why did you go from the showerhead to the coaxial then in later design studies?

MR. SLOOP: Coaxial is harder to make than the showerhead. Actually, the difference between the two is a matter of engineering design. The coaxial is just one tube within another, and the big problem is how do you keep the annulus around the tube constant. You have to have spacers or something of this sort. With the showerhead we didn't have that problem, we had a much better control, and our design concept was to get an intimate mixing of the fuel and oxidizer by using a hell of a lot of holes. So the showerhead was a very fine showerhead. That is, we had a hell of a lot of hydrogen and oxygen holes broken in very fine spray. You are familiar with showerheads. Some of the luxury types have a hell of a lot of holes that feel like a real soft velvety feel that you're under.

QUESTION: But the RL-10 design used kind of a co-axial injector design, didn't they?

MR. SLOOP: RL-10 -- actually no. I think -- and I'm not dead sure of that -- the oxygen came through tubes, and then the hydrogen came through the face, which essentially is like a showerhead.

QUESTION: But I think that that face, too, had some coaxial things. About five percent of the hydrogen was used for cooling across the face.

MR. SLOOP: I'd have to look back. You may be more right than I on that -- I'd have to check, but I don't think so. I think it was first like I said. Are you defining coaxial the same way I am?

QUESTION: Maybe that's the problem.

MR. SLOOP: Let me draw what a coaxial would look like and what a showerhead would look like. I'm a lousy artist. Let's say this is the injector face, and I won't worry about two dimension. Our showerhead looked like this. Say the oxygen was in this chamber, and it came through the injector face. Then the hydrogen came into this and came through a lot of holes that surrounded this.

QUESTION: Okay.

MR. SLOOP: If you had a coaxial, it would be two tubes. This would be the hydrogen tube, and this would be the oxygen tube, and if you were to look at this it would be like this. And if you would look at this, you would see a discrete series of holes. And of course, unless you have a space, if this tube due to heating or imperfect installation wanders over, to give you an exaggeration, you get something like that, and it's almost impossible, if you were to look under magnification, to have one like this unless it has spacers.

QUESTION: As I recall, the final injector design for the RL-10 and the J-2 is more along this line, but they actually had a --

MR. SLOOP: At one time they used a rigid mesh. You see, that's essentially this kind of a design rather than co-axial, in my thinking. They had to have their hydrogen flowing through in a bunch of tiny small holes. It flowed through mil-lions of them in this mesh, which was sort of a diffusion.

QUESTION: Do you know where that rigid mesh design concept came from?

MR. SLOOP: No, I don't. We used porous wall film cooling years ago, and it may have come from that. There was a lot of work on that sort of thing both at Lewis and JPL. For example, we would make a rocket engine out of porous metal, called transpiration cooling or sweat cooling, and then the water would come through the porous metal and vaporize on the inner surface and keep the wall cool. Origimesh -- I think it's a trade name for a controlled prosity type of metal wall that was made, and I think they were of course aware of this kind of work. Quite a few people did this. In fact, there's even been some of it done in aerodynamic things to control boundary layers and things like this. So I don't know where they got the idea, but it's probably like a lot of other things, evolution of thinking on their part.

QUESTION: You talk about your showerhead design here as being very successful. Could you make some comments as to what was different about it or what made it so much better than previous designs?

MR. SLOOP: Well, the fact that it was a very fine scale type of showerhead. By that I mean we used many, many small holes, and also I think the characteristics of hydrogen. In fact, we're using hydrogen as a coolant, and the velocity of the hydrogen, plus the finest scale, was the real --

QUESTION: As you recall, this was used in some of the early RL-10 engine designs?

MR. SLOOP: Yes, there was a man down here in town, Charlie Keane, who works for NASA who you probably ought to discuss this with, who was in on the early phase of that. Pratt and Whitney visited us three times or more. I have a record somewhere of the dates they visited us. At one time -- and this is pure recollection -- they claimed they openly borrowed our injector design, but I haven't in my mind the exact evolution.

QUESTION: This was from Lewis?

MR. SLOOP: Yes.

QUESTION: They said they just borrowed it from Lewis?

MR. SLOOP: Yes.

QUESTION: Rocketdyne, did they have any people visiting Lewis?

MR. SLOOP: Oh, yes.

QUESTION: Anybody by name?

MR. SLOOP: Well, we had Tom Myers who was one of the -- I should tell you this, that the NACA specialist said to me on rocket engines, which was formed in 1950 and ran until NASA 1958, always had a North American, Rocketdyne representative, had Bell representatives, had P and W, as I recall. I think we had P and W. And we met two or three or four times a year, and we always review our programs, and they in turn review theirs, and we had a very intimate communication, an informal communication at the worker level, with these companies.

NACA was viewed as a friend to all, with no objective, no axe to grind, and so they freely gave us access to what they were doing and kept up with what we were doing, and we had a steady stream of visitors to our place. Tom Myers, if I recall, was the North American representative on our subcommittee, and we visited them a number of times. I would have to go back to some of the earlier papers to name a few more. I forget whether

Castenholtz was there at that time or not. A fellow who works now for NASA was strong in their combustion installations, and R. J. Thompson, who was our chief scientist for the years, probably still is, was always active in our work and followed it. Bob came from N. W. Kellogg, and they had a rocket effort going, and when that folded he went with North American. John Tormey who worked in liquid rockets for years and then went over into solid rockets was . There are a few others but their names escape me.

QUESTION: Would you make some comments about the --

MR. SLOOP: I was incidentally on the J-2 source selection. I was on the F-1 and the J-2.

QUESTION: Could you just tell us about the J-2 source selection, design and things like that?

MR. SLOOP: I really don't recall anything of great moment on that. It seems to me that I had my papers, source selection for the F-1, returned to me about four or five years ago, and perhaps if I went through those it would jog my memory. Tischler would be your best man on that because he's here. He was the chief honcho for that work at that time, and I think he'd be a better source. What is it you want to know specifically?

QUESTION: Almost anything, as a matter of fact.

QUESTION: We have to reconstruct the whole story.

QUESTION: One of the things that John and I are interested in, too, is where the technology came from, say, for the J-2. Did it come out of Lewis? Did they borrow from Pratt and Whitney? Did they pick up stuff from the H-1 unit? Could it have been on the J-2? What kind of design problems did they get into, and how did they resolve them, and this kind of thing. Almost anything you can remember about this particular time.

MR. SLOOP: The background for hydrogen/oxygen is fairly broad. You are familiar with the work that went on at Ohio State.

QUESTION: I wanted to ask you about that, Johnston. Do you want to start with him? Could you tell us more about him?

MR. SLOOP: Johnston was a very brilliant professor who did excellent work in cryogenics, in hydrogen liquefaction. That was his forte. He had a number of students, graduate students, and he had quite a sizable cryogenic laboratory going. His design was used by Aerojet for their hydrogen liquefaction plant. And you are familiar with this recent history that came out on Aerojet?

QUESTION: Yes, three guys, I had forgotten.

MR. SLOOP: I read the damn thing. I thought it was a lousy paper. I had to read to the last page before I found what I wanted, namely when the hell they ran their engine, and my own notes were better than that because I said in a paper in

1963 that they ran their engine in 1949, 3,000 pounds, and the thing that disappointed me is that the prime mover in Aerojet hydrogen work was a man named Dave Young -- not Robert Young, David Young -- and he's referenced in the paper in the end but I couldn't find his name anywhere in the paper.

Coming back to Johnston, he was strong in thermodynamic properties, hydrogen, liquefaction of hydrogen, and things of this sort. And at Ohio State they had a German, Rudolph -- I forget his last name -- he was a very good kineticist in combustion. And Loren Bollinger, who is deceased, was the prime mover in their little rocket lab. They built a little rocket lab. The rocket lab was established about 1946. In fact, I've got a paper somewhere of the history of the Ohio State rocket lab. It's only a few sentences, but it says it was established in 1946 and apparently I picked it up on a trip to Ohio State. I saw their rocket facility, and it was due to primary student power and labor, and so forth. It was small-scale, but as I recall, they operated then. They wrote a paper in 1952, I think, or 1953, and I ran across it while I was writing this paper. Let's see if I can find it. Didn't I reference that in here?

QUESTION: Johnston is on page 33, 184. 1951.

MR. SLOOP: I haven't read that paper in a long time, but that might give you a good lead. I ran across it in doing research for this paper. They ran oxygen/hydrogen prior to '51, at least that is one of their reports. That will give you leads to others.

QUESTION: Okay, so much for Johnston. He came up to Cleveland then and helped you do some of your work?

MR. SLOOP: No, we always looked upon Johnston as a source of -- well, we negotiated with him to get some liquid hydrogen. He was liquefying hydrogen and we were trying to negotiate to have it hauled in a Dewar by truck from his facility to Lewis for our test and it never came down. But he was a complex individual and was also consulting, was strong in hydrogen liquefaction process. His design was used by Aerojet.

I guess the first experimental hydrogen/oxygen that I became aware of was in 1945 or '46 when there was a sweat cooling symposium at Wright Field, and at that symposium Dave Young gave a paper on -- it seems to me it was a 1,000-pound or 500-pound thrust hydrogen/oxygen rocket. It was a tiny thing. It was built just like this, simple, and if I recall he hadn't run it, but this was his design. This was part of the same contract that is well-recorded in this history we referred to, under Navy sponsorship, and they ran a 3,000-pound thrust rocket.

Incidentally, I thought they ran the rocket pump and thrust chamber at the same time, but I never could find that fact out from that paper. All I got was the two had been operating. But I think they did try to cobble them together and claim that they ran in order to satisfy the requirements. They had a lot of trouble. So Aerojet was strong and JPL did a little work on hydrogen.

Those were the only ones I recall.

QUESTION: Did you ever meet Walter Stethalter at Rocketdyne? He was at one time one of the program managers on J-2. I was curious about his background.

MR. SLOOP: By the way, Sam Hoffman, former president of Rocketdyne, was with me on this panel. He's retired now, and he'd be a good source of information because he was in the company during the time of the J-2 and this key phase. And this chief engineer -- his name escapes me -- would be another excellent contact.

QUESTION: Brennant, Bill Brennant, by any chance?

MR. SLOOP: I think Brennant came later.

QUESTION: Paul Castenholtz?

MR. SLOOP: He came later, too. I'd have to look at my papers.

QUESTION: What were the special design features of the J-2 as you see it?

MR. SLOOP: Well, the thing about hydrogen/oxygen that each investigator found out was it's a damn easy combination to run. There was a lot of psychological fear of hydrogen from its explosive characteristics. The flammability in oxygen is from 4 to 96 percent, and there was a great deal of reluctance to work with it, and of course the zeppelin experience had everybody psyched out on the stuff. But once you started using it, it became amazingly simple, easy to burn, easy to cool, and I think each person finding this out -- in other words, it's hard to foul yourself up when you're working with hydrogen.

To go to the other extreme, one of the worst combinations man has ever had to fight with is nitric acid/gasoline. This is a bastard combination. The injector is critical and everything about it is so critical. Hydrogen/oxygen, on the other hand, is really a nice combination to work with. Of course, we were after hydrogen/oxygen but we had our sights set on fluorine/hydrogen, and when we got around to hydrogen/oxygen it was a piece of cake, after all the difficulties we had with fluorine.

QUESTION: Did you get much specific impulse by going to fluorine rather than oxygen?

MR. SLOOP: Yes, you get --

QUESTION: -- carried out to make a whole new system to do that, because you already had --

MR. SLOOP: The answer to your question is historically it obviously is not because you've got one and you don't have the other. But fluorine had two things going for it: One, its density is one-and-a-half times that of oxygen; two, you use less hydrogen. Therefore the density combination is greater. And three, it's spontaneously ignited. So we felt these were good factors. The trouble with fluorine is it is too active a reaction for common use. But all these things help. Its actual performance in terms of specific impulse is not that much greater. It is some. It's, as I recall, 15 or 20 units, at altitude, it goes from 420 or so specific impulse to 450. We actually did some experiments where we added a little bit of fluorine to the oxygen and made it hypergolic for ignitions, used fluorine as a starter. (Inaudible)

So those were the incentives. But the stuff is just too tough to work with.

QUESTION: Would you say once you got the liquid oxygen pretty well down to it would be easy to work, just because you had cryogenic technology. (Inaudible.)

What I'm trying to say, wasn't the real big step in liquid oxygen technology and not necessarily hydrogen technology?

MR. SLOOP: Well, I'm not sure that that's true, although liquid nitrogen and liquid oxygen have been around a long time. There's a hell of a lot of knowhow in handling it. I think that handling liquid hydrogen wasn't all that tough, but there was a great deal of fear of explosions and other things. As I say, I think people were psyched out against it, but they found it much easier to use than liquid nitrogen.

QUESTION: When did you begin to make that psychological turnaround?

MR. SLOOP: Well, I guess when we started actually using it. We went to a hell of a lot of precautions and we found they were useful, although hydrogen has a wide explosive limit, it's also light and diffuses light real rapidly, so it's not too easy to get a hydrogen explosion, and some of the fires we did have, or explosions, were not too great in severity.

QUESTION: You did have some, though.

MR. SLOOP: Yes. For example, if you vent hydrogen through a pipe, as we did -- say you want to depressurize a tank; you just open the valve and vent it to the atmosphere. Chances are excellent that you will get an ignition and a boom from it. We did it several times. The reason for the ignition was a number of possibilities. One was just static discharge.

QUESTON: (Inaudible.)

MR. SLOOP: You see this facility. This was built for fluorine as well as hydrogen, and this is about 25 feet in diameter, and 20,000-pound thrust engine, sits down at the bottom, and discharges, and this is sort of like a big U. All these are water sprays to absorb the hydrogen fluoride in the . It goes to 50,000 gallons of water. We discharge this whole tank through here in one minute. We can hydrogen/oxygen at one time. The idea is that we catch all of the hydrogen fluoride before it escapes to the atmosphere. You have a great big mesh, a screen-type mesh; right here at the top is sort of a screen, just hundreds of spray stations.

One time we were running we had an explosion I guess around in here, and that damned mesh thing floated up just like a flying saucer. It didn't go up more than about 30 feet.

(Inaudible.)

QUESTION: It's not easy for me to really understand, I guess, the relationship of a place like Lewis to industry. The fact, you know, that they can all come in and get this information now. I guess what I'm getting at is where does a lot of this stuff really begin? Maybe it's hard to say who really comes up with the idea because there is such a mixing at this point in a center.

MR. SLOOP: Well, part of the answer to what's bothering you, to go back to the reason why NACA was established in 1915. It was established by the Government to study the problems of flight with a view towards a practical solution. But one of the basic reasons was that a lot of this research required facilities that were so expensive that they were beyond the reach of any individual company, and therefore the Government as a whole would build these enormous wind tunnels and millions of dollars worth of facilities and do fundamental work in aerodynamics and propulsion and so forth, make it available to U.S. industry in general and benefit them all. In other words, a Government laboratory exists for the benefit of all the people, and therefore our information is public property. We do it not for individual use or special use but for the benefit of all. Also NACA did not have big contracts with industry as does NASA. We didn't develop them. We were more purely on research. The primary contract work was with universities, and these labs we were not in competition with. We were a corollary or complementary to their needs. For that reason they came to hear our information, and we had research conferences. It's an old tradition. Like there's a picture of a research conference of NACA in 1933, and about all the big names in aeronautics are gathered in that picture in 1933, to hear the latest results in aeronautical research during that time period, and it's this kind of tradition and involvement.

Now, when we got into this business, as I mentioned in this paper, we got into it through the back door. First of all, there were pretty high-level management in NACA that didn't think we should be in rocketry, so we were named the high pressure combustion section. And we found as Johnny-come-latelies that we were way behind JPL and others in many of the aspects. They were the big wheels in the rocket field. So we tried to choose a field that wasn't so well plowed, and also one that was fairly close to our capabilities in propulsion, and so on and so forth. That goes quickly to liquid propellants. And then we decided to try to jump over everyone who was working on things like -- there was a lot of attention on alcohol and oxygen and things for missiles for the military, and acid and gasoline. We decided we would try to leapfrog that and go into high energy propellants, and that's why we got interested in boron and ammonia fuels and hydrogenous fuels and chlorines and ozone and liquid oxygen mixtures. We did probably more work with this at that time than anyone else. We weren't the first, though, in any of these. For example, North American and a man named Doyle did work on chlorine and gaseous fluorine and hydrogen before we did. Bell Aircraft was not too far behind us. They worked some. The Air Force got interested in it from the work we were doing and sponsored the work at Bell.

QUESTION: As I recall, you got some of your supplies from Air Force plants, hydrogen, for example.

MR. SLOOP: That's a good story and one that I don't know too much about and want to trace down. The Air Force got real hot on hydrogen for high altitude bombers, and I think that's the reason they built the Air Force plant. They had Johnston working with it. Also, some of the equipment we got surplus from the Air Force that was Johnston-designed was connected with the atom bomb. We didn't know a damn thing about that except that here was the equipment.

QUESTION: He was involved with the Manhattan project for a long time.

MR. SLOOP: He must have been. I think that's where he got his biggest support, but I don't know.

QUESTION: One question that came to mind that I'd appreciate your comment on, if you have time, Mr. Sloop. That is, one of the arguments against the fifth orbital rendezvous was the inflight refueling of the liquid hydrogen. Do you feel that that could have been safely done or do you think that would have comprised a very high risk effort?

MR. SLOOP: I think at that time we would have considered it a fairly high risk effort. We still haven't done that.

QUESTION: What about now, do you think it could be--

MR. SLOOP: I don't think it's too tough a problem although we are proposing for fiscal '73 an experiment to find out, and that means we have a way to go.

QUESTION: Do you feel we could have made it to the moon had we opted for the   approach rather than --

MR. SLOOP: I think we could have; Ithink we could have gotten it either way.

Are you familiar with this? Haveyou seen this? It's marked confidential but it's been declassified.

QUESTION: I don't know. John has been working more on the early areas there.

MR. SLOOP: Now, Canright, there's a man you should talk to, Richard Canright. Have you run across him?

QUESTION: He's out at Douglas?

MR. SLOOP: Yes.

QUESTION: He retired just about the time we got out there. He was either on vacation or out of town or doing something.

MR. SLOOP: Canright was familiar with our work from being on our subcommittee, and I think he was one of the prime movers in starting Saturn, the first stage of Saturn, but not necessarily (inaudible.)

QUESTION: Let me ask you, again from your work with liquid hydrogen, do you see any major differences between the RL-10 and the J-2 engines, each having a kind of distinct personality. I realize the fuel feed systems are somewhat different. Are they really two terribly distinct propulsion systems?

MR. SLOOP: I think the RL-10 definitely influenced the J-2. There's no doubt in my mind of that. And the people who did the J-2 had a certain amount of access to the information on the RL-10. Just how much I don't know. You'd have to ask them. There's a hell of a lot of exchange of information even though there were proprietary rights --

QUESTION: It's interesting that they did share a lot of the information. Was that a friendly basis?

MR. SLOOP: Yes.

QUESTION: They were engaged in a national program of sorts, and they all felt the commonality of goal then?

MR. SLOOP: Yes.

QUESTION: Would it be possible to borrow these two items. You say we have a copy in History.

MR. SLOOP: I wouldn't want to give you these until I get them properly declassified. I could get that, I think.

QUESTION: We'll be around again later this afternoon.

We've taken a good deal of your time and we appreciate that.

I'm interested in Johnston. John and I are both from Ohio State, and I thought I'd like to maybe do a little bit more about Johnston and that rocket thing down there. Maybe I can get in touch with you again a little later on. If you find that early history thing, I can ask you about it.

MR. SLOOP: You know, Johnston was a very interesting type character. I remember one of his graduate students named White. Johnston would work the tail off of his graduate students and they would carry, say, 95 percent of the work and the initiative, and the final dissertation or paper, who was the senior author? H. L. Johnston.

QUESTION: Do you recall any of those graduate students by name who subsequently went to Rocketdyne or to ABMA?

MR. SLOOP: White's name I recall but I don't know where he went after that. Loren Bollinger, you know, was shot to death by a robber at Ohio State. He was in Columbus, set up a little consulting firm, and saw me the week before he was shot. He was still in the rocket field, consulting in combustion and so forth. This happened about three or four years ago. He was accosted at night on the stairway in Columbus.

QUESTION: Could you make a statement about the work at Ohio State in kind of an historical context someway comparing it to JPL and Aerojet and the work at Lewis?

MR. SLOOP: Of the three I would rate them this way:

I believe that the Aerojet work was by far the most heavily financed, therefore the biggest effort; there were more people, more horsepower put onto the job at Aerojet. The work at JPL was very small, and the work at Ohio State compared to it was very small. The Ohio State work was done more as an adjunct to educational purposes, I believe, industrial development. But they had a bona fide rocket lab. I visited there and saw it and they had some unique types of equipment. It seems to me that they were one of the first to use strain gauges to weigh tanks as a means of determining the propellant. And if I recall, they also used -- I think they used a flexure plate to do it. We used that idea for a measuring of thrust. I think we swiped it from -- I can't recall who did it first. But a lot of people used ball bearing parallelogram type of thrust structure, with ball bearings here and here and here, and this is fixed and this can move back and forth, and you use that to measure thrust.

It seems to me that I recall that Ohio State used a flexure plate to weigh their tanks, and we adapted that idea to replace the ball bearings. That was one of the unique instrumentation techniques, and there's fairly good literature on this. I don't think you'd have very much trouble tracking it down. If you get that report that I referenced here -- that's number 7 -- I'm sure it will lead you to others. If you look up Johnston in chemical abstracts you will find about 150 or 200 references because he was a prolific man. He had a lot of good graduate students.

I may be crossing your path on that because I am intending to enlarge this a little bit. If there's anything I run across I'd be happy to give it to you, and if you learn more I'd like to hear.

I kept some notes of this and I've got them somewhere. I'm going to look them up. I wrote a paper on hydrogen in 1963 and I don't know if I referenced it or not. But in there I did look up the notes and that's why I wrote about Ohio State, and so this time I went back to try to find sources, but you'll find I think quite a good bit of literature built up there.

QUESTION: Before we leave, there's one other kind of informal question I wanted to ask. I know you've been with NACA for a long time, and I've been playing with the idea of a biography of E. P. Warner. Did you know him at all, have any contact with him?

MR. SLOOP: Very little. No, I had none with him. I'm trying to -- where did I run across Warner?

(Whereupon, the interview was concluded.)