NASA HEADQUARTERS NACA ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

ROBERT C. HENDRICKS
INTERVIEWED BY REBECCA WRIGHT
CLEVELAND, OHIO - JUNE 3, 2014

The following information is from an oral history session conducted on June 3, 2014, with Robert C. Hendricks in Cleveland, Ohio, as part of the National Advisory Committee for Aeronautics [NACA] Oral History Project, sponsored by the NASA Headquarters History Office. The interviewer was Rebecca Wright, assisted by Sandra Johnson.

SUMMARY

A look at some of Robert C. Hendricks's experience with aerospace engine development at the National Advisory Committee for Aeronautics [NACA] from its beginnings through the years to NACA's transition to the National Aeronautics and Space Administration [NASA] is presented in an interview-style format.

INTRODUCTION

This report is based upon an interview conducted with Robert C. Hendricks, a Senior Technologist at the NASA Glenn Research Center in Cleveland, Ohio. Although at first he thought he was "not going to be here very long," Robert Hendricks has been with this facility since 1957. This report presents his experience with the development of rocket engines, focusing on his time at the NACA Lewis Flight Propulsion Laboratory.

Today's NASA Glenn Research Center started out in 1942 when the National Advisory Committee for Aeronautics started building its Aircraft Engine Research Laboratory [AERL] in Cleveland, Ohio. In 1947 the lab was renamed the NACA Flight Propulsion Laboratory to reflect the expansion of the research. In September 1948, following the death of the NACA's Director of Aeronautics, George Lewis, the name was changed to the NACA Lewis Flight

Propulsion Laboratory [LFPL]. Then on 1 October 1958, the LFPL was incorporated into the new space agency, NASA, and it was renamed the NASA Lewis Research Center [LeRC]. Following John Glenn's flight on the Space Shuttle, the Center name was changed again on 1 March 1999 to the NASA Glenn Research Center [GRC].

Robert Hendricks began his lifetime involved with aerospace engines while studying aerospace engineering at the Ohio State University [OSU]. When there in 1952, Robert's introduction to aircraft engines came when he was hired for the summer as a draftsman for North American Aviation [NAA][1] in Columbus, Ohio [where he worked on the F-86, AJ-2, F-51, and FJ-4 aircraft over a period of three summers]. The summers of 1955 and 1956 were spent at the NAA-Rocketdyne and Rocketdyne Canoga Park facilities in California working as a Junior Engineer on the G-38 and Redstone, and other classified rocket engine designs.

Robert learned how rocket engines[2] worked from the Rocketdyne Rocket Engine Course, where instructors Dean Dentry and Ennis Staggers taught him rocket engine components and integration design. Also, discussions with a number of NAA Columbus engineers taught him drafting/engineering and gave him firsthand knowledge of fighters and attack bombers up close on the assembly line.

The experiences at NAA, Rocketdyne, OSU, and eventually at NACA/NASA have been the backbone of Robert's professional development.

---

[1]North American Aviation was a major American aircraft manufacturer, founded in 1928. In 1955, it formed Rocketdyne as a separate division, and in 1967 it merged with Rockwell of to form North American Rockwell, later to become Rockwell International.

[2]A jet engine generates thrust by pressure differences, discharging a fast-moving jet formed by a chemical reaction, usually combustion of jet-fuel with air. A rocket engine generates thrust using stored propellants, a fuel and an oxidizer, for its reactions and generally no external air; a rocket engine can operate in both space and an atmosphere.

The remainder of this report will follow the course of the interview to present Robert's experience at the NACA [and later with NASA], presenting his involvement with aerospace engine development.

INTERVIEW

WRIGHT: We understand that you have been at GRC for many years, but you started your career here in 1957. Can you tell us how you became part of the employee workforce here?

HENDRICKS: It was in 1957 between my OSU graduation and U.S. Air Force assignment that summer. I looked around at various options I had, and most of the options were such that they wanted long-term employments. The salaries weren't much better, but what they offered here [NACA LFPL] was essentially a continuation of my education: namely, that I'd worked at Rocketdyne and NAA. I worked at NAA in Columbus for three summers, and I worked for Rocketdyne for two summers. I worked at both of those places, so I knew rocket engines quite well, and I knew a little bit about airplanes, too.

When I came here to the LFPL, George Kinney, Jr., interviewed me, walked me around, and told me about some of the things that were being done here. The interesting part was they offered short-term courses—what they called internal courses—that were taught by people here at the lab who were experts at what they were doing. They wanted you to know how to do what they were doing. That did interest me, so I came here, first of all, as an NACA employee, and then eventually the Air Force stationed me here. That was principally the reason I wanted to come here: It was a short-term appointment. I said, "Okay, I'm not going to be here very long." Needless to say, I'm still around.

WRIGHT: What was your long-term goal at that time [late 1950s, early 1960s]? What did you want to do after you finished the short term?

HENDRICKS: Rocket engines: I wanted to design them, build them, and so forth. We did it at Rocketdyne. That was sort of an interesting series. When I first started at Rocketdyne, they were building rocket engines by hand. We built them in the shop. We designed them in one room, and we built the rocket engines in another area of the shop, so you could go from one place to the other. At both NAA-Columbus, Ohio, and Rocketdyne, most of the designers were on the drafting boards, and we had experts who checked our work. Design changes were issued as Engineering Orders [EOs]. These changes [EO-changes to the working drawings were made after a problem was discovered during manufacturing and incorporated into the blueprints] had to be accurate and specific for those on the assembly line building the airplane or rocket engine to understand and implement.

The NAA planes were fighter aircraft; pilots and our Nation depended upon them to perform as intended. At both NAA-Columbus and Rocketdyne, you were turning whatever your concept was into actual hardware, right there. At NAA-Columbus, experienced test-pilots flew rigorous test flights to ensure performance. At Rocketdyne, they performance test-fired those rocket engines up at Santa Susana Mountains in California, and that was an experience I'll never forget. We were in the mountain in the block house, and when they fired that G-38 engine, which was 136 000 pounds of thrust each barrel—and there were three barrels in that thing—that whole mountain shook. Everything just shook. I was awed by the power, and I just couldn't believe it. That was a small rocket engine compared to the stuff that came afterwards.

When I was still here at the NACA LFPL [early 1960], I went back to Rocketdyne to meet with my old boss, Dean Dentry. Dean showed me the F-1, which is the million-pound thrust engine they were developing. Rocketdyne had a lot of problems with it. Here at LFPL/NASA we were studying combustion and instabilities and other rocket engine components that contributed heavily to resolution of the F-1 problems [the development of any new engine design with a multitude of interacting components all coming together into a functioning engine of 1.5 million pounds thrust; simply stated, no one had ever built an engine that big, turbopumps, nozzles, thrust vectoring, engine externals, injectors to cite a few]. So we helped out a little bit on issues relating to the injector and combustion instabilities. I had the opportunity to share our findings with Dean Dentry. The 300,000 pounds didn't seem like very much compared to that 1.5-million-pound thrust engine. My unique and exceptional experience at NAA and Rocketdyne became my portal to NACA LFPL.

When I was here at LFPL, we were talking about small engines because they were the type of engines with 5000 to a possible 20,000 pounds of thrust. They were really small, but the fundamentals were pretty much the same. Instabilities occurred in the engines, and combustion was a big problem, feeding the propellants through the pumps, pressurized tanks, all the safety issues, and things like that. Propellants became a big factor. I worked at Rocketdyne on a small engine, what they called a rocket-assisted takeoff [RATO], on an FJ-4 aircraft. It was a Navy aircraft, and they wanted to get it off the deck in a hurry and so it was a small engine. I came back from Rocketdyne [to NAA and to finish school at OSU] and I thought, well, I would be an expert in that engine, but NAA had an expert in the Rocketdyne-RATO engine, so it was kind of interesting in that the experience sort of set the stage for these types of smaller engines that we had here at the NACA at the time.

WRIGHT: Was it a time period of evolution for these types of engines?

HENDRICKS: Yes, because you were talking about the V-2 rocket, which ran about 50,000 pounds of thrust, and of course the V-2 is very famous for its destructive capabilities in World War II. The other thing is that they didn't know quite how to make these big engines stable. Scaling was a problem, and as the rocket engine industry and NACA/NASA moved forward in the rocket industry, that became sort of a known fact. When Rocketdyne was building the F-1 engine, they used to have huge instability problems that had to be solved. We were working on the same types of problems at the NACA, so that fed into the whole scheme of development of scaling up from small to something big. At a million-and-a-half pounds of thrust, the throat of that F-1 rocket engine was huge.

WRIGHT: About how many feet, do you think?

HENDRICKS: About 3 feet for the nozzle throat diameter, something like that. The F-1 was big, and the injector head was also big, with anti-oscillation baffles and lots of injector ports. If you stand beside it, it's like looking up 15 plus feet. The Saturn rocket had five F-1 engines on it when it launched the Moon vehicle, so there were several of them in there.

WRIGHT: What did you go to work on first, when you first got here? What were some of the first projects?

HENDRICKS: Yes, that was interesting, too, because the NACA-Air Force started out with the NAA X-15 engine. The X-15 was an Air Force aircraft that they couldn't get to operate very well, so NACA took it over. It was quite secret at the time, as to what we were doing here. Yet one of the first X-15 [man-rated] engines was so safe, it wouldn't fire. That was not a very good engine. Then, the NACA and Air Force got the RMI, Reaction Motors Incorporated, to develop a spherical-head engine, which was like that of the V-2, in a sense, as it looked a little bit like that. That had a lot of combustion instabilities, and that was the first thing we began to work on—the LOX-[liquid oxygen-] ammonia-type engine.

It was one of those things where the pilot Scott Crossfield crawled down under the B-52 wing, into the X-15. He sat with the liquid ammonia and LOX tanks right behind him, and he fired that engine when it dropped. It was an extreme risk, and I don't know how he ever had the guts to do that, but he did. They flew it to the edge of space, and there were a lot of problems with stability there. It tumbled, and of course, some were lost, but they eventually got that all fixed up. That became the forerunner of the mechanics that enabled Rocketdyne to eventually build the NAA-Rockwell Space Shuttle vehicles and the engines for NASA.

It was a good experience, and one of those things that led to a big conflict between my military assignment at Wright-Patterson Air Force Base and my assignment here at the NACA. Colonel Leslie Pattillo was the Air Force liaison at the NACA at that time, and Colonel Pattillo apparently had more clout than the Brigadier General down at Wright-Patterson: I was AWOL [absent without leave] for almost a week, and Pattillo told me, "You're going to stay here."

I said, "No, I've got to go back to the Air Force or be considered AWOL down there."

He said, "You're assigned here." There was a distinct fight between Pattillo and the Brigadier down at Wright Field, and Pattillo won. I stayed here and worked on the X-15, and so that was my first assignment.

WRIGHT: That's quite an assignment to start out with, isn't it?

HENDRICKS: It was an interesting one, for sure. We wrote reports, and I'd never written a report like that before. It goes back into 1957. The interesting part about that rocket history is that when I was at Rocketdyne for the first year, at the Slauson Avenue facility, which was in 1955, designers' meetings would be a gathering of about a half dozen people. I had one drawing board, so, diagonally, one drawing board away there were gathered about half dozen people. We were working on the Redstone Engine, and some of the G-38 hardware, and also the old V-2 [or upgraded Redstone] hardware. My workstation [a drafting board] was adjacent to this gathering of world-renowned-group of scientists—I later found out they were Wernher von Braun and his associates—who were discussing satellite type of projects, that included an upgraded version of the Redstone engine.

Rocketdyne upgraded the Redstone Engine-to the point where it would launch a satellite. This was 1955. In 1956, the vonBraun group had the launch capabilities/capacity available to launch a satellite, but we weren't allowed to do it. When I came here to work at NACA [June 1957], I also learned about some of the missions that we were doing around here, undercover. Namely, that we were looking at going to Mars, we were looking at going to the Moon, all these trajectories and different things. Wolfgang Moeckel was the guy who was directing that, and

Robert W. Graham was my supervisor and a very good guy, a very strong supporter, not only of my work, but of the NACA and NASA work—very strong person

The point was that we were doing these things undercover. Dr. John F. Victory, was at NACA/NASA Headquarters as some associate director or something like that. Hugh L. Dryden was supportive; he was the NACA Director. He and Graham had a very good relationship, so there was a lot of work there that was done. NACA/NASA's Dr. Victory said that Congress would not hear of us working on satellites or anything to do with going into space. He said that was distinctly a no-no, you cannot say anything about that. That really surprised me because we had the capability of launching that vehicle; we had the capability of launching it in 1955-1956 with the Redstone, and of course, in fall of 1957, that all changed very rapidly.

All of a sudden: "Why aren't you working on this?" We didn't tell them that we had been working on this, but the thing that surprised me is that they decided they needed to go to this Vanguard missile rather than the Redstone missile because the Redstone was already available to do that; the Vanguard was not. Vanguard had several failures, but the Redstone always fired and went off. There's a lot of early experience in there which I didn't agree with, but I was too far down the totem pole to do anything about it. I didn't know who to talk to. The people who were gathered at that table at the NAA-Rocketdyne Slauson Avenue facility in the summer of 1955 included none other than Wernher von Braun, and I didn't know that at the time.

We had some good support out of the Marshall Space Flight Center [Huntsville, Alabama] in the first years at NASA [i.e., when we transitioned over]. That NACA-NASA transition happened very rapidly, too, much more rapidly than what one would think. The NACA people, at that time, dismantled, for some reason, the jet-engine turbine and compressor group, and there were some excellent people in that group, a world-renowned turbine and compressor group. Some of those people were put into our rocket section. Even Doc Graham had two or three people, including Don and Eleanor Guentert, who were from that group. We were working under John L. Sloop at the time as part of Dr. Walter T. Olsen's division when Dr. Edward R. Sharp was LeRC's Director.

There were some really key people that came out of that group, but NACA had enormous experience, or enormous clout—I would say clout—worldwide in that area. It may not be realized that none of us had much experience in jet engines during the World War II, except for the Germans, and they flew those jet aircraft. That was really a surprise. Had the Germans had a few more months, they might have gained air superiority. It's hard to see how they kept it up, but they did. Fuels were a problem for them. The Germans seemed to make them during the war. We learned a lot of stuff from them: for example, rocket engines, propellants, and then rocket flight dynamics afterwards and jet aircraft.

WRIGHT: Based on what you say, you chose to come here because it was going to be short term, but did you know a lot about NACA's philosophy and how the employees worked, the organization, basically the whole philosophy of NACA? Did you know a lot about how the organization functioned?

HENDRICKS: Before I came here? I didn't care. It didn't bother me a bit—I wasn't going to stay here very long. Besides, I was an Air Force guy, anyhow, so what's the difference? The Air Force would be moving me anyway.

WRIGHT: They were going to move you, huh?

HENDRICKS: Yes, the Air Force was going to move me, anyhow. I found out that the NACA had a philosophy of teaching you: "We want you to know how this works. We want you to know this." NACA would take time to do that and that was valuable to me. NACA and even early NASA had courses we went through to learn how to work instruments, learn how a boundary layer works, and so forth. We had to learn all kinds of things. The guys that were doing the work were the guys that were teaching us what they wanted us to know so we could help them and do their work. It's not the case anymore—they don't do that now. We had "organizational meetings;" granted, they were in the evening, and the whole lab was invited. Everybody from the lab went—not everybody, but there would be several hundred people who would gather in the Administration Building auditorium. There, you discussed things. Those things that didn't work, you wanted to know why they didn't work—maybe we can help you out. We don't do that anymore.

WRIGHT: Like an open forum, whatever was in your area?

HENDRICKS: Like an open forum, yes. You learned a lot from those people. Those people had all the experiences, and it was a big, huge learning curve. I couldn't have gotten that kind of education anywhere; no place in the world—no university—had that kind of expertise. Talk to Simon Ostrach, he'll tell you: we had the greatest things since sliced bread, but those outside NACA/NASA had nothing like that. Ostrach was here, part of that teaching group, and we had some of the finest people in the world here.

WRIGHT: They must have believed in sharing knowledge if they were such great teachers.

HENDRICKS: They didn't have any qualms about that. They'd work right along with you. I worked with the people in the instrumentation group, and we had technicians who were top notch, absolutely top notch. You'd give them a sketch of what you thought you wanted to do, and a couple of days later, they'd call you and say, "Here, is this what you want?" "Wow, yes, that'll work." We had machine shops here, we had people who had fabrication machine shops, which were like the same thing they had at Rocketdyne. People built things and actually made them work. We had metallurgy shops, we had everything here—all gone now.

WRIGHT: Were you able to work side-by-side by these technicians, if you needed to, to have something changed?

HENDRICKS: Yes.

WRIGHT: Was it kind of a partnership?

HENDRICKS: Yes, and the same way with the guys in the machine shop. All the people worked as sort of an integral unit. At that time, we didn't have this business of "what's your WBS [Work Breakdown Structure] number," and "I'm not going to work on this because you don't have a WBS," or anything like that. They were always interested in what you were doing and why, you talked to them about it, and they didn't just make what you wanted, they made it better. When you did your experiments, they wanted to know what you did and how it worked and all that type of thing—how can we make it better for you? Always, it was always the same way. It's all gone.

WRIGHT: You stated about writing a report, in 1957, of the work that you did on the X-15. Talk to us about how important those reports were, and what all it took to put those reports together that were passed on and passed through to other people.

HENDRICKS: It's very important to document what you do and what we did at that time. So people have probably said, "That's rudimentary; we know that." Yes, they know that now, but they didn't know that, then. We didn't know it, then. The idea of the NACA reports were very precise; namely, that you had to be able to express yourself enough for other people to duplicate what you did and get it right. If others couldn't duplicate what you did, then you did the experiment wrong. That was what was going on.

You had to take a lot of pains in writing your reports. They had to be written very carefully. We had editors, we had secretaries that helped us type, and things like that. We had technical people that did drawings for us, or we did our own drawings and they made them look good for reports. Stuff that we messed up, they would fix up. It was always one of those things where you continued to contribute. We always had big fights among the scientific people about how to express this stuff, and "Why did you do this?" and "Why did you do that?" "You didn't do this," "You didn't do that," and "Go back and rewrite this." You'd probably write three or four drafts before you ever got the thing into the technical editors, and then they would go through it, too.

We used to have committees of people who would check these things out very carefully. We weren't permitted, at that time, to publish in the scientific journals, and so forth, because they weren't considered accurate enough. They weren't considered to be complete enough. My opinion right now is there is much in the journals materials that cannot be reproduced. There's no way you can do it. They've become pretty much academic, but they are journals, and that's where people say, "Okay, you've got to publish or perish," and that type of thing. The NACA wasn't that way. It was published here as a NACA document, and that's going to be the record—*the* record—and anybody else has to be able to reproduce it.

WRIGHT: I imagine there's a library that you went to, to read everybody else's?

HENDRICKS: The library was our key, and that was one of the major things we had. The library was upstairs in the Administration Building, and I'd spend time up in there. The management welcomed that. You would go to the library and spend some time. "Okay, you're not working on our project." Okay, so what? You are learning. You're learning. We had a man who did technical translations. He was excellent. He would take some of those old German reports and translate them into English so we could take some of the Russian reports [we had some technical translators do the Russian], but mostly German reports. Excellent guy. He would take care of the library and was very interesting. We'd spend time in the library—the library was a key thing. That's gone.

WRIGHT: As I visualize what you're describing, it was learning in a very supportive knowledge exchange, information-sharing time. At what point do you feel that started to move away more from the research and the knowledge-building into maybe even development?

HENDRICKS: As soon as NASA decided to get projects people involved. As soon as they decided they were going to split the research and project off into project offices. That happened, of course, after the Apollo shots. I will say, after the Shuttle became operational, after something like that, it was one of those times in a frame period of, "Okay, NASA, what are you doing next? Why aren't you this?" Budget cuts became one of those things where, "You got too many people here," "You got too many people there," "We've got to look into your budgets," "We've got to make sure that you're not spending this or that," or something like that.

Some of the oversights of people who were outside the Government, wanting to get on the gravy train of the Government, and we called them Beltway Bandits. They were all around Washington, D.C. They're still all around Washington. A lot of the NASA Centers' staff became contractors rather than civil servants, which was a big mistake. Huge mistake. Great for the contractors because they all made a lot of money off of it, but as soon as that happened, I don't know, that spells the demise of the exchange. The things changed, at that time.

WRIGHT: After it became NASA, you got really involved in working with liquid oxygen, is that correct?

HENDRICKS: Hydrogen, yes.

WRIGHT: Can you share with us those early days, of when you were starting to work with that, and then how that evolved through the years with what you did?

HENDRICKS: Of course. There was a lot of work that was done prior to that, which I guess I could have known about when I was at Rocketdyne, but we were working primarily with rocket propellants [hydrocarbons] and LOX. When I came to NACA, we were primarily still working with hydrocarbon rocket propellants. We were working a little bit with hydrogen, but not too much. As I said, the first I was to tackle was the X-15 engine, which was LOX-ammonia. From that particular project, we said, "Hey, maybe we should be going to space." I knew people like Vearl N. Huff very well, who knew the thermodynamics and the propellants and all that type of stuff; Doc Walter Olson and Bob Graham; and others like Del Tischler, Richard S. Brokaw, Richard J. Priem, Gerald "Jerry" Morrell, Sanford Gordon, Frank Zeleznik, Bonnie McBride, Erwin Zaretsky and all those people who worked so well in this area. They all had a good idea of maybe some of the rocket engines, but the propellants were one of those things—and particularly liquid hydrogen was one of those things—where we didn't have a good grasp of what it could do. We theoretically knew that it was very good propellant fuel, as long as it was with a good oxidizer.

At NASA LeRC, Howard W. Douglass started investigating hydrogen-fluorine engines, and I don't know if you ever smelled fluorine or not, but in parts per billion, it smarts the nose. I remember walking along the road between where I was testing, which was a high-energy fuel lab, and the old NACA rocket lab, and they had fired a hydrogen-fluorine engine. The smell of fluorine could be detected through the scrubber and everything else in the area. The smell of fluorine, it smarts your nose real quick. Just a little bit of fluorine would be devastating. The thought of firing that thing off at Cape Canaveral, Florida, and having an accident down there—you would devastate the whole state.

It was one of those things where there was a mindset of, "Well, we need to get the best, the highest specific impulse." Yes, the hydrogen-fluorine gave you a higher impulse, but not that much more than hydrogen-oxygen. It took a proven hydrogen-oxygen flight—the flying of the B-57, the Canberra, over the lakes—and Abe Silverstein convincing Wernher von Braun that, "Hey, look, we've flown this stuff." "You've flown this stuff?" [Paul Orden and others were heavily involved in that early work.] Then, we started in with the liquid hydrogen in earnest at that point, or I would say very close to that point.

It followed directly, the next assignment I had was to determine how to handle liquid hydrogen and its heat transfer properties; that sort of followed the X-15 assignment. Since I was still at NACA/NASA, still an Air Force guy, I had a little bit more latitude in going around between people, different divisions, and so forth, and could take flights on the C-47 and go down to Wright Field and hook rides out of there, or whatever, to wherever, when Colonel Pattillo was still here. It was a good relationship; we could fly right out of the NACA/NASA hanger apron.

We built one of the first heat transfer rigs [experimental test systems]. It was a low-pressure rig, operating on what we called an HLJ Dewar [Herrick L. Johnson Dewar], which was a small liquid hydrogen Dewar transfer buggy. We built that system, well, in less than a year, and had it operational. We did it out at the old high-energy fuels lab, which is Building 51 [Area 51...Ha!]. We had grounded vent stacks [electrical], but our cardinal rule was "no arcs, no sparks." We had a very successful trip through all the history of that development of the liquid hydrogen from the word "go." We had one stack fire, and that was some 20, 30 feet above the roof, but that's when lightning hit the stacks.

WRIGHT: Didn't have a lot of control over that one, did you?

HENDRICKS: No, we had purged stacks, and we purged it with gaseous nitrogen, and it went out pretty quickly, so we didn't have any problem with it. That series of HLJ-tests became the baseline report for two-phase flows and somewhat for designs towards rocket engines in the low-pressure region for liquid hydrogen. Doc Graham [Chief] was the supervisor for that. Bob Ehlers and I worked the X-15 LOX -Ammonia project and then we started with Bob Friedman with the liquid hydrogen Dewars. I would guess we gathered and published the data, which surprised me, to find out that all our NACA/NASA reports, which were pretty closely held, weren't secret, but most of the community didn't know about NASA reports. When I became Vice President of the International Institute of Refrigeration in 1975, I chaired a session at the international cryogenics meeting in Moscow, Russia. Afterward, one of the Russians came up to me and said, "We know all about your work."

I said, "How did you find out?"

He said, "We've read all your reports." The Russians not only acquired those reports, but they knew all about what we were doing. It was an eye-opening experience.

WRIGHT: Very surprising. How long were you part of the Air Force?

HENDRICKS: About two years—about a year-and-a-half or two years, something like that. Then I was part of the Reserves for a long time after that.

WRIGHT: You were somewhat still affiliated with the military when you went to Russia?

HENDRICKS: When I first started, yes. When I first started, as I said, Colonel Pattillo was still here at NACA and he got me assigned here as NACA personnel, and then they transferred me over to NASA.

WRIGHT: Were you still part of the Reserves when you went to Russia for that meeting in 1975? That'd have been 20 years.

HENDRICKS: No, I don't believe so. I think I'd signed off, and got an honorable discharge. They said, "Well, you're not keeping up with your active duty, so we'll give you a discharge if you want one."

I said, "Good. " So they did.

WRIGHT: Talk to us a little bit more about that. From what I understand from what I read, the work that you were doing certainly played an important part in the Space Shuttle Main Engine [SSME].

HENDRICKS: Not only just the Shuttle, but more importantly, all the upper-stage vehicles. Everything that the Apollo did and the Shuttle, certainly, but the Apollo couldn't have gone where it did without the upper-stage vehicle, without upper-stage hydrogen. None of the upper-stage research vehicles will go any place without the liquid hydrogen.

We learned a lot. We learned that liquid hydrogen has ortho and para compositions, [ortho: aligned spin states, para: opposite spin states] and you have to have it stored with para, otherwise it boils off and you don't have anything. If you don't keep it in what we called MLI, or multi-layer insulations, that was a development problem, too. I didn't work on the MLI so much as we used it. We used different types of insulations and things like this, so we developed insulations. We developed sealing. We developed methods to transfer the liquid hydrogen. We developed Dewars, storage methods, and safety procedures—big time.

We knew a little bit about safety. We had one accident that I do remember quite well, namely, that we were hooked up to a liquid hydrogen Dewar—a big one—and they blew a gasket on one of the seals in the transfer lines. At that time, I was working in a cell adjacent to our work cell 103, and I was recording, at that time, the power levels. The other people were up in the control room, and all of a sudden, I couldn't find anybody in the control room [there was no walkie-talkie radio contact]. I said, "Is anybody there?" Nobody was around.

I was looking around, and saw this cloud of stuff on the outside. What's going on here? I walked into the cell to find out that the cell was filled with some type of stuff. It didn't really dawn on me too much what it was, but I knew I had to shut that Dewar off. Otherwise, there'd be mess everywhere. I did shut it off, and I had to go in and shut the valves off on the Dewar transfer [the big transfer] tank. I shut those off, and of course, then the leak stopped. Hydrogen is very forgiving if you don't have sparks or arcs. Very forgiving because hydrogen rises, and so, even though it was very dense when cold and would come down, it would always rise as it warmed up. That convection of the upward flow brought oxygen along with it. I wasn't in any danger, other than the fact that if I had had static electricity or something like that, it would have been not so good.

I came out and found out the fire department was just sitting outside, where the 10 x 10 Foot Supersonic Wind Tunnel is now, probably, maybe a good football field and a half, something like that, from the cell where the leaking transfer tank was. The fire department was sitting out there, waiting to come in, if needed. All the guys that were mechanics and everybody else was standing out there with them. I went out and I said, "What's going on, here?"

Bob Friedman says, "Well, there's a big spill in the cell. We can't go in there."

I said, "Well, I got it all shut off."

He replied: "What are you talking about?" It was quite an experience.

WRIGHT: Yes, for everybody. Was there industry, people like Rocketdyne or other people that were working on the same thing you were?

HENDRICKS: Sure. We worked with Rocketdyne and we worked with Pratt & Whitney, Aerojet and, mostly with Rocketdyne. Not only because of my own experience with those people, but Aerojet and Rocketdyne were competitors. They would swap people back and forth to acquire secrets, and that's how they got information transferred. A few took advantage and would hire in at a significantly raised salary, and he'd transfer over there, and that type of thing. Rocketdyne had, I would say, a cadre of just absolutely excellent designers, fabrication techniques, and test facilities. They built the big engines, and they built the J-2 engines for the liquid hydrogen engines. The RL-10s were built by Pratt & Whitney, and that was liquid hydrogen. The big, key issue is that they built a lot of these things and they worked, but they didn't know why they worked. The things that we added were not only the smarts to understand why they worked, but also the handling characteristics and all the heat transfer information designed into them. Unfortunately [or fortunately], it became designed into the Russian vehicles as well.

WRIGHT: How did you share this information with them, or did they come to you?

HENDRICKS: Who?

WRIGHT: With Rocketdyne and Aerojet?

HENDRICKS: They would come here. That's the time that contractors came here. We had some contractors that worked with us and some contractors that worked for the Air Force or someplace else, and they all came here. They built their engines and we told them all; everybody that came here basically got the same type of information. There wasn't a barrier between this contractor and that contractor like you have today. It just didn't exist. If it did, I didn't know about it.

WRIGHT: Your information that you gathered here was shared to those who were using it.

HENDRICKS: Yes, it was widely shared.

WRIGHT: Looking back on the years that you spent working with that, what did you find to be the most challenging time of working with that whole project, and all those elements? Other than the time when everybody was standing outside. What was the most challenging part of working with that?

HENDRICKS: It was a challenge to try to understand why things were working the way they were because we had never seen this before. There was stuff that we had never seen before, which is great, but when you'd run across it, you'd try to explain it and couldn't explain it. It took us a long time and a lot of effort to try to understand it. It was frustrating in some respects, but challenging in every other respect.

We had a lot of support from our technicians, and as I said, we'd try things and something would go wrong. I remember we had technicians that would stay with us all night long, when we ran tests. If we had problems, they weren't about to say that we have to go home. They were there, and they'd go fix the problem. They would be right there with us. For example, we had liquid hydrogen in little tiny tubes on the side of our liquid hydrogen transfer devices, which were hard to see. We had guys that would come into the cell after we had trouble with the experiment, and they'd take their little torches and they'd touch those things up and put them back together again. It was just amazing, what these people could do.

WRIGHT: Was there ever a time that you felt or that you thought management was going to abandon this project, or did it feel like everybody was on board to support it to the end?

HENDRICKS: I can't remember that. We had a lot of good support at that time because people didn't understand what was going on, and we were coming up with new stuff all the time. I remember that Abe Silverstein was supportive, but he didn't interfere. Graham was leading things, and I don't know, it just worked out well. I was probably ambitious, too, but aggressive. I wanted to know why it worked.

With the Lord's help, I could make things work, and that was one of the things that I would say enabled me, with respect to the technicians and the machinists, I'd go talk to them and tell them about what we wanted. I knew what their problems were and how they could do it, or might try to do it. Like I said, they always made it better.

WRIGHT: At some point, you started doing other things, like I think I understand you do some things with alternative fuels? Is that where you have moved, segued into?

HENDRICKS: That was a long time afterwards. We ran ceramics, and we ran thermal barrier coatings. Thermal barrier coatings were a big part of our stuff in the rocket industry, too, because at Rocketdyne, we had run what we called a thermal barrier coating inside the engine, in order to keep it together. It was just too hot. Some opportunities came to work on some of the thermal barriers, and I worked on those as well, ceramic coatings. The big issue was the seals in the Shuttle vehicles.

Otto K. Goetz [Chief Engineer] came up from NASA Marshall; he personally came up and wanted to talk to me. He didn't want to talk to anybody else—he came personally to me and said, "We got a problem with our Space Shuttle Main Engine pumps." They'd run them and blow them up, run them and blow them up, and run them and blow them up. He said, "We don't know why." He built some equivalent pumps and some simulators. They were exact same size and everything as the pump, but the pump wasn't very big. Huge power ratio in those pumps. After I toured around with him, I told him what I could do, I told him what we thought we could do for him in the vehicles, and he said, "I'll send you the casings, you run the tests. I want that data." We found out that a lot of the seals could be unstable.

George L. von Pragenau down there at NASA Marshall also got in on the program. He came up with an idea that the instabilities were at a certain stage in the operation of the pump and how the seals, then, and bearings were causing all the problems. Zaretsky did a lot of work on the bearings. The rotors were just eventually rubbing themselves on the casings. We had three SSME seals sent by Otto that we instrumented: one was a straight, one was a three-step, and one was a conventional one that Rocketdyne had designed for the pumps, which was what we called a labyrinth. I found out that the labyrinth was unstable.

We didn't have a rotating system, we had a static system, but we just knew what the flows were doing. We highly instrumented everything and found out that that seals could be the problem. Then, we found out that if they did a straight seal, that had much better stability and the three-step became a hammer. Namely, if you begin to move it off-center a little bit, it would pull things back in a big hurry. They began to put all that, put all that information together, and with that information, they finally began to be able to make the pump stable, and run stable. Without that, they wouldn't have been able to do it. There was a big transition between what we could do in heat transfer because we knew the knowledge and so forth and what we did with the fluid dynamics in the Shuttle pumps and the instabilities in those pumps. The Shuttle was flying because of that.

WRIGHT: When you were asked to take a look at that from Mr. Goetz, did that become your priority project at that time?

HENDRICKS: I just told him I'd do it.

WRIGHT: You continued doing everything else you were doing as well? Or was this something that got put at the top of the list because of its nature?

HENDRICKS: It became a priority item because—let me back up—Abe felt that we had run as much hydrogen as we probably could or should. I ran also liquid methane, and things like that, in that tank. He probably felt that we had enough data to do the rocket engine designs, so he was looking to try to wind down the thing anyhow, and that probably precipitated the changeover. I told Otto, "This is something that's very important and I can do this."

He said, "Do it," and so we did it.

WRIGHT: Do you remember what timeframe that was?

HENDRICKS: I don't know. It was early in the Shuttle development, where NASA was very early in the Shuttle engine developments, where they were having all kinds of trouble. The NASA Marshall-Rocketdyne test crews blew them up, one after the other, on the test stand. They just couldn't keep the pumps together.

WRIGHT: What other type of work did you do with that program, the Shuttle Program? You mentioned this one—is there other aspects that you were able to lend your skills and talents to, as well?

HENDRICKS: This is getting way far into NASA.

WRIGHT: It is, but we're here.

HENDRICKS: Once we got, in my opinion, the Shuttle stuff straightened out—because there was a long effort in that we'd had several meetings with people from all over the free world who were looking at instabilities. The French were heavily involved in finding out what we knew, and eventually they built their own engine, as you probably know. Also, they built their own launch vehicles from that type of stuff. They piggybacked off an awful lot of our data in order to build their systems, so that they now have a competitive launch system. I was really surprised at the Russians because you couldn't get any information in or out of that place. I worked with the détente committees [President Richard M. Nixon Détente Agreements with USSR].

WRIGHT: I was going to ask you that—I saw that on the list. Share with us how you got that opportunity to do that, about the détente committees, how did you get involved with that?

HENDRICKS: I was handling the Cryogenic Engineering Conference for a long time. Bascom W. Birmingham, at that time, he was the director of NBS [the National Bureau of Standards], now it's NIST [the National Institute of Standards and Technology]. Bascom, apparently, knew what I could do from a lot of meetings, and he wanted me and Dave Daney [the NBS advocate] to go to be a part of this détente committee on superconductivity. I thought, "Well, okay." We went over and we began to work with the Russians as well as at NBS on cryogenic properties, cryogenic heat transfer, and all that kind of stuff. Then, of course, that's all part of that détente agreement; they would come here and work at our labs, then we'd go there and work in their labs.

We went to Russia and worked in their labs. It was a very interesting experience. The Russians didn't take notes. There were maybe 30 of them in the room, something like that. We had our translator, and they had their translator. Our translator was invited to sit in the back of the room. Their translator was going to do all the translating. They took no notes—I was the only one taking notes—as to what they were going to do, when they were going to do it, and what they thought they could get out of it. They took no notes.

The translator told me later, "They didn't translate that the way you said it." I said, "Interesting." I listened the next time, and the next time I was looking at them they're still not talking notes. I would ask questions and make sure that I understood what they were doing. By the time we left, everything that they said they were going to do, they did. Everything that they said they were going to give us, in terms of material, data-wise, they did. We walked out of there with more than we ever walked in with. We got a cultural experience with those people: They quit at five o'clock, and everything shut down, just like that. It's now time to go to the circus, it's now time to go to the symphony, it's now time to go to the ballet, it's now time to have a big dinner, and so forth. Whatever it was—whatever the situation—that was their time with us because they had the State approval, diplomatic immunity, if you like, to do whatever they wanted to do. They had carte blanche. They had the money, they had the approval, so they would take us everyplace. That was their way of getting their perks out of the system.

We also learned, while I was there, about the Baseball coil magnets. They took us there, to show us some of the Kurchatov Institute Lab, and I said, "Interesting, what's that up there?"

"Oh, that's our magnet." I knew all about the magnet because I knew that the U.S. cryogenics industry and government groups built it here in the United States, they transported it there in the C-130—a huge airplane—and they rolled the truck transporter out of that plane. They opened the cargo bay and the semi rolled off that platform with that magnet. The Russians just had their mouths wide open. The Air Force and cryogenics groups took that from Sheremetyevo airport, which is way out, drove it down the big highway that interconnects Moscow and the airport straight into Moscow and into that lab. People saw that thing all the way, with American flags on it. It was quite an experience. I didn't witness that, but the U.S. group told me about it. The Russians were extremely impressed with the work that we did. They flew us out to Novosibirsk also,[3] and they made sure that we met with Samson S. Kutateladze, who was *the* expert in the area of heat transfer.

He has several books out, but Kutateladze was one of those people—I would say he was as much an American as he was a Communist. He was head of the Government out there, so he had to excuse himself one day. He said, "You must excuse me for I now have to go make bureaucracy." Just an amazing guy, but he sat there and he told me at dinner, during the 1975 visit, "Look, you and I are alike. We have the same ideals. Same background. Watch out for the Chinese, they'll overrun you." They've got a population 10 times ours, they'll overrun us.

WRIGHT: It's an amazing time for you to be there.

---

[3] First visit to Novosibirsk during 1975 International Institute of Refrigeration Moscow meeting, arranged with assistance of Prof. Maurizio Cumo, University of Rome. Novosibirsk gets warm enough to water ski in summer, very cold in winter, meter of ice with golden autumn, and the birch trees turning between. Beautiful.

HENDRICKS: Détente was quite an experience, and we learned a lot. As for those people who went there with the CIA [Central Intelligence Agency], the CIA wanted to know everything you did when you came back. The CIA wanted you to act as one of their agents. I said "no." I said, "I'm not going to do that." Those people who did got in a lot of trouble. They didn't get any information from the Russians. We did, we had no problems. They knew who we were, we knew who they were, they wanted to know more about our life, we wanted to know more about their life. We formed a bridge. I hope it eventually led to the bridge that I still hope exists.

WRIGHT: It did. Do you feel like you were able to apply any of the knowledge you brought back?

HENDRICKS: Yes, because we learned a lot about the problems with superconducting transmission power transmission lines. The biggest problem is that they carry a lot of energy. If they go normal [what we call normal; namely, they become like an ordinary conductor], what do you do with all that power? Where does it go? It's just going to blow things up. You have to be very careful about how you put things together, where they are placed, how they operate, and things like that. There are some superconducting lines now, and the temperature has been able to come up, but not like what was promised. It was always promised, "We'll run these cables and you won't have any power transmission line problems." We learned a lot.

WRIGHT: Your short-term assignment, or your short-term position that you took when you came here, lasted a number of years. You've been here 50?

HENDRICKS: Yes, 55 years I guess, since 1957, yes.

WRIGHT: Are you still working on new and exciting projects?

HENDRICKS: Yes, I can't tell you what I'm working on right now.

WRIGHT: That's great to know you're working on new stuff.

HENDRICKS: It's related to energy, as you might expect.

WRIGHT: I was going to ask Sandra if she had a couple of questions for you.

JOHNSON: As you mentioned, you're working on things you can't tell us about, and the stuff with the Russians. At the very beginning, with the X-15, and those other things you said that had been worked on that were somewhat secret, did you have to have any special clearances? Or because you were in the Air Force, did you already have those clearances?

HENDRICKS: I did, but strange enough, when I came here to NACA, they did the clearance again. We were pretty particular in the early days, anyhow, about your clearances. I found out a lot later in life, that it wasn't just my grandfather that they interviewed. It was the neighbors. They interviewed all the neighbors near the farm, around the farms, around the neighborhood down there in Worthington, Ohio. They told me about it later, but I didn't know about it then. I didn't even learn about a lot of this stuff until after most of them passed away. At funerals and things like that, people said, "Do you know that so-and-so—?" I didn't know that.

JOHNSON: Do you think that was a common experience for most people that were working at that time on all projects?

HENDRICKS: I don't know that. I don't know, but I was surprised that not only they interviewed my family and associates and people that I was familiar with, and references that you put down, but they went into the history, family history, and where you're from and what you were doing and how you worked or didn't work, and what kind of person were you, and could you be trusted. All that type of thing.

JOHNSON: That's very interesting.

HENDRICKS: I had secret clearance—I still have it—but I didn't want top secret. After I found out what was in secret and why it was secret, I didn't want to know any more about it. There's a lot of stuff you don't want to know. You don't want to know. I'll just say that.

JOHNSON: I believe you. There's a lot I don't want to know.

HENDRICKS: I thank the Good Lord that we still have a country where we're able to worship God and some people don't. There are a lot of people that don't. I still have a clearance, but I don't use it much anymore. I shy away from classified projects as much as I can.

JOHNSON: Thank you for answering that, I appreciate it.

WRIGHT: I was reading some information that was out on the site, one of the comments that was made in this article was that you have been quoted as saying that you're "driven by the applications of my research." Could you explain that to us?

HENDRICKS: It's kind of hard to explain. I guess, let me put it this way: if you don't know something and you want to find out, or if you don't know—it's maybe the inquisitive nature of humankind—I want to know. I want to find out, if I can find out. I want to know what this is—why does this do this? Why does it work like that? I'd like to find out about it. I think that's what you're asking, maybe?

One of my sons has what I call original thought, and the other one is more like myself. I don't know where John gets his original thought from, but he comes up with something after he thinks about it for a long time—why didn't I think about that? We used to call him Lucite because he'd sit around and he'd watch us work, he'd watch us paint and stuff like that, and Lucite was the work skipper—I don't know if you remember that ad or not—he wouldn't work until he had an idea of what we were doing, and then Lucite [John] would say, "Well, why don't you do this?" It was sometimes humiliating.

The boys and even the girls, they still have that type of inquisitive spirit and individual talents, "Why do you want to do that? Why?" Maybe that's the thing that gets to me, and I want to know. If I can make it work, oh, my gosh, you can't believe how thrilling it is to have something that nobody's ever known, come to light. I think that's the thing that really, really pierces and paces me right now: nobody has ever known this before, and here it is.

WRIGHT: Your lab was like your laboratory of discovery. I guess you kept discovering new pieces?

HENDRICKS: Yes, we were able to do that. It's much harder, now. It's so much harder. We've got people—I don't know, at one time—I guess, I'd have a few administrative people looking over to find out what I was doing, and every time they would put a roadblock in my way. Graham had left. He retired, and it was just so many people putting roadblocks in your way. Then again, it was budget, one of these things driven by Congress, and Congress, well, I won't say that they were the perpetrators of the things, but we had so many oversight people that it was just difficult.

When you work with something, you have a feeling of whether it's going to work or not going to work, and what's safe or not safe. Say I were describing to you that I had to go in and turn the valve off on the hydrogen tank when there is just hydrogen blowing all over everywhere and you're in the cloud of hydrogen, and you would say, "Would that be safe?" It wasn't whether it was safe or not, it was that you had to do it. You read a lot about soldiers and things like that. They just had to do it. It wasn't something that you wanted to do, necessarily. It just had to be done. A lot of that stuff had to be done. I came up many times with new things, brand-new things, things that nobody had ever seen before. That was the impetus. It keeps you going.

WRIGHT: That's the excitement. Were there other things that you thought about that you'd like to share with us today, before we close?

HENDRICKS: I don't know—it's been a good experience with NACA, and knowing NACA, and knowing how they operated, and the people who are involved with NACA. They rolled over into NASA, early NASA. It was quite a thrilling experience, and the projects that I've had to work on, they've always been challenging. More than challenging, they've been presented opportunities, I've gotten a lot of honors, and I used to travel a lot. I don't like to travel anymore. It's really a harassment, to travel. I imagine you get into that, too. I would say that it's been a good time.

Right now we're getting, I would say, a lot of good support. We have enormous oversight, and that I don't appreciate. Things that you want to do, you can't get done. There's little or nothing here anymore to enable you to do that, like the machine shops and the technicians and the support people are scarce. It's so hard. You have to almost go outside, find somebody outside, as a contractor, to do it. It's not right.

I don't know what will become of the organization, but I'm too far along in my career for them to worry about or for me to worry about. I long since learned that I have to chart my own course, independent of the management. If some of the management doesn't like it, well, okay, they're not going to like it. There have been a lot of managers that have not liked it, and they've been very difficult to deal with. So difficult that I don't deal with them.

WRIGHT: Sounds like you have a good plan.

HENDRICKS: I have found this one thing, that when I hand a plan to management, and management rejected it flat, absolutely flat, I knew I was on to something. That's what I pursue. I guess I had to be turned down absolutely flat in order to make it work.

WRIGHT: Thanks for that lesson. We'll hang on to that. Thank you for coming and spending time with us today. We appreciate it.

HENDRICKS: You're welcome.

CONCLUDING REMARKS

This interview reveals a personal account of a parcel of the career of Robert C. Hendricks at the NACA and his involvement with early aerospace [rocket] engine development and progression.