# NASA HEADQUARTERS NACA ORAL HISTORY PROJECT
# ORAL HISTORY TRANSCRIPT

ROBERT W. GRAHAM
INTERVIEWED BY SANDRA JOHNSON
SAN JOSE, CALIFORNIA – 30 SEPTEMBER 2005

JOHNSON: Today is September 30th, 2005. This oral history session is being conducted with Robert Graham of Fairview Park, Ohio, as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project sponsored by the NASA [National Aeronautics and Space Administration] Headquarters History Office. This interview is being held in San Jose, California, during the NACA Reunion XI. The interviewer is Sandra Johnson.

I want to thank you for coming by today and spending time with us to share your history. I'd like to begin today by asking you how you first learned of NACA, the National Advisory Committee for Aeronautics.

GRAHAM: Well, I'm a native of Cleveland, Ohio, and, of course, we knew of the NACA Laboratory, so when I first finished undergraduate work the summer of [19]'48, I was employed for the first time at Lewis [Flight Propulsion Laboratory, Cleveland, Ohio]. My first assignment was with some studies of axial flow compressors. The Center was just beginning to convert from a reciprocating engine research to jet engine, and they were doing some very basic work in the behavior of axial flow compressors. I did that for three summers, actually. The rest of the time I was at Purdue University [West Lafayette, Indiana], teaching and taking graduate work. So it was kind of interesting. I had to resign several times and be reemployed every summer that I came back.

Then when I was at Purdue University, the last two years I was there, I was finishing a doctorate, and I was working on a contract that Lewis had with my major professor, Maurice [J.] Zucrow, Dr. Maurice Zucrow, who was on one of the subcommittees in NACA, in fact. When I finished, although I interviewed at various engine companies, General Electric and Westinghouse, he wanted me to come back to Lewis, and so I was pleased to do that. So I actually began full-time continuous employment in January of [19]'53.

Then I joined a section headed by Harry [Harold B.] Finger, who's here, and it was in axial flow compressors of the Compressor and Turbine Division, headed by Mr. Oscar [W.] Schey. I studied a phenomenon in compressors called rotating stall. That was an issue of uncertainty in the behavior of engines during their stall phases, when airflow is cut on the engine, what would happen to the aerodynamics over the compressor blades. What happened was there would be zones of high and low flow that seemed to circulate around and rotate at part of the speed of the compressor, and they could be disastrous.

In fact, I had a disaster in one of my test rigs, where I actually lost all the blades in the research compressor because of running at stall conditions, rotating stall conditions, where it's off-design [operation] of the compressor, but the engine has to go through that when starting it and stopping, or maybe throttling.

What may be of interest to you is that in—now, this is the period of, say, from [19]'53 to [19]'56, and in [19]'55 I went back to Purdue University to recruit. In fact, I went several times to that university to recruit and also to the University of Illinois [Urbana, Illinois]. But on this particular occasion we were advertising—I should say the agency was advertising—for a test pilot for the X-15, and luckily, I had somebody respond at Purdue University. He was named Neil [A.] Armstrong. So I have the distinction of the first approval of this young man to be part

of NACA, and of course, he came to Cleveland first before coming out here [California]. So we have a continuing occasional communication, and I've seen him a few times recently, too.

Then in [19]'56 I decided that I would like to go back to the rocket work, which I was doing. I don't know if I mentioned that. It was rocket heat transfer studies that I was doing with my major professor, and it was white fuming nitric acid as a coolant for rocket engines, and that's what I did my doctoral thesis on. So I decided that perhaps I ought to go back to that and actually the monitor of my thesis work from Lewis was Dr. Walter T. Olson, who happened to be the Division Chief in the rocket area. They had just a Rocket Branch, maybe about thirty people, and I joined. John [L.] Sloop was Branch Chief. You may have heard of him. He eventually went to Washington [D.C.] when NASA formed.

I started then in [19]'56 to go back to rocket work, and I was assigned as a lead person in a rocket tunnel. This was a tunnel that was hooked up to a rocket engine and it would act like an aerodynamic tunnel would act, except that the driving gas was the rocket exhaust, which happened to be [from] ammonia-oxygen propellants. It ran about at approximately, I think, Mach 6, in that tunnel. Of course, we would operate only for a few seconds. But I did some heat transfer studies of various shapes in that tunnel.

There was a kind of another spin-off of that. The X-15 was propelled by ammonia-oxygen. So we did some, what we called spud [evaluation]. They were the injector of the engines for the X-15 was in kind of a what they called a spud configuration, like a shower head or something, and we were asked to use that rocket engine that drove this tunnel to also do some of that research [for] an optimum engine. Bell Aircraft, I think it was at that time, was responsible for the engine.

Then in [19]'57, back in the NACA days, they had what they called inspections, annual inspections, and Dr. Olson assigned me to be the lead person in the inspection of the new high-energy rocket facility in South Forty at Lewis Research Center [Cleveland, Ohio], which was just recently dismantled. So we had the inspection, and it was during that period that the Russian Sputnik went up. In fact, it was in the October of that year.

Some of the NACA officials, and maybe I shouldn't name them, but John [F.] Victory, the Secretary of the [NACA]—a fine man. He was a little bit critical at our rehearsal about what we were doing to show off this facility, because we had built a kind of a showplace outside of the rocket facility. On it was actually structural steel, and then overhead we had a panoramic view of the sky, and we were talking about how high energy would enhance our possibilities for payloads. He was a little bit afraid that the public wouldn't appreciate that, because public people are coming and congressional people are coming. I think he was a little bit afraid that perhaps that would be too strong a story, too Buck-Rogery.

Well, so he advised us to take down some of the stars and satellites that we had up there on this panoramic [sky], and talk about, more about, ballistic trajectories. Well, after Sputnik we didn't hear anything more, and we didn't take down our exhibits. In fact, we enhanced them and even put a Sputnik up there to show people where it was going. And we demonstrated some high-energy conditions and actually demonstrated the use of fluorine and showed what fluorine did to materials.

In a way, it was perhaps a little bit scary way to demonstrate some things, but we did it, and nobody got hurt or anything else, and it became kind of a featured showplace for a while. And, of course, we showed what the [possibilities] of the new facility would be.

So I was one of the first officeholders attached to that facility. They had a building specially built for control, and it was the office building for the South Forty Rocket Facility.

Now, when NASA was formulated, then shortly after, there was a policy—I don't know who instituted it; probably Dr. [Abraham "Abe"] Silverstein or somebody—that he wanted to indoctrinate certain people in the Laboratory with some of the rocket technology, and we had in-house courses and a number of things at Lewis at that time, and up even while we were NASA, where experts within the Laboratory would speak or teach classes. So this was an expansion of that. So we had classes in rocketry, and I was charged with organizing that. I just did part of the lecturing, and while I was involved in that, that was pretty much a full-time assignment for several months for me.

So that's kind of my history in NACA. Oh, one other thing, one other item that I should mention. I was made a Section Head at that time, about [19]'56, [19]'57, and looking at high energy, we, of course, felt that hydrogen would be probably the fuel of the future. Of course, Dr. Silverstein was a very strong advocate of hydrogen and had demonstrated hydrogen use even in airplanes as a fuel. So I think he was a deciding upper management person that promoted hydrogen for upper stages in Apollo, when he was temporarily assigned to Washington when the manned space program began. I guess he and [Wernher] von Braun had some issues on that. You may have heard of that. But he, [Silverstein], was convinced that we needed hydrogen for the payloads that were going to be in the Apollo Program, and he, of course, was a man, as you'll see in that document, that named the Apollo, the Apollo.

Well, because of that evidence of interest in hydrogen, I decided that I think we ought to study hydrogen as a coolant. Hydrogen's properties were not well known, and just what state the hydrogen would be used was also a question, but it was agreed that it would have to be stored in

a liquefied fashion, which, of course, was a very, very cold cryogenic. So we simulated heat transfer in the cooling tubes of rocket engines by developing a rig which had a single tube, which was heated electrically to high temperature, and then we had a supply tank of the liquid that was pushed up by pressure up through this test section, which was very adequately instrumented with thermocouples and pressure taps. So we got some early data on what hydrogen's heat transfer characteristics were over a range of pressures, from, when it was two phase, up through near critical to very high pressure, up to 2,000 pounds.

So that was before NASA, and of course that project continued very active when NASA was formed. Then when the RL-10 engine built by Pratt and Whitney, the first hydrogen-oxygen engine, which was then used on the Atlas/Centaur. We were working very cooperatively with Pratt and Whitney, and feeding them raw data, actually, from our test rig. There was a lack of confidence as to what would happen in the throat region of the engine with high-energy fuels. It proved out that because of a number of things, physical [properties] of hydrogen, the curvature effects in the engine at the throat, that the engine could be cooled, and, of course, it was.

But it was a basic study, and that put us into basic studies of boiling and near critical [states]. The critical point is where the fluid is neither gas nor liquid, but it undergoes very dramatic changes in properties, all kinds of—like conductivity and viscosity and, of course, density. That was a big question, because we thought we might be doing cooling around the critical point of the fluid. So that became a major project, and we spent several years, actually, in that area, overlapping into NASA.

So I developed a very fine fundamental heat transfer group that looked at problems primarily that were related to the cooling of the engine. We studied geometric effects, gravity effects, as well as the property effects of the hydrogen. We did a lot of studies of boiling,

because the mechanism of boiling was one of the big questions. We actually took pictures of hydrogen under boiling and hydrogen boiling under multigravity. We didn't manage to do it under zero gravity, but we had plans to do that, and we lost the facility, an airplane that we were going to do that in.

So that's my story of my NACA career, and since that time I've done a lot of heat transfer, basic heat transfer and fluid mechanics work, in my group. During the crisis, the first fuel crisis, you know, we did a lot of work on alternate fuels, and I personally was involved in research with some other people in my section on using agricultural crops for generation of methane and oils.

JOHNSON: Well, if you want to go back for just a minute, and let's talk about some of the time when you first started at NACA. You mentioned that you worked there in the summers. When you were working and going to school and then working during the summers, before you started working at NACA, what impression did you have of what was going on out there? As you mentioned, you lived in the area, but what impression did you have of Lewis, or at that time before it was Lewis—

GRAHAM: Well, it was kind of exciting. I thought it was exciting, and I was majoring undergraduate in what was a heat power option at Case. When I graduated, it was Case Institute of Technology [Cleveland, Ohio]. When I started, it was Case School of Applied Science. My education was interrupted in World War II. I was away for three years in the Army. Interestingly, I ended up with a technical group that was attached to the Ballistic Research Lab

[laboratory] at Aberdeen Proving Ground [Maryland], so I had a good experience in the Army in research in this group.

JOHNSON: After the war, obviously, the situation with the United States globally and the Cold War beginning between the U.S. and the Soviet Union, was there a feeling when you first began working full-time again—well, during the summers and then when you started working full-time—a feeling of needing to catch up with what Germany had done during the war and trying to push that forward in the United States, that research?

GRAHAM: Yes. We had one very famous German come to Cleveland, Ernst R. G. Eckert. He was a heat transfer expert and eventually went back to teaching and became professor at [the University of] Minnesota [Minneapolis, Minnesota]. I think he's still living, as far as I know, and he may be. He must be ninety-five or something like that. So, yes. And, of course, we wanted to make sure that people realized, also, that liquid engine rocketry was pioneered by Dr. [Robert H.] Goddard.

JOHNSON: As far as the community around Lewis, what was the perception of what was going on at NACA? Did they have any idea of the type of work that you were doing there?

GRAHAM: I don't think so. That's been one of the problems that we've had. This is why we had the Speakers Bureau.

JOHNSON: So they weren't aware of what you were doing.

GRAHAM: And Cleveland, the Cleveland Laboratory hasn't had, I think, the press that some of the others have had, or, I think the political backing.

JOHNSON: When you first started there, it was around 1948, is that correct, the summers?

GRAHAM: Yes.

JOHNSON: And then you began working there full-time after that. Can you describe the facilities and what it was like when you first started working there, as far as the buildings and what you had to work with and the areas you worked in?

GRAHAM: Well, the test procedure was vastly different than we do today. Everything is electronic today, as far as receiving and operating on data. In those days we had walls of manometer tubes, for instance, with my compressor readings. They had walls of manometer tubes, some of them were mercury, some of them tetrabromide; probably fluids that we couldn't use today. But anyway, the way of taking a data shot was to take a photograph of these tubes, so we would get pressures where we had instrumentation, and then we had computers, which were women, who would read these tubes. They were given calculational procedures on how to process that data and give us information back.

So you'd run a test maybe for several hours, and you'd have these films, big camera with big sheets, big pieces of film that were used to photograph these things. Then these women had the tedious job of, with the magnifiers, reading the data and then transferring it to calculators,

which at that time were Monroe calculators, mechanical devices. So we didn't know how a test ran maybe till a week or two weeks later.

JOHNSON: How closely did you work with the women, with the computers, as an engineer?

GRAHAM: Quite closely. One of the first ladies I had became the first Division Chief at Lewis, woman Division Chief. She was in charge of computing.

JOHNSON: What was her name?

GRAHAM: Peg Yoner [phonetic]. But they were wonderful people. You talk about women's work, in those days it was a kind of a separation. Women also helped us with report writing, like even that report that I gave you. I had an editor that worked with me on that to put it in a proper format and make sure language was correct, grammatically correct, and everything else. So I always had high respect for these people, and the mechanics, technicians. Engineers couldn't do research without the aid of these people, and I let them know that.

Now, it's also interesting, you asked me about my acquaintanceship with that area. I used to go to the National Air Races when I was a kid, and the site of the National Air Races became the site of Lewis. I'm talking about the twenties and thirties, because next week I'll be eighty-three, so I've lived through an interesting era. If you read in that thing, you'll find something of my feelings about that place and its relationship to the air races, and some of the people that I got to know. For instance, I had the pleasure of having lunch with Jimmy [James H.] Doolittle once. He was visiting the Lab. And as a boy, I saw him race in the Thompson

Trophy. So I had an emotional attachment to the place because of my boyhood interest in aviation and going to the air races. That was the big event of the summer as a family, because my mother would pack a picnic and we'd go. I lived way over in the east side of Cleveland, and this was the west side, and it was quite a trek in those days in my father's 1927 Buick. [Laughter] There weren't many roads from the east side to the west side without traffic signals or deviations and that sort of thing. It wasn't a straight highway excursion.

So I had a lot of interest in feeling them out before I joined, and I thought that was a wonderful place. At that time there was a feeling about civil servants being—you know, a civil servant job was an honor. Today I'm afraid that has disappeared. I could have made more money going to industry than I did starting at Lewis, but it was the attraction of the academic atmosphere. Well, in fact, when I went there permanently, I said I'd maybe stay five years and I'd go back to teaching. That was the only reason I got a doctorate was to be qualified to get on a college staff.

Well, I never did. And it was great; it was really great. Wonderful people, very challenging projects, a lot of freedom to institute your programs, and while you had to sell them to the management, even as a Section Head, I had to report directly to Abe Silverstein when he was Director, and every year I'd have a presentation to give him on what we were doing and what we expected to do. He had a remarkable memory. I was embarrassed one time when he remembered something I had done the year before, and I had forgotten to really, in that presentation, to relate to that.

JOHNSON: And he remembered.

GRAHAM: He remembered. He was a genius, really an exceptional man. Sometimes could be pretty rough with you, but he loved his staff, and he was a visionary. I wish NASA had more of them right now.

JOHNSON: You mentioned the Rocket Engine Test Facility and being officed there. The wind tunnels and that facility was all being built at that time. Did you have any interaction as far as the design or how the engine test facility was being built?

GRAHAM: No, that was really a little bit before my joining the Rocket Branch. They had a big struggle to get money to do that, and so it was people in that branch before I joined it. I think my major professor had a big influence on it, to them getting it, as a member of the subcommittee that he was on. Zucrow wrote one of the first jet propulsion and gas turbine books, which included rocketry; textbooks for graduate study. When I started with him, it was notes that he gave us, and eventually it became a book.

JOHNSON: Can you describe some of the experiments that you ran? I know you mentioned the early ones with the photographing the tubes and all that sort of thing. But when were the tests run? Were they run during the day, or was notice given that the tests were going to be run so that the community or the surrounding area would know what was going on? Or maybe if you could just describe a typical test or different types of tests.

GRAHAM: Well, the tests that I'd run were a pretty small prototype. The control room wouldn't be maybe quite as big as this room, and then we'd have the test facility over in the—and maybe a

bullet-proof window to look in, and the cameras and so forth. The manometer tubes would be in the operation room, and the camera, so we'd click the pictures as we went from test point to test point. But the major facilities, they were very careful to build mufflers, because some people in the community objected to the noise. But the major wind tunnels had mufflers at Lewis; still do. And, of course, they ran at night because of electrical power demand. That's when the industry was off-line, like the ten-by-ten supersonic tunnel and the eight-by-six supersonic tunnel in that facility. But when it came to hydrogen, I had to run that at night, and the area was roped off. We just ejected the hydrogen into the air after it would pass through the test section. We had a stack, we called it.

JOHNSON: You mentioned NACA inspections and the one in 1957. How often did those inspections take place?

GRAHAM: Well, I guess they were yearly, but they were passed around, so the three Centers, it was every three years that you got the assignment.

JOHNSON: Who was normally involved during that inspection? Was it the entire Lab? Were you all involved in presenting what was being done at that time?

GRAHAM: Well, a good piece of the Lab. It did take quite a manpower effort. It was given a very high priority because congressional people, representatives of industry came. It was a public exposure of what was going on in these Centers, the three major NACA Centers. So, yes, it was a big project.

JOHNSON: In November of 1957 I read that you took part in the NACA Flight Propulsion Conference that was being held at Lewis, and you were one of the presenters on "Propulsion Requirements for Earth Satellites," and with some suggestions for the possibility of landing on the Moon at that time. Can you talk about that for a minute?

GRAHAM: Now, that's a while ago. My memory probably wasn't—it's kind of interesting, I just recently had my reports bound up. I found a bindery near where I live, and they did it for a very reasonable figure, so I've threaded through some of these things, and I do remember seeing that. I have that document of that conference.

They were primarily, you might say, "back of the envelope" studies of propulsion needs and how high energy was such an important element in making these missions possible. Certainly the reason we got to the Moon first was because that was a good decision, to use hydrogen. Now, I don't say that the configuration that they went through with the Saturn series couldn't be improved today; perhaps it could. But with the technology that we had and with our information from calculations that showed how that would impact specific impulse, as far as high-energy fuels, and that impulse would translate into a payload capability.

I remember we had models made that showed three-stage vehicles with the upper stages, hydrogen; the upper stages, kerosene or jet fuel. You could visually see how it was. Now, in that course of study that I mentioned, I had a professor from Case, astronomer, come out and give a series on celestial mechanics. That, I think, was used by people who were studying missions, and gave people appreciation of the basic equations that would be used to talk about getting into orbit and so forth and translating orbits and moving into space.

So coming back to that propulsion conference, that was the kind of thing that we went through. It was kind of visionary stuff, not in detailed calculations or designs, but just showing what kind of vehicles had to be thought of and so forth. We also talked about electric propulsion, ion engines. Of course, Lewis had a big part in that, because one of the inventors of some of the electrical propulsion engines came from Lewis. I don't know where he is now, and I'm trying to think of his name now. It will maybe come to me. I can see him, but I can't remember his name.

JOHNSON: Well, during that time, of course, like I mentioned that one of the suggestions was the possibility of landing on the Moon. When do you remember that idea first being discussed?

GRAHAM: I guess when I probably joined the Rocket Branch in the mid-fifties to [19]'60, just before Sputnik.

JOHNSON: Did that seem far-fetched at that time to you—

GRAHAM: No, no, no.

JOHNSON: —or was that something that you felt that was achievable?

GRAHAM: Yes, I thought it was exciting, and of course, we had done enough simple calculations to indicate what rockets could do in terms of getting up into orbit.

JOHNSON: Well, you came there—you were still relatively young, in your twenties at that time, and I know a lot of the people that worked there, and it was an academic-type setting, that there were a lot of young people there. Were there a lot of social activities that you took part in?

GRAHAM: Oh yes. It was a family. We were very self-contained as a Center. Everything was done there, machine shop, model shop, instrumentation. If you had any kind of a problem, whether it was aeronautical or whether it was at home, you could find somebody that could help you. It was a marvelous source of information. I built my own house, and I had electrical help from the electricians at the Lab. I did the finishing, putting receptacles and things like that in, but to make the major circuits and patch into the power grid, I had a fellow come over on the weekends and did the wiring. And it was like that.

JOHNSON: Did you join the Speakers Bureau then, or was that something you did later?

GRAHAM: Well, it wasn't called the Speakers Bureau when I joined it. When I quit, I think I was probably one of the first that did that, and I think my first speech was in [19]'55.

JOHNSON: Were you given the information to give to the public, or were you allowed to come up—

GRAHAM: No, you generated your own talks.

JOHNSON: Was there a big interest from the public?

GRAHAM: Oh, I had some wonderful experiences.

JOHNSON: What type of groups did you talk to?

GRAHAM: Oh, all kinds of people, professionals and insurance people, you name it, and, of course, a lot of Kiwanis and clubs like that, church people, church organizations, civic groups. It was an interesting activity.

JOHNSON: Did you notice the interest pick up after Sputnik as opposed to before Sputnik, as far as the public was concerned, in what was happening?

GRAHAM: I think generally that happened, yes. I think my first talk was actually not in Cleveland. It was in Fort Wayne [Indiana]. I was asked to go to a group in Fort Wayne to talk.

JOHNSON: If you had to look back over your years when it was still the NACA, what do you think your most significant contribution during that time period would have been?

GRAHAM: I think when I managed to get consent from management to explore liquid hydrogen as a coolant, and I think my reputation as a researcher, which had international recognition, was based pretty much on that and the general topic of heat transfer, basic heat transfer. Lewis had one of the outstanding groups in heat transfer in the world, and I was privileged to have one of the groups in that. So it wasn't the only heat transfer section, but outstanding people.

JOHNSON: Was that a difficult process, getting that approval?

GRAHAM: I had to fight for it, but, actually, I got support primarily from my Division Chief. He said, "Go ahead." I had no problems of going ahead with it after that at all. I was fortunate to get a few people to work on it. I worked with them, people that I supervised. I had one man who was outstanding. He's still at Lewis. He was a dynamo, so he was really the project leader, eventually, in the experiments.

JOHNSON: Do you recall his name?

GRAHAM: Yes. Robert Hendricks.

JOHNSON: Did you work with George [M.] Low when he was there?

GRAHAM: Yes. Yes. In fact, on that rocket tunnel that I mentioned, I worked with George Low and some of his people on that. In fact, he loaned me one of his designers to help in designing that rig—the operational sell—Robert [R.] Godman. He died, I think, last year. George Low was a remarkable guy.

JOHNSON: If you had to look back and talk about your greatest challenge during that time, would it be something different than getting that approval?

GRAHAM: There was always a challenge to getting support for what you were doing, but that was healthy, I thought, and you were accountable. So as you moved along in your projects and as you saw what you were going to do in the future, you had to justify it, and that's why we had these séances with Dr. Silverstein. [Laughs]

I can remember in that whole program of heat transfer, and I was beginning to focus more on other aspects other than the rocket coolant itself, on the gas inside, how it was transferring heat to the wall and what was the interacting feature between the cooling liquid and the gas that was flowing through, the hot gas. I thought that we needed some basic aerodynamic studies, simulations of effects on the boundary layer, on the gas as it transferred heat to the wall and so forth. So I had a program generally that looked at that whole interacting process, and I was proposing boundary layer studies, because it turned out that whether the boundary layer was laminar or turbulent, would, of course, be a big control on the heat transfer.

So there was a phenomenon called laminarization, where a normally turbulent boundary layer, if it went through a curve on a concave surface—or convex surface—the boundary layer would change character. That was one of the fortunate things that happened on the gas side, that because that gas had to go over a throat and a bend, the boundary layer would change from turbulent to laminar, and in the heat transfer process into that very critical hot throat section, the gases wouldn't penetrate as much, or transfer as much heat, as if it had been completely turbulent.

I wanted to verify this over a number of geometries and situations, and said I was going to embark on some aerodynamic studies that related to that, basic studies. So he heard that and he liked it. He says, "You need some aerodynamic people." And in a few weeks I had assigned to me, transferred, people who were experts in doing aerodynamic experiments from the tunnels.

So that's kind of the way it kind of operated for me, very favorably, and I was able to have a kind of a coherent overall research that related to a very critical problem of propulsion.

JOHNSON: So there was lots of cooperation.

GRAHAM: Lots of cooperation. People trusted one another. There wasn't somebody trying to get ahead. I felt like we were a team. That was the family nature of the Center.

JOHNSON: After 1958 when the change as far as NACA turning into NASA, was there any major change in your duties or what you were assigned to at that time, or did you just continue on working in the same section?

GRAHAM: After [19]'58, yes, my section grew. I had at one time fourteen, fifteen people, and then I'd have summer faculty come in and work with us, and summer students. So I had a big group to deal with.

JOHNSON: Did you notice any other changes in the Center? Besides increasing in size, did you notice anything in the change—as you said, it was a family. People interacted. There was a lot of cooperation. Did that hold true once it transferred to NASA?

GRAHAM: Yes, that worked for a while, but then it began, I think after the success in the Moon, that sort of went away, and then there was the contracting business that you did outside work rather than—the Center wasn't as self-dependent as it was before.

JOHNSON: Well, during that time, I know you mentioned, of course, Neil Armstrong worked there for a while, and that was before he was well known, but I know that people would visit the Centers or the Labs at that time, famous people, politicians. Do you remember any incidents or any visitors coming to Lewis, or anything surrounding that?

GRAHAM: Yes, I remember when people were there, and I've seen these people occasionally around the Laboratory and have met some of them. Like I mentioned, James Doolittle, of course, had a history with NACA.

JOHNSON: Anyone else come to mind?

GRAHAM: Well, there was a number of people in my area of heat transfer expertise. I had international visitors, some of them world-renowned. [Ralf] Schmidt, actually one of the great people and recognized all over the world for his heat transfer. And Eckert coming back.

JOHNSON: Well, you mentioned Abe Silverstein, and I know he was instrumental in bringing the Centaur Program to Lewis to, I guess, debug, if you will, the program when it was having problems. Did you participate in that program?

GRAHAM: I wasn't part of the Centaur Group, no, but I feel like my connection to the RL-10 was significant. So I guess the change was when we became more project-minded rather than

research-minded. I stayed pretty much with the groups that were on the research side of the house. Development became a bigger thing in the Laboratory.

JOHNSON: And you stayed more on the research—

GRAHAM: I stayed in research. That was what I wanted to do, and there was a place for that, and I wasn't threatened by staying where I was. But I didn't end up my career there. I kind of bumped up, became a Chief Scientist for one division in aerothermodynamics, and so I acted as a helper to the Division Chief in reviewing all the technical work and reviewing reports as they were developed.

Then the last five years that I was there, there was an interesting job that had been recommended by a committee that Silverstein had headed, and that was to have an Independent Technology Assessment Group, and so I became the Manager of that group. What we were charged with was kind of looking on the outside and seeing how Lewis' work compared, and also to check about duplication. In doing that, I was the focus point in the Center for Independent Research and Development, the IRAD Program—you ever heard of that?—which involved mainly our agency and the Department of Defense.

So we would go and grade industry on how they were managing their independent research money to see if they were using it wisely and doing research with it. So my group, I had five engineers in the office, and so we divided up the Center into areas. I had experts in certain areas in the Center. Then we would arrange these IRAD trips where we would go and we'd take some of the Lewis people with us on the plane. We'd have the NASA plane, and we'd

go and grade people, various industries, along with the representatives from other government agencies. So that was helpful in making an assessment of how Lewis was doing.

We also managed the Small Business Innovative Research Program for the Center, and we were responsible for the annual report of the Center. I personally took some of the information from my colleagues and wrote and even had conferences with the Director as to what we thought was going on. We tried to be independent and fair and that sort of thing. It was a kind of a neat thing to do, but I wasn't doing any research anymore. So I did that until I retired.

JOHNSON: When did you retire?

GRAHAM: 1990. But I went back to the Lab. [Laughs]

JOHNSON: Did you.

GRAHAM: Yes, for thirteen years on a part-time basis in the Safety Office. The Director indicated to me that maybe if I was interested in coming back—this was Larry [Lawrence J.] Ross, one Director—they needed to make up a safety manual. They had a lot of memoranda about directives for safety, but they needed to have a manual that could be amended and so forth. So I was asked to help a young man, full-time employee, to work on that. So, of course, we got lots of people in the Lab helping to write chapters. At first I think there were twenty-one chapters or something like that. I insisted—this is what I did, boldly, even though I was a

contractor—I insisted that that be done the way NASA reports are done and that it be a NASA report. So it works through the Editorial Office and so forth, just like writing a report.

So I did that till I retired three years ago, fully. Well, I was only part-time. So my relationship with the Lab has covered fifty-five years.

JOHNSON: That's a long career. Go back to the sixties and that time period after it became NASA and when the Apollo Program was gearing up, and the Centaur and working toward the Surveyor landing on the Moon and then, in [19]'67 the Apollo 1 fire, and then the eventual landing on the Moon in [19]'69. What was the feeling at Lewis or the atmosphere like during all the accomplishments and those sad times, too, as far as the work that was being done and how it was helping to contribute to—

GRAHAM: The motivation was great, really great, and general morale was very, very high. People would go out of their way to work and help others, even, in their programs. It was a golden era. But, unfortunately, once the accomplishment was made and then where to go next wasn't clarified, I think that's where NASA has had some of its problems.

JOHNSON: As you mentioned, the goal was achieved. Then in [19]'72, [19]'73, when things started winding down and there were RIFs [reductions in force] across NASA, and I know they affected Lewis, how did that affect your work at that time, or did it affect that work?

GRAHAM: Well, I had to let go of, I think, just one person, but I felt sorry I had to do that. That's hard. That's one of the hardest things a manager has to do, because the fellow was doing

okay, and it's just too bad that I had to—I was told I had to choose who, and who did I want to let go.

JOHNSON: Do you know if he went on to the private sector, or was he moved into another position?

GRAHAM: No, he left.

JOHNSON: What was the morale like at Lewis during that time period, as compared to just a few years before when it was so high?

GRAHAM: Well, when you start talking about firing, you know, that doesn't help. Then people begin to guard themselves, you know, people and organizations.

JOHNSON: Was the social atmosphere still there as it was in the early days?

GRAHAM: No.

JOHNSON: Things changed?

GRAHAM: Things changed.

JOHNSON: But you were still a part of that Speakers Bureau at that time?

GRAHAM: Oh yes. I was in the Speakers Bureau up until—even when I was a contractor. I think I probably left about late maybe 1998 or something like that.

JOHNSON: Do you have any memories or any involvement with the Plum Brook [Sandusky, Ohio] facility?

GRAHAM: Well, this connection was when I had the Technology Assessment, I had some acquaintances out there, and made a few visits, but I hadn't had any close. There was one tunnel that we were interested in, a supersonic tunnel, that, I guess, eventually it ended up out there, and we were interested in that.

JOHNSON: You mentioned your achievements during the NACA time. Is there anything during that NASA time that you'd like to mention?

GRAHAM: Well, we still carried on a basic research program in heat transfer, and even though the hydrogen sort of tapered off eventually, but one of my colleagues and I wrote a book on boiling and near-critical heat transfer, and published that in the seventies. So we sort of documented some of the work that was done. This fellow was Chinese, Taiwanese, and he was doing—when I heard about him, when we were entering some of this boiling research, I heard about him through some of my connections. I was active in technical societies, the American Society of Mechanical Engineers. He was doing postdoctoral work. Even though he wasn't a

citizen at that time, we managed to get him on board, and Dr. [Hugh L.] Dryden approved his coming. So Y. Y. Hsu and I wrote this book.

JOHNSON: Did you belong to any other technical societies or professional societies?

GRAHAM: Well, I was a part of the IAA [International Academy of Astronautics], too. I kind of dropped that, but I was very active in ASME [American Society of Mechanical Engineers]. I was a Governor of the society, and I was Senior Vice President for Public Affairs, and one time I was a candidate for President, but I wasn't selected. But up till quite recently, I was active in it. Kind of dropped out three years ago in any committee work or anything like that.

JOHNSON: Just taking it easy now?

GRAHAM: I think that you give other people a chance.

JOHNSON: Is there anything you can think of that we haven't talked about, as far as your NACA experience, that you'd like to mention?

GRAHAM: Well, I had an interesting experience with Dr. Dryden. In that book there, it's mentioned, too, in talking about him, so you could refer to that, but I got the idea—I'm very active in my church, and our church is in a campus on Baldwin-Wallace College in Cleveland, Ohio. I don't know if you've heard of it. I talked to the President one time—he's a member of our church, as well—and said I'd like to have a conference, which eventually was called

Conversations about the Ethical Use of Knowledge. We got an assortment of experts to come, and Dr. Dryden consented to come. He stayed at my house. Great, great guy, really.

JOHNSON: Quite an experience, I imagine.

GRAHAM: Yes. So he wrote a big piece, and I have a record of that conference. That was in the early sixties. So that was very much of a highlight for me. I was General Chairman of the conference. It was down at Baldwin-Wallace College. A number of people from Lewis came over and participated. So that's, I think, probably one of the really nice memories. I had such regard for that guy. He was so humble, and yet so brilliant. We used to get a Christmas card from him every year. [Laughter] That was, "Hugh and Libby Dryden," just that.

JOHNSON: Well, you had a chance to work with some very amazing people during your career.

GRAHAM: Oh yes. Yes.

JOHNSON: Quite an opportunity.

GRAHAM: It was. I've had an exciting life.

JOHNSON: You don't regret not going back to teaching.

GRAHAM: No. And, of course, what happened was I had so much opportunity to teach, and I was an Adjunct Professor at North Carolina State University [Raleigh, North Carolina], and I lectured at a number of colleges, Minnesota, Purdue, [University of] Cincinnati [Cincinnati, Ohio].

JOHNSON: So you got to do both, then.

GRAHAM: Yes, and I guess you build your house, and you get emotionally attached. You get emotionally attached to your community, and you have children and they're there. You know what happens. You get anchored down.

JOHNSON: Did the social life involve your entire family? Did the children of the people you worked with, did they all go to school with your kids and did the social life extend?

GRAHAM: No. We had picnics. We had a lot of really great family picnics at Lewis, and the Christmas programs. When the kids were little, they had a great program there for Christmas season. It was interesting. People of all faiths and colors and everything, but we got along.

JOHNSON: A special time.

GRAHAM: Yes.

JOHNSON: Well, if there's not anything else, I appreciate you coming by to visit with us and sharing this information with us, and we'll definitely use this, this history that you wrote.

GRAHAM: Okay.

[End of interview]