NASA HEADQUARTERS NACA ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

ERWIN V. ZARETSKY
INTERVIEWED BY REBECCA WRIGHT
CLEVELAND, OHIO – JUNE 4, 2014

WRIGHT: Today is June 4, 2014. This oral history session is being conducted with Erv Zaretsky at NASA's Glenn Research Center in Cleveland, Ohio as part of the NACA [National Advisory Committee for Aeronautics] Oral History Project, sponsored by the NASA Headquarters History Office. Interviewer is Rebecca Wright, assisted by Sandra Johnson. Thank you so much for stopping in and giving us your afternoon to be part of our project. We'd like for you to start today, if you would, telling us how you became part of the NACA.

ZARETSKY: That's a very interesting question. First of all, it wasn't my intent [to become part of NACA]. I was actually forced—figuratively speaking, not literally speaking of course. To give you a little background about myself, I went to the Illinois Institute of Technology in Chicago, Illinois. I started there in 1953 as a freshman, and I finished on June 6, 1957, which means it's almost 57 years since I graduated. When I went into the school, it was at the ending of the Korean War—I don't think it was quite over at the time—and I joined the ROTC [Reserve Officers' Training Corps].

My intent was to go into the Air Force, and if I were to go to flight school and become a pilot, I would probably have stayed in the military. That was my original intent. I was in the ROTC and went through what they call the Basic Corps and the Advanced Corps. At one time during the course of my ROTC training, there was an organization—which is now the [American] Institute of Aeronautics and Astronautics, AIAA—but it was [formed] from two organizations which merged: the Institute of the Aeronautical Sciences, and there was the American Rocket [Society]. I believe that's what it was called.

I was very active in the Institute of the Aeronautical Sciences, and one of the things that we had shown for one of our student meetings was a crash fire program [motion picture film], which was from the NACA. I had no association with NACA—but that program was headed by Irv Pinkel, I. Irving Pinkel, and one of the participants was Solomon Weiss. There was a whole group of people that actually crashed airplanes with instrumented dummies, [and analyzed] the fires. There was a lot of photography which was involved with it at the time, to see how aircraft could be made safer for the passengers. This was, in retrospect, my first real introduction to what was NACA, but it never really dawned on me. It was just an interesting movie. It was fast and well done, came out of Lewis [Research Center, Cleveland, now Glenn Research Center]. At that time, it was called the [NACA Lewis] Flight Propulsion [Laboratory].

In my last semester of college—I think it was January of 1957—there was a note on the bulletin board from a professor of mine, Stothe [P. "Scotty"] Kezios. This was on, I think, a Monday morning. He said I should get to see him in the afternoon, he had time. I stopped in the office and I said, "Professor Kezios, you wanted to see me?"

He says, "Yes. I arranged an interview for you with NACA."

I said, "What is NACA?"

He said, "It's the National Advisory Committee for Aeronautics."

I said, "I don't think I ever heard of them. What kind of an organization are they?"

This is as verbatim as I remember it. He said, "It's a research organization, it's a government organization."

I said, "Well, where is it located?"

He said, “Cleveland, Ohio.”

At that time, I already knew when I was supposed to be going to flight school. That was supposed to be in September, and I was supposed to be commissioned in [June]—in fact, [June] 6—and I did need a temporary job between [June] 6 and September.

I said to him, “Professor Kezios, first of all, I’m supposed to go into the Air Force. I’m supposed to go to flight school. I’m not really interested in living in Cleveland, Ohio. I have a few weeks before I go into active duty.” I wanted to go to the West Coast. I had family there, I sort of liked the California scene at the time, being a young, single guy.

He said, “Look, I already made arrangements for you to have an interview, and I have a grant from NACA. If you don’t show up, it’s going to make me look bad, and it can, in fact, jeopardize my grant. If it jeopardizes my grant, it’s going to jeopardize your grade in heat transfer.”

I said, “Professor Kezios, don’t say anything more. I’ll show up. Where am I supposed to show up?”

This was almost literally speaking what the conversation was. He said at the Drake Hotel, I think it was a Wednesday afternoon. It was after lunch, almost like the time it is now. The Drake Hotel was just north of the Loop [business district] in Chicago [Illinois]. You could take a streetcar from the college and get there in maybe 15, 20 minutes.

On Wednesday, after my morning classes, I went to the Drake Hotel. I went up to whatever floor it was and knocked on the door, and there were two gentlemen there, Joe [Joseph M.] Savino and Pat [Patrick] Chiarito. Joe was a younger guy, and Pat was a little older. Pat looked like the consummate Italian gentleman. If you were to cast him [in a movie] as an Italian aristocrat, that’s how he would look, stereotypically. Joe was a good guy. Both of them became friends of mine.

I walked in there and my conversation—I was a little arrogant at the time, I was a young guy. I said, “Gentlemen,” I introduced myself, and I said Professor Kezios had arranged for me to be here.

He said, “Yes, we have a little résumé of you here, we know a little about you.”

I said, “You really don’t want to interview me.”

They said, “Why not?”

I said, “Because I’m not looking for a permanent job. I have to be honest with you, I need a temporary job, because I’m going into the Air Force and I need some time between the time I graduate and the time I go into the Air Force. I am going to be a pilot, and I’ll probably stay in the service if I finish flight school.”

Joe, I remember this very well, says to me, “That’s not a problem, that’s great.”

I said, “I don’t understand.”

He said, “First of all, NACA has military pilots and they have ex-pilots as test pilots.” I swear this is what happened. He said—this is 1957—“In 1955, we just hired an ex-Navy pilot at Lewis, Neil [A.] Armstrong.” He said, “What you could do—we could make arrangements. You work for us, you go to flight school, and then after you finish flight school, you could come back and be a test pilot for NACA.” That really caught my fancy.

I said, “Great idea. That’s really interesting.” Pat, of course, chimed in too at the time, but that was the thing that caught my eye.

They said, “Would you like to come out to see the laboratory?”

I said, “No, not really necessary, it’s a temporary job.”

WRIGHT: New definition for "temporary," right?

ZARETSKY: This was a temporary job. How bad could it be, right? So I fill out the application. Actually, I also filled out an application for Dryden [Flight Research Center, California], the Air Force base at Edwards [California], because they had solicited interviews also. I had interviewed with them, so I had an offer from them also, to go out to Edwards Air Force Base. I got a brochure from NACA and from Lewis, indicating the various areas that would be of interest based upon my background.

One of the reasons they were recruiting was they had planned, or they were in the process of opening up [NACA] Plum Brook [Reactor Facility, Sandusky, Ohio] as the nuclear station, and they were looking for people to do heat transfer. Professor Kezios was a heat transfer professor, and that's where their interest was. But my interest was more in the fluids area, and I also had a course—although I have to be candid with you, I struggled with the course—on lubrication and bearing technology with Professor [Paul R.] Trumpler. So I had some background in that [area].

This is an important part of the story—just two weeks before I'm supposed to graduate from college and be commissioned, I got called in by the Air Force ROTC commander, actually one of the professors, and he says, "I have good news for you." This guy was a terrific guy, by the way, and I had a lot of respect for him. He said, "You're going to be able to go to flight school and get a regular commission." What I was getting was a reserve commission, so if you went to, let's say, [U.S. Military Academy at] West Point [New York]—the [U.S.] Air Force Academy [Colorado] did not exist at the time, as I remember it—you got a regular commission.

A regular commission was a coveted-type thing if you were in the career mode in the Air Force.

I said, "Sir, what do I need to do?"

He said, "Well, here's the deal." I had a four-year commitment for the Air Force, so if I had not finished flight school for whatever reason, I would have had four years. If I wasn't going to flight school, I had a commitment of three years. He said, "Your commitment is going to be five years after you finish flight school." If you went to advanced flight school, it was eight years.

I said, "What if I don't finish flight school?" because you could wash out for physical reasons, or if you didn't have, perhaps, the skills to complete it.

He said, "You still have a minimum five-year obligation if you wash out." That means up to eight years in the military. [I did not want to commit myself upfront to this time period of military service.]

I said, "What's my alternative?"

He said, "You don't want the alternative." For the record, the man was Major Lyle Grey, who was a terrific guy. He retired as a colonel. I was in touch with him many years later.

He said, "You don't want the alternative."

I said, "What's the alternative?"

"You don't want it."

I said, "What is it, sir?"

He said, "You can go in as an engineer for three years."

I said, "I'll take that," and if you excuse the expression, they were all POed at me. They were really upset at me, because I was the first one they were offering it to. I don't know if any of the other cadets took that offer or not, for flight school. Now I wasn't going to flight school. I had a three-year obligation, I was going in as an engineer. I called up NACA and I said, "What's the chance of being stationed here?"

Army, Navy, and Air Force—they had young lieutenants who were coming out of college, who had engineering or science degrees. They were bringing them in here as military officers, but assigned to civilian shots. The person I talked to was a person named Art [Arthur] Levine. Art was deputy [chief for] human resources, or human personnel, however they called it. A nice guy, and I got to know him well later on. He said, "We'll see about getting you assigned to work here at the laboratory on a permanent basis in the military capacity."

It was June 6 I graduated—it'll be two days [from today]—and July 2 is when I started [working] here. I graduated, and I took a month off. July 2, which was a [Tuesday] of 1957, I reported here. In the interim period of time, they sent me brochures of the various work that was being done. I chose the Fluid Systems Component Division that was headed by Irv Pinkel. Pinkel was a very interesting man. Very, very accomplished, very interesting.

I came here and Pinkel interviewed me, and a fellow named Ed Bisson, Edmond E. Bisson. Ed was a terrific guy. They asked me what I'm interested in, because I indicated heat transfer from Professor Kezios. He said, "Well, we do some heat transfer, but there's other areas too, you might be interested in. What's your background?" In the course of discussing my background, I talked about this course with Professor Trumpler on lubrication. Ed Bisson's eyes lit up, and he said, "Well, I want you to talk to two other people to be interviewed." One was a fellow named Bob Johnson, Robert L. Johnson, the other is William J. [Bill] Anderson.

They're all gone [now].

WRIGHT: Had they been at NACA for a long time?

ZARETSKY: Yes, Ed actually came from [NACA] Langley [Research Center, Hampton, Virginia]. Ed was a terrific guy. I've got to get my composure.

WRIGHT: That's okay, we've got all afternoon.

ZARETSKY: Ed had said the course I had with Trumpler was unique. In fact, today it's not really taught at most schools. I knew a lot of stuff, and things that people here, who were working in the area, actually didn't know. I learned it out of Trumpler's course.

I interviewed further with Robert Johnson, and then reported to Ed Bisson. Ed was the associate division chief, and Pinkel was the division chief. Ed was also acting branch chief, as it was organized at the time. Bob had one of the sections, and Bill Anderson had one of the other sections. I felt more comfortable in the area that Bill had, but I wasn't really worried about it because this is still a "temporary" job. My flight school days were over, by the way.

They said, "Which of the groups would you be interested in working?"

I said, "I like Anderson's group, I think I'd like to work there." One of the people who was there was a fellow named Thomas L. [Tom] Carter. Tom got there in 1955 as an Army lieutenant, working the area under Bill. I was assigned to Tom, and about two weeks after I started to work there, I got a brown envelope from the Air Force with a direct duty assignment to Korea.

I was supposed to do ground power maintenance, electric power distribution, with a stopover for classes in Cheyenne, Wyoming, at the F.E. [Francis E.] Warren Air Force Base. I got a hold of Art Levine and I said to Art, "Art, what about the assignment here?"

He said, "We're working on it, we're working on it." I'm familiarizing myself with the lubrication and bearing technology, and what is now known as tribology. But it wasn't called tribology then, it was called lubrication, friction and wear. I don't recall the name of the branch—you might want to check organizational charts at the time.

I started to analyze data, started to do work really under Tom Carter's tutelage. Tom was himself an interesting person. He was, in September, going back to Harvard [University, Cambridge, Massachusetts] to get his MBA [Master of Business Administration], so he would be gone for at least a year, maybe two years. He had a degree in metallurgy out of RPI, Rensselaer Polytechnic Institute [Troy, New York], and they were running a test rig.

There were a lot of thing going on, so basically I was inheriting a lot of things that I didn't start but were already existing. Six weeks comes, and I'm on my way out. I'm on active duty as an engineering officer, with an assignment to Korea. I'm [first] going to be at F.E. Warren Air Force Base [to learn maintenance] procedures for problem equipment, electric power [distribution], diesel engine generators, [and] vehicle maintenance.

I'm at the officers' club at F.E. Warren, and I'm sitting at the bar. There's someone sitting behind me, we were like bookends on a shelf. I was talking to somebody on my left, and this party was talking to somebody on his right. In the course of the conversation, I heard the Lewis Flight Propulsion Laboratory mentioned, so I turned around and I said, "Did you work there?"

He said, "Yes." Turns out this was Sal Grisaffe, Salvatore [J.] Grisaffe, who had the same history. He actually is from Chicago also, but he went to the University of Illinois [at Urbana Champaign] in metallurgy, and he was also there for six weeks and got called.

I told him, "Well, they're still working on this and we still could end up at Lewis."

He said, "Don't hold your breath."

It turns out that we were on the same plane. He was assigned to Korea also, as a supply officer, and I was supposed to be in electric power distribution. We were on the plane, which was a [Lockheed L-1049] Flying Tigers [Super] Constellation. I remember this very well. We were sitting on the right-hand side in sort of the center of the plane, and as the plane's taking off I turned to Sal. We were sitting next to each other and I said, "Well, maybe they'll stop the plane."

The plane was just ready to take off, and the plane stops, the breaks go on, they reverse [the propellers]. They had engine trouble, but I didn't know that at the time. I said, "They're going to pull us off the plane."

Sal said, "No, no, they're not going to pull us off."

"They're going to pull us off the plane. They're stopping the plane to pull us off, to go to NACA." Turns out it was engine trouble.

WRIGHT: You were half right.

ZARETSKY: It delayed us going. I was in Korea—and I'm glad I went there, by the way. I made a contribution and it was very interesting. One of those experiences in life you don't necessarily volunteer for, but when you have it and you're exposed to it and the people, was very important.

My reassignment from Korea was Cleveland, so the joke was, "Are you better off in Korea or are you better off in Cleveland?" That was a standing joke. Sal retired from here also. He's another interesting guy.

I think it was August [1958], there was a [Second Taiwan Strait] Crisis, if you remember this. The mainland Chinese [People's Republic of China] were bombarding Quemoy and Matsu, which are the offshore islands off of Formosa [Taiwan]. We actually were on a war footing at the time. I don't think people realize how serious that was. Of course we had issues, as today, with the North Koreans, and we had issues with the Chinese. They were threatening to invade—in fact, they probably would have invaded—the offshore islands.

I'm ready to go back to Cleveland, and I was called into the colonel's office. I had 60 people working for me at the time, and I was responsible for all the Air Force electric power in Korea. He said, "We want to have you take 12 of your men for an assignment."

"Where at, sir?"

"I don't know. Give us 12."

So I, with a sergeant, selected 12 men who had the various skill levels that were required, and then that evening I was told to report to the flight line, to take enough clothes with me for TDY [Temporary Duty], that I had a secret assignment. They didn't tell me where I was going. They said, "We can't tell you anything more." Gave me sealed orders and put me on a plane, and then it was like out of a spy movie.

Then I got another set of orders from Japan to Okinawa, and then from Okinawa to Taiwan. From Taiwan I got another set of orders to get on the train to go to Tainan, which is southern Taiwan. I [was] actually [assigned to Tainan Air Force Base]. I ended up there putting together for the [U.S.] Air Force an electric power distribution system for the troops that were coming in to support the Chinese nationalists [Republic of China] in case of an invasion. I spent three months there putting things together.

In Korea, we lived in a Quonset [prefabricated steel] hut. There were six of us, six officers, living in a Quonset hut. We used to call them “hooch mates”. One of the fellows I lived with [in Korea], Joe [Joseph M.] Kacicz called me up [from Korea] on what they called MARS radio. This was before the Internet. MARS was the Military Amateur Radio System. I remember this very well. He said, “Erv, you’ve got to get back to Cleveland within 30 days. Otherwise, you’re going to lose your assignment to NASA.”

NACA became NASA during that period. I was on military leave here, so I wasn’t on the payroll. I wasn’t getting paid by NACA or NASA, but I was on the personnel roles. It was like going away to National Guard duties, this was my status. So I made arrangements—which is another story by itself, which would take too long to go into—to get back to Cleveland within 30 days. That was an adventure story. If I write my book, it’ll be in my book. But I was able to get out of there.

The bombardment of the offshore islands had stopped at that particular time, and the units that we were supporting—with the radar units, the communications, and missile systems—were being pulled back to the United States. I was able to leave, get back. I reported back here [to NASA Lewis Research Center]. I believe it was in January of 1959. I was gone approximately a year and a half, and when I came here [the NASA Lewis Personnel, now Human Resources] didn’t expect me. The Air Force forgot to tell them I was coming back. I walked in there, and they thought that I was here to visit.

I saw Art Levine [in the Personnel office] and I said, “Art, you didn’t make it [happen]. You didn’t get me off the plane.” He said, “How’s everything going?” No one expected me. I was just here. The orders came [with my assignment from the Air Force], I think, about a month later. The [Air Force] started to check to see where I was, and [the assignment] just followed me here. Had I not gotten back [from Formosa] the Air Force was, in fact, going to cancel my assignment here, which would not have been good. Had that call not come in from Joe Kacicz, I would have never known that. They probably would have kept me on Formosa for at least 30 more days.

I got back here and I was, literally speaking, assigned to my old desk. Tom Carter was there. He was taking courses, pre-med [medical] courses. He was actually accepted for Case Western Reserve [University, Cleveland] Medical School. I don't think it was Case at the time, I think it was Western Reserve Medical School. He said, "I'm leaving, I'm going to be gone in September." He said, "No one knows this. I'm going to be a doctor." He became a radiologist, moved to Michigan. I think he's deceased now. I started to do his work.

I don't know if you have gone through my résumé or not, but you [will] notice [that] I have another degree besides engineering. I have a law degree. When I got back here, I knew less about engineering than the day I got out of school. There were a lot of bright people here, and I figured I'm not going to make it as an engineer. I'm gone approximately a year-and-a-half, and I forgot too much. I mean, to learn this would be impossible, so I applied to go to law school and I was accepted.

I started law school, I think it was in September of 1959. But I never told anyone I was going to law school because Tom said, "If you tell anyone here that you're taking another degree besides an advanced engineering or science degree, even if you stay here, it's not going to be good for you. Just don't say anything." So I never said anything. There were a couple of other people here at the lab who were doing the same thing. A fellow named Norm [Norman T.]

Musial ended up being a patent attorney here. I don't know if [you came across his] name or not, but he retired from NASA and he became the mayor of North Olmsted, which is a [Cleveland] suburb out here. Norm would be in his 80's today.

It turns out that I was not just successful [at my engineering work], I was really successful at what I was doing. I knew more than I thought I knew, and you don't realize what talent you have. Tom left, and all the work that he was doing ends up on my desk, and I'm turning out papers. I don't know what's required [to succeed, but] the papers I'm turning out get [a lot of positive] attention.

I had put together a [research] program. I was basically doing [research on] rolling-element fatigue, which is bearing fatigue. Fatigue is the limiting life of bearings. Why is this important? Let's take early 1950s, let's say post-World War II through the middle or end of 1950, [and the early jet engines].

The life of the bearings was limiting the life of the jet engine. The cost of jet propulsion and the commercial airplane was a function of how many times they have to replace their engines, so it was expensive. At that time, the life of the bearings was about 300 hours. Not only was it 300 hours, but there was a program—they were looking into a supersonic aircraft, and they were projecting bearing speeds about two to three times what the engines were running at the time, and temperatures which were significantly higher, two to three times higher, than they were actually achieving.

The idea at the time—and this was in 1957, when they hired me in—was to increase the bearing temperatures to 800 degrees [Fahrenheit], as I remember it, and the speeds to what they called 3,000,000 DN. DN was sort of an artificial speed parameter used for bearings. It's the bearing bore, in millimeters, times the speed in RPM [revolutions per minute]. So if you have a

100-millimeter shaft on the engine and you are running it at, let's say, 30,000 RPM, that's 3,000,000 DN. The bearings were running by half that at the time. One million to 1.5 times, that was the speed. Temperatures were less than 300 degrees, and they wanted higher temperatures.

My assignment was to increase the temperature of the bearing, increase the speed, and then increase the life. So we built test rigs. Some of the things we did were very unique. When I got back here in 1959, Tom and I built what was then called the five-ball fatigue tester. The five-ball fatigue tester, I think, has more published endurance data for bearing materials than any published group in the world, ever, which is another interesting story. It was all associated with this, so if I go on like a Russian novel, you'll understand.

The idea was you can't run one or two tests. The types of data that we were seeking were not deterministic. In other words, you start something, it ends at the same time. It varied. Could be up to 100 times scatter on the data, so you have to treat this statistically. In order to treat it statistically, you have to run a lot of data for a long period of time. That means you don't run one test rig, you have to run multiple test rigs.

[To digress a bit, on my first day back at Lewis in 1959, after my Air Force duty overseas ended, the first person that I met after I finished with Personnel was] Ed [Edmond E.] Bisson. Ed had an interesting background himself. He had French Canadian ancestry. He was born in Vermont, but he spoke with a little bit of a French Canadian tinge to his voice. The first one I met when I got back in '59 from the Air Force duty overseas was Ed. Ed said to me, "Bill Anderson conceived of a *five-ball* fatigue tester." It sounded to me like "fireball."

I didn't say anything, but when I saw Bill I said, "What's the fireball fatigue tester?"

He said, "No, no, it's not a *fireball* fatigue tester. It's a *five-ball* fatigue tester."

He said he had a concept, and this was a bearing ball pyramid on four bearing balls. It simulates an actual bearing, except instead of having an inner ring and outer ring and balls in between—or if it was a roller bearing, rollers in between—a ball was the inner ring. This way it's an inexpensive way of testing, and you can run multiple tests. The idea was to build a lot of testers, run them 24 hours a day, 7 days a week. Starting in January of '59, and ending in March of '59, Tom Carter and I designed the tester, and we had the first one running. Never would this be able to be done today. We were doing sketches. The shop, I believe, was in this building, and so we'd run to the shop, get a part made. We were really blessed.

There was another man, Eldred [H.] Johnson, involved. He was an aircraft mechanic [in the Army Air Corps during World War II]. Perhaps if he'd been born in another time he would have gone to college and become an engineer. Really a super bright guy, but he was a skilled craftsman. He was really good. We would run back and forth between the shop and the building which, at that time, was called the Chem [Chemistry] Lab. I don't know what it's called now—I think it's Building 6 here at the laboratory. We would bring the parts and he would be putting everything together with the hardware and everything. You couldn't do something like that today. We were able to get the first rig and data in three months, from the time we started.

Then, I was going down to help Eldred—we called him John—and he said, "Would you do me a favor?"

I said, "What's that?"

He said, "Don't come down here anymore."

WRIGHT: Didn't need your help?

ZARETSKY: He said, “Don’t come down anymore. I’ll run the rigs and get the data, and you write the reports.” That wasn’t quite [how it] worked out, because we all shared [the work], but basically that’s what it was. He ran the downstairs, [and I ran the upstairs]. We had about a 70 percent utilization rate all the time I was here. We got 70 percent of 7 days a week, 24 hours a day, absolutely phenomenal. There’s a whole story about that that’ll take too long, but I think we had over 500,000 test hours over a period of—the rig stopped running in 1984, so that was 25 years. I think [that this] represents the bulk of the materials that we did, [but also most of the bearing material comparisons published in the open technical literature].

At that same time, I had a plan to do some additional work to attack some of the other issues, the 3,000,000 DN [bearing] and high temperature [bearing] lubrication. One of the advantages of being in the Air Force was I had friends at Wright-Patterson Air Force Base [Ohio]. There was an Air Force liaison office here that was there for years. They had a [Douglas] C-47 [Skytrain], which is a [Douglas] DC-3, and they would fly, I think, once a week to Wright-Patterson, so I would hitch a ride with them.

I would see my buddies there, and I’d also buy liquor there because it was inexpensive to bring back for the parties we had here. I would get down there, and so I needed a bona fide excuse. “Why do you want to go down there?” “Oh, I want to buy my booze for the weekend”—couldn’t do that. They had a [bearing] lubrication program going at the time. This was 1959.

There was a person [at Wright-Patterson] named Hamilton [A.] Smith who was the project manager. They were doing some work in a field called elastohydrodynamics, and they were running [tests and obtaining] data for several years, beginning in the mid-50’s, early 1950’s. They had a whole database of high-temperature lubricants. He invited me to be a member of the Air Force Advisory Committee on High-Temperature Lubrication, which they had up there. I'm a young guy, a second lieutenant—I got to be first lieutenant—so I was NASA's representative to the Air Force Advisory Committee.

I made contacts with that [committee]. This was the basis for the work that I was able to do in high-temperature lubrications. Because on this committee, there was a person from [Socony] Mobil Oil Company, named Ed [Edward A.] Oberright. Ed was a member of the committee, and I got to know Ed—he was an older gentlemen—and we got to talk about some of the work that they were doing. In the early 1950s—I remember this from my Air Force ROTC days—the Air Force had a program to have a nuclear bomber.

I don't know if you are familiar with this or not, but the nuclear bomber was a bomber that would be propelled by nuclear propulsion. It would heat up the gases to give it propulsion, which could theoretically stay in the air without landing for days on end. They needed to have lubricants that were resistant to radiation. Mobil Oil had a program to study various types of chemistries, and they studied, 30-some chemistries, as I recall.

Ed told me none of these chemistries were really resistant to radiation, but they were all good high-temperature lubricants. He said at least they held up well with what they called "thermally stable" and "oxidatively stable" at higher temperatures than the current oils were, which were the ester-based oils. Through Ed and then through the Air Force, I got their data, and I got sample lubricants, and we did a whole lubrication program starting in 1959. I was going down there more than just getting the liquor for the parties. I was bringing back lubricants. That was really a terrific program.

WRIGHT: Did you have to propose that to NACA, to have that here?

ZARETSKY: I had to propose it because no, you just couldn't arbitrarily do it yourself. Let me get back to the story at bat. Bill Anderson was a real supporter and was a partner actually, and we had to propose this to Irv Pinkel. I put together a presentation with Bill's help, and at that time you had these viewgraphs [transparencies for overhead projectors]. You put the viewgraphs on, and you made the viewgraph through a Thermofax machine. I don't know if you recall that or not. You're too young to remember that, but it wasn't like today with the copy machines.

I think we had one Xerox [Corporation Ltd.] copy machine in [the late] 1950s, and the first one was in the library here. [The cost] was $25,000, as I recall, and you had to get permission to use it. You just couldn't arbitrarily walk up and use a machine. I put together a presentation, maybe a 15-minute to half-hour presentation for Irv Pinkel, with a budget and everything else—that we proposed to do, [outlining] the relationship with the Air Force. I walked in with Bill into Irv Pinkel's office—which was in Building 5, I believe. I walk in and I'm ready to give the presentation to Irv, and he says to me, "You don't have to give the presentation. Tell me what you need."

I pull up the last slide, "This is what we need, this is what we're going to try to do."

He said, "Okay, you got it. Now, if you want to give me the presentation, I'll listen to you." He said, "My job is to allow you to do your job." This sounds exceptional today, but that was the management style as existed at this laboratory in those days. They were enablers. They enabled young guys like myself [to excel].

If you messed up and you didn't do a good job, you were pretty much finished. But if you did a good job, you performed, and you got results—not everything you would do would be successful, but if you got reasonable results for what you did, and the objectives were within the mission profile of NASA—it became NASA from NACA in 1958, is when the transition occurred. But it was the same management, same people. The support was absolutely there. I never even questioned the fact that the support would be there. Meanwhile, I'm turning out papers.

The first paper I actually physically published was in 1960. You've got the list there. I remember giving that, but that's another story. Also in 1960, a very interesting thing happened. Again, it's a reflection on the people who were here. General Motors [Company] Research Laboratories, in Warren, Michigan—in the 50's, when I went to college—was probably the premier place [to work]. If you got a job there, that was really a five-star job offer for a lot of mechanical engineers. It was highly regarded. They had really a terrific staff there.

Every so many years, they would select a subject, and they would invite—at no cost to the invitees, they would organize a symposium, invite the top people from around the world to present a paper or summation of the work they were doing, and they would bring all these people together. I think that's the only time in my whole career that I saw something like this. Rolling Contact Phenomena was the [name of the] symposium. This is occurring, I think, in October of 1960, if my memory is good. Bill Anderson and Ed Bisson got an invitation to attend.

The invitation included all expenses to be paid, but being government employees, they could not accept the expenses. They said to GM, "We'd like to bring one of our young researchers there," meaning me, and it wouldn't cost them anything. The response from General Motors, the organizer said, "Since we're not paying for you, and since we're not going to paying for him, it's perfectly okay." That was a pivotal, pivotal moment in my career, and I'll explain to you why.

I went and I coauthored a paper with Bill Anderson. Bill was the first author, I was the second author. [Tom] Carter had already left—he left in 1959 to go to medical school—and I actually started law school at the time, but no one knew that I went to law school. I don't think this would have happened had they known I was going to law school. I went to General Motors Research, and Bill gave the paper. At this meeting, I met just about all the significant players [in the field, some of] whom are historical figures now.

I'll name some names. There's two people in the area of bearing technology that I think were major contributors, in my opinion. One in the first half of the 20th century, and one in the second half of the 20th century. One was a man from Sweden named Arvid Palmgren, and the other was a person from the United States called A. Burton Jones [Jr.], Burt Jones.

They were there. I didn't meet Palmgren personally, but we were about five feet from each other at the hotel check-in, and Bill Anderson mentioned, "That's Dr. Palmgren." We sort of nodded heads, so that was like a pass-over, so to speak, at that time. The other person was Burt Jones, who I met, but there was a third person who was very instrumental and very important, and that was a fellow named Waloddi Weibull. He's from Sweden, also.

Weibull is important for the world of statistics. He came up with what they call Weibull statistics, the Weibull plot, which is the basis for much of the life-prediction work that has been done on bearings. Palmgren used Weibull's work. What's interesting about Weibull is I got to be friends with him at that meeting, and that was really pivotal. It was important for me. I met Weibull at that particular time, and he was intrigued with the data that we had, that Bill and myself had presented at that meeting. We had discussions and spent time at that meeting together. I met Burt Jones at that time. I got to know Burt Jones, but there's also another person who was there, and his name was Walter Wernitz, from Braunschweig University [of

Technology] in Germany. Wernitz turns out to be a very interesting guy. Really, very interesting. I got to be friends with him at the meeting.

I [subsequently] met Weibull in New York [City] [in 1962], and then we had him here at the laboratory in January 1963. I got a lot of insight from Weibull as to why he did some of the things he did, which is really important. You can't read this in the publication. He [first] published his work in 1939, but the work that he had originated started in 1937. He gave some insight of what he did and why he did it, and then one of the things he had told me is that he had suggested to Palmgren to use his equation and his [statistical] distribution in the bearing life prediction [model]. This was not stated by Palmgren in his publications.

This was very important, by the way, because it ties everything together. It also was the basis for all the work that I did in bearings [and gears] after that time. There was a Professor [Gustaf] Lundberg who worked with Palmgren—you'll see Lundberg-Palmgren Theory—and now I've added to their theory based upon my knowledge of what Weibull did and why they did it. You understand why they make certain assumptions, which really doesn't come from the literature.

That formulated the background of the work that I did. Then, Wernitz did work on traction drives. Traction drives is what they used to call friction drives. In other words, it's like gears with no teeth; it's rollers [loaded] together. We had a [traction drive] program here which we put together in the 70's and 80's, but basically, had I not been exposed to [Dr. Wernitz] at that meeting, this probably would never have happened. That one meeting was pivotal. And another area, which was work that was done at General Motors. A fellow named—and I met him there too, on a casual basis—J.O. Almen. He did pioneering work in residual stresses.

I have papers published on all these subjects. It's been a continuous thing. We've been able to use that as a basis for increasing bearing life. We started in 1959, and we were able to reach 700 degrees Fahrenheit by the late 1960s. It turns out that there's no jet engines that are flying [today] at 3,000,000 DN [bearing speed]. However, they're flying higher [engine bearing] speeds, approximately 1.5 to 2 times what they flew when I started [my research]. The other parts of the jet engine [are not] able to handle the higher speeds. The bearings [can]. We ran [the first] 3,000,000 DN bearings in 1973. [We also ran at higher temperatures.] In the late 1960s, we [demonstrated oil and bearing temperatures to] 700 degrees [Fahrenheit. Most of the oils that we tested] were developed by Mobil Oil.

[Last month] I was [at the STLE Annual] Meeting in Florida. I was relating the story about Ed Oberright [to a person who works at Exxon Mobil. He] didn't know some of the background of the oil development. When Ed [Oberright] retired from Mobil, he told me that there were 10 products as a result of the work that we did. This fellow told me that there were now over 100 that Mobil has out now. Had we not done the work—[had Ed not taken] these oils that were developed for the Air Force in the early 50's and gave them to us, and had we not run them in the five-ball fatigue tester to get the results—[these products may not] have come to fruition.

We also did work in elastohydrodynamic lubrication, which is the film thickness [that separates the loaded components in a bearing or gear]. People at the time didn't believe that there was an oil film separating these contacts. The work started in pre-World War II Russia, and was carried over from Russia into post-World War II [Great Britain], and then into the United States in the late [50's. The United States Air Force (Wright-Paterson)] did pioneering work in this area [together with the Battelle Memorial Institute in Columbus, Ohio].

[My work at NASA in partnership with the Air Force, and Battelle showed the relationship between these thin films and the bearing life that you get. The bigger the film, the longer the bearing life. So now how do you get a bigger film? What parameters does the lubricant have to have? Based on the results of our work, the Mobil researchers were able to develop formulations that would improve the elastohydrodynamic film forming properties of the oils. We'd then test these lubricant formulations in the NASA five-ball testers to determine which of these oil formulations were getting longer lives.]

The work that was being done at Battelle was sponsored by the Air Force. This is a result of the Air Force relationship, of going down there [Wright-Patterson Air Force Base], and Ham Smith. We were [then] able to show an interrelationship between full-scale bearings, the [lubricant film thickness] measurements, and the lubricant properties. I think I'm co-author on the first paper that was published in that area, early 60's. It's in that list someplace, "The Role of Elastohydrodynamic Lubrication in Rolling-Contact Fatigue." [1963]

These are the [working] relationships [and friendships] that were built up. We developed work with General Electric [Company] [to test] full-scale bearings. A fellow there named Eric [N.] Bamberger, there's a few other people who were involved, and that carried on into the mid-1970s. We were able to run [bearings to] 3,000,000 DN. Last year it was 40 years [ago] that we [successfully tested] the first 3,000,000 DN bearings.

Myself, Eric Bamberger from General Electric, and Hans [R.] Signer, who was working for Industrial Tectonics [Inc.], built test rigs at Industrial Tectonics with NASA money. The three of us designed the rigs and built them and designed the bearings. Bill Anderson was involved, obviously, with me. There was another relationship, [someone who] just [has] died, Ted [Tedric A.] Harris. He worked for SKF [Svenska Kullagerfabriken AB (Swedish Ball Bearing Factory AB)]. He left SKF and was working with Burt Jones.

At the time, in the 60's, [Burt Jones and Ted Harris were] the analytical heavy-hitters. I had a personal relationship with Ted. Actually, in between, we gave Ted a grant or a contract for $25,000, and he developed a high-speed bearing [computer] program. We had him do an analysis of the bearing Bill and I had designed for 3,000,000 DN, and we needed to change the design a little bit, very slightly. Had we not changed the design based upon Ted's analysis, we would never have been successful. That was the difference between success and failure. I have to give acknowledgement.

I wasn't supposed to be there in the late 1960's or '70s. I had finished law school. And a year before I had finished law school—no one knew I went to law school—I was offered a job to be a patent advisor here. This was 1962. When I started law school in '59, I was thinking that I would, in fact, leave here, go work at the [U.S.] Patent [and Trademark] Office [Washington, DC] for a year, get some experience, and go into private practice. At that time Norm Musial, who went to law school with me—he was actually, I think, two years ahead of me—was a patent attorney here. He said, "Why don't you come and work for us?" I thought, "This is a good opportunity to see if I like it." I can learn something, get a jump on things. At this particular time, I'm a GS [General Schedule pay grade]-12.

There was another interesting thing—let me back off for a second, and remind me to come back to this in case I forget. We've got to go back to the Air Force again, I'm still in the Air Force program. I'm supposed to be getting out. I think in August of 1960 my three years were going to be up. I got a notification from the Air Force of an early out. They had an excess of officers, and the people they considered non-strategic, I was one of them [who had the option of the early out]. If you worked here, you weren't [considered] strategic. It did not matter what [area you worked in].

They were giving the opportunity to leave service earlier, but I had to let them know in two weeks. No one knows I'm going to law school, and I am not going to be finishing law school till 1963, so I'm stuck here. It was a four-year [law school] program, part-time. Normally it would be full-time for three years. I see my good friend, Art Levine. I said, "Art, I've got to make a decision."

He said, "What's that?"

I said, "Well, I just got this notification about either getting an early out or continuing [in the Air Force]. I want to know what my salary will be when I get out."

He said, "You'll be a GS-9."

I said, "Art, when I left here in 1957, you said no matter how long I'm in the Air Force, if I come back, I'll get the same promotions as if I were still working here."

He said, "Yes, that's true, but things have changed. That's not the rules anymore." He said, "When you get out, you can then be eligible to wait at least a year to be an [GS-] 11, but you'd have to go through a promotion board."

I said, "Art, I'm supposed to get out of the Air Force in August. I don't have time to find another position, so I'm going to tell you what. I'm going to stay here till August, but August comes, I'm leaving." Just like that.

He said, "Take my word for it."

I said, "The last time I took your word for something, I ended up in Korea." I said, "Put it in writing."

"We don't put it in writing."

I said, “Put it in writing.”

Abe Silverstein was the [Center] Director, a great guy, probably the best director at NASA, either before or after. Great guy, great man. You’ll probably hear that from other people. You had Gene [Eugene J.] Manganiello as Deputy Director. Gene was also a first-rate guy. These people were—I get choked up—terrific people. Apparently Art talked to Gene, and Gene told them, “Give him a letter,” so I understand it. So I got my letter, and I stayed on. I’m still doing research. If you look at the period of time, you’ll see papers for the year I was in the Patent Office. I was in the Patent Office from January ’63 to January ’64. On my résumé I think the exact dates are listed, but this is practically correct.

I met my wife—she wasn’t my wife then—and that was sort of crazy, too. That was because of the laboratory. You’re going to see how things sort of come together. In 1959, when I got back here, one of the parties who worked here at the lab was a fellow named Sy [Seymour] Lieblein. Sy did some really good work in the area of propulsion. There’s an equation, I think, named after him. He was an older guy. He was like 38 years old at the time, and that, to me, was old. Everything is relative. He was a confirmed bachelor—he passed away a few years ago and never got married. He was in his 80’s.

He [Lieblein] called me up, he said, “Would you do me a favor? One of the guys in the lab here has a sister-in-law who’s here visiting. His wife had a baby, and she’s [here in Cleveland] helping out with the baby. He’s looking for someone to take her around. I said I would, then I found out her age, and she’s 18 years old. I said I’d do it, but she’s far too young for me. Would you call her up and take her out?”

So I called her up and I took her out, and we went out that summer. Her mother thought I was too fast for her.

WRIGHT: That's right, you were a military guy.

ZARETSKY: I was still in the military, yes, at the time. I'll tell you a side story. I had woodblock prints from Japan, and I had them hanging up with a lot of Japanese things. I spent a lot of time in Japan, going back and forth between Korea and then Formosa. I invited her up to my apartment for a sukiyaki dinner, and to see my Japanese etchings.

JOHNSON: So classic.

WRIGHT: Right out of a movie, isn't it?

ZARETSKY: It worked at the time, what can I tell you. Her mother finds out about this, and [said no] and she went back to New York City. At the time, [she lived with] her parents there. Her mother was here with her. I didn't see or talk to her from that time. It was just a summer-type thing.

We now go to 1963. I'm out of the Air Force, working in the Patent Office here, and I have some business in New York. I was still doing technical work, so I was really doing two jobs. I was still finishing up the [research] work that I was doing, and I was working in the Patent Office. The Patent Office was a good experience because I got to know who was doing what around the laboratory and the [technical] area, so I got to be really familiar with a lot of people here.

There was a technical meeting—I was on a committee for ASME [American Society of Mechanical Engineers], and I was going to New York City for a meeting. I'm walking in the parking lot, I'm going to take my car to bring it over to the airport and catch a plane—this is like 5:30—and I bump into Morris Perlmutter, who was my wife's brother-in-law. I knew Morris indirectly, and he says, "Hi, where are you going?" [I told him that I was going to] New York City. He says, "Oh, you remember my sister-in-law? She's out of college, she's a schoolteacher. Why don't you call her up when you're there."

I said, "I don't have her telephone number."

He said, "I have her number," so he gives it to me.

I said, "If I have some time, I'll call her up."

I finished up early, I called her up. She was taking [classes] at Hunter College [New York City] at the time. I arranged to meet her and I took her out. We dated for a year. A year later, we were married, almost to the day.

That's how I met my wife. It wouldn't have happened had I not come here. You see how things connect? You go from Professor Kezios, who literally forces me to take the interview, and then coming here with the Air Force—I almost didn't make it back from the Air Force—and then meeting my wife. Then, I had to make a decision.

One other thing—let me just tell you about the character [and leadership] of the people who I worked for and worked with. When I went to the Patent Office, Irv Pinkel said to me, "I hate to see you go." I already had a promotion from a GS-12 to a GS-13 that was going in. They couldn't give me a GS-13 to go to the Patent Office; I had to stay a GS-12. "If you decide you don't like it or it's not for you, you can always come back, and you'll have your old job back." That's what he told me. I didn't think I'd be coming back. I thanked him, I appreciated that.

A year later, I'm finding this patent work's not for me. My temperament wasn't [suited] for this [type of work]. I knew I wasn't going to go to Washington to [work in] the Patent Office, as I already decided. I was already intent on being married to my wife, so I had to make some decisions. I could quit, but the money I was making was relatively-speaking pretty decent money, and I liked [the engineering research that] I was doing, quite frankly, and how successful I'd been.

I went back to see Irv Pinkel—I saw him with Bill Anderson and Ed Bisson, together—and I said, "Irv, you gave me an offer." I said, "I'm going to take you up on your offer, if you still want me."

He said, "Of course." I was already working with Bill and turning out papers, so we were still doing the work, and Eldred Johnson [was still running the test rigs]—he was a great guy. I went back into the [research] work, and I really never left after that.

That's the basis of the story. That brings you from NACA to NASA. I stayed around here until the 50th anniversary [of NASA] because there were 16 people who were still on the staff—you'd have to verify the number—from all the laboratories who were NACA, who were here from October of 1958—even though I was overseas [with the Air Force], I was still on the books—to 2008, for the anniversary.

One of the things I want to state also, which is really interesting to see—I don't know how your activities are for college or for school, but you've been to reunions? Everyone's been to at least one reunion. You know the spirit of people getting together and seeing each other for a long time. I don't recall what year it is—it's got to be a good 30 years ago, maybe in [1985]—Sandra, you talked about going to an NACA reunion, which reunion was that?

JOHNSON: We were at the 2005 reunion in California, and the 2008 reunion.

WRIGHT: At Langley.

ZARETSKY: This was before. [I believe it was the third NACA reunion held in Cleveland]. They had a [tenth] reunion here in Cleveland [in 2003]. I have the brochure of that. There were two reunions here actually, but at the first one there were people who worked for NACA in the 1930s. They didn't work here, but they worked at Langley, they worked at [NACA] Ames [Research Center, Moffett Field, California].

What really amazed me is the *esprit de corps* that these people had, and the good feelings they had for the Agency and for the people they worked with, and for the people they worked for. That, to me, is an amazing thing. That's one of the most impressive things that I experienced. Art Levine was there, by the way. We were joking about the stories I just told you. There was a lot of camaraderie, there were a lot of people who felt good about what they did. They made a big [contribution]. I don't know if today—let's say we were to get into a space race similar to what we were with the Soviet Union, if we could do the same things that were done. The people who did it, no one had to tell them what to do.

What impressed me when I came here for the first time—it's 1957, for six weeks—five o'clock would come, lights would be on, people would be working. Worked till six, seven o'clock, people coming in on weekends. No one told them to come in. They didn't get credit. They got some compensatory time, but for the most part they were doing this on their own.

Then, when we were keeping the rigs going, no one paid overtime for Eldred Johnson. He came on his own, I came on my own. Other people did the same thing—I wasn't exceptional. [Bob [Robert C.] Hendricks is a good example of this dedication.] I think you talked to Bob Hendricks yesterday. Bob is my senior by a month. He got here a month before I did, and he was from Ohio State [University, Columbus].

Bob is a top-notch guy. I think he's probably the smartest guy—I can tell you a Bob Hendricks story. You want me to tell it?

WRIGHT: Sure.

ZARETSKY: Bob has gotten to be a good friend of mine over the years. I don't know how many years ago, but it's got to be a good 30 years—there was a conference in Hawaii. I told my wife, "I'm going to give a paper for the conference in Hawaii, and we're going to go to Hawaii," so I bought our tickets and everything else. I put the paper in, got accepted.

Two or three weeks before the conference, I get a letter—this is before email—from the conference chairperson. He said, "This is supposed to be a conference on heat transfer"—notice why I got hired here immediately, on heat transfer—"and there is nothing in your paper on heat transfer." Which is correct. It was a stress analysis, a statistical paper, life prediction. It had to do with Weibull. What I was doing is taking Weibull and bringing his work to other areas besides bearings. Basically, I was the transferee.

Here I have my ticket, I promised my wife—and my wife and I have a [very] good relationship. One of the things is if I make a promise to her, I don't break it. She has a good memory too, so I didn't want to have that happen. I'd still be hearing about how we were supposed to go to Hawaii and we didn't go.

I said, “Bob, I have a real problem. Let me tell you the problem. We could put thermal stresses in the paper and I could reanalyze the paper based on thermal stresses, but I haven’t done this since the day I graduated from college.” That was his [line of] work.

Bob said, “No problem.” We sat down, and within about four or five hours we had the paper revised. I should have made him second author on the paper. We did this, and I published the paper, but I don’t recall if he was a coauthor with me or not. I don’t think I put him down because the paper was already accepted. He was giving a paper at the time, too.

Bob is that type of guy. He has been good in just so many fields, and we’ve coauthored—if you look at the list, and probably his list—many papers together, even today. He mentored a person, Brian [L.] Vlcek, who was a high school student out of Parma [Senior] High School [Parma, Ohio]. I just met with Brian in Florida at the STLE [Society of Tribologists and Lubrication Engineers] meeting, and Brian was endowed chairman of the mechanical engineering department at Georgia Southern University [Statesboro]. You see the relationships that have occurred. We’ve also coauthored papers together. Bob was, I think, one of the top-notch guys at this laboratory, or maybe even the Agency, absolutely first-rate guy.

I want to get back to that GM meeting, remember the one I told you about in 1960? Walter Wernitz, from Germany—for whatever reason, I got to be buddies with him at the meeting. I was a young guy. He was significantly older than I was, and he looked like the stereotypical—I talked about Pat Chiarito looking like the consummate Italian aristocrat? I want to say that he looked like a stereotypical [German of World War II], if you look at the World War II pictures—big guy, brawny, stern face. He wasn’t a Nazi, by the way. I want to get that clarified, but the first impression you’d say, “Gee, where did he spend the war?”

For whatever reason, I got to be friends with him, and he came out to visit here in Cleveland. Wernitz worked on traction drives. We [later developed] a whole traction drive program as a result of that meeting. That came about more than a dozen years later. We're out to dinner, he's here in Cleveland, and he tells me about his World War II experience. This is really extremely interesting. This is what makes modern history. The person he worked for was a Professor Lutz [phonetic] at Braunschweig University.

Lutz, as I remember, was his Ph.D. advisor. He got his Ph.D. under Lutz. Wernitz was also a pilot. Lutz was somehow appointed to [a high position in] the German Air Force [Luftwaffe]. Wernitz gets his degree in Germany, it's like 1937, and he comes to MIT [Massachusetts Institute of Technology, Cambridge] [for post-graduate work] as I remember him telling me the story. This is good for history, by the way, it's a good story.

He was at MIT for the postgraduate work, and he gets a telegram from Lutz. Lutz tells him, "We're going to have a war. You'd better get back to Germany, I need you as my personal pilot." Wernitz went back to Germany and he was Lutz's pilot, getting Lutz around. Lutz was involved, obviously, with the higher-ups. I don't know what Lutz's role was or anything like that.

Wernitz tells me [that Lutz wrote to him that, "Before you come back [to Germany], see if you can make arrangements to tour the aircraft manufacturing plants within the United States." He said he did a tour of the United States, and he went to every [aircraft] manufacturing plant to see what was being done from aircraft to aircraft engines. "To this day," he said, "I can't figure out why they let me do it." He said this to me, "Why did they let me do it?"

He said the thing that he noted when he got there was that the new planes—like the [North American] B-25s [Mitchell]—they had front-nose landing gear instead of [a rear] landing gear, standing on an angle and taking off. He [Wernitz] said, "I saw this to be an advantage for aircraft interceptors, so I went back to Professor Lutz in Germany. I told him what I saw." The thing that was innovative and that was different, that the Luftwaffe didn't have, was this. He [Lutz] said, "We need to change the design of the planes."

[Wernitz] said at that time, in 1939 as I recall, [Adolf] Hitler had ordered a freeze on all new aircraft designs, and they wouldn't make any design changes. I remember him telling this story, and I thought to myself, "Thank goodness." In 1943 they had the first German jet, and they put the tricycle landing gear on that. That ended up being there, but that's the result of Wernitz's trip in 1939. That plane could have made a difference. I think it's the ME-109 [Messerschmitt Bf 109]. That's just a little side story about the 1960 meeting.

Weibull was here, and I met with Weibull in [December 1963]. There's another side story about Weibull. I tell this story, it's really funny. I corresponded with him, and just a very interesting mind, very nice guy. Though the Swedes were supposed to have been neutral during World War II, they weren't very neutral. As an aside, I found out a lot of things, that Weibull was telling me. He was a skier, and he would ski in northern Germany while the war was going on. The war was going on, but they were taking vacations in Germany.

These are interesting things, you pick up a tidbit. He was like 72 years old at the time. To me, 72 was old. It's not so old anymore. He would summer in Sweden and winter in Switzerland, and he was in his second marriage. His wife was 38 years old, and she could have been a Swedish actress. She was beautiful. Had an 18-year-old daughter. Gives you some ideas.

He was a visiting professor at Columbia University [New York City] at the time, and he said he bought himself a Buick convertible so he can drive into the mountains of Switzerland.

He said the European cars didn't have enough power, so he got one of these big, big cars. I had made arrangements, corresponding to meet him. I called him up at Columbia, and I said I'm going to be in New York again, and I'd like to take him out to lunch. I made arrangements to meet him at the restaurant called Scandia restaurant, which is a smörgåsbord-type restaurant, Swedish style.

I don't know if the restaurant's still in business, I think it still is. Expensive, by the way. I made arrangements to meet him for lunch there. After I hang up, I called the restaurant and I said, "I'd like to make a reservation for two at this specific time, specific date." The party who I talked to, who I think was the *maître d'*, said, "I'm sorry sir, you know that we're having a waiters strike in New York City. We have a two-to-three-week waiting list."

I said, "I got a real problem. I have a guest from Sweden coming in, and I've already made pre-arrangements to meet him at the restaurant."

He said, "What's your name?"

I said, "Zaretsky."

"Oh, Mr. Zaretsky, why didn't you say so? We'll have a table for you, and what was your visitor's name?"

I said, "Professor Waloddi Weibull, Dr. Weibull."

"Okay, we'll put his name down."

He got there a little early, and I walk in there, "Oh, Mr. Zaretsky, sit down." There were free drinks and everything else. I didn't understand this. It was either Weibull or myself. It couldn't be myself, it must be Weibull. It turns out that the speaker [majority leader] of the New York State Senate was named Joseph Zaretzki, and they thought I was him. Weibull was super impressed, let me tell you. Weibull was so impressed that I made arrangements for him [to come to Cleveland]. There's a picture, by the way, of Weibull—not at the restaurant, but here.

He came down in January [1963]. He spent some time [at Lewis]. I can't take my liquor anymore, liquor is something that my system doesn't handle well. I took Weibull out to dinner, and he drank me under the table. I couldn't believe anyone could have this capacity that he had. But I discussed the technical things that I learned from him—situations and history, the personalities, everything else.

There is a picture in the archives here—I have a copy, I've got it hanging up—with a letter from Weibull at Columbia University, acknowledging the arrangements for him to come here in 1963. There was also an article in, I think, the *Cleveland Plain Dealer* [newspaper] at that time, about him being here. That's just another story about these people. They all influenced the work that has occurred here, and that was applied. It shows the future impact.

One of the reasons I'm still here—people say, "Why are you still coming in?" I jokingly say, "I'm really making up for my extended lunch periods when I worked." In reality, when I'm here—first of all, I enjoy the people, I enjoy the environment. I always worked ahead of myself. When I had my group, I used to tell the guys, "Don't promise anything you haven't accomplished or you haven't finished, because if you put 10 things down and you do 9 of the 10 things, people are not going to remember the 9 things you did—they'll only remember the 10th thing that you didn't do." I said, "Never put that down."

So I had a lot of things going that I never put down. A lot of that is related to the Space Shuttle. I will tell you this, and I'm not trying to be egotistical. If it had not been for me, they would have lost one Space Shuttle after the *Challenger* [STS 51-L accident] for sure. Warning them cost me a lot here, in terms of relationships with people. How do you prove a negative? It was only after the *Challenger* came down that I got a call. They said, "We'll talk to you now." Had they not done so, for sure they would have lost a Shuttle.

I've concentrated [my discussion] on the 1950s, 1960s. In 1970, things started to [change]. The old-timers started to retire and leave, and you had new people coming in. The people who were in management positions here at the laboratory—and I think at NASA Headquarters [Washington, DC] because I interfaced a lot with the people at NASA Headquarters—[nearly] all these people came up from the Centers. They were themselves very accomplished, technically.

People didn't move up in the organization without having really a good technical track record, and for the most part, good management skills. I call it leadership skills. Everyone can be a manager, but not everyone can be a leader. Things started to change in the 70's. The [NACA] old-timers [were retiring]. Unfortunately, there were people who were being put into management positions who really didn't understand, in many cases, the technologies which they were managing. Not that they were bad people or that they weren't good managers; they just didn't have the [technical] background and the experience.

Just as a side note—[it is] my experience that it takes a good engineer or scientist coming into a [new] position, I'm talking about a good one, I'm not talking about someone [who is] mediocre, it takes three to five years for that person to really come up to a level of performance that really is required for a place like this, [to be] a self-motivating contributor.

In most cases, the great ideas that came into this organization—solving problems, new programs, etc.—came from research engineers or scientists who had an idea, and then were able to develop the idea and bring it up through the [management] chain. What was important about this was that the people who were on top [of the management chain] got there not necessarily because they were great managers, but they were great technical leaders. They understood the technology, and they could appreciate the technology. They could appreciate the risks and were able to grasp what would help the people out. This was important. This started to change in the 70's, as the old-timers started to retire, pass away, and [new] people started to come in.

The people who were coming into leadership, technical positions, were not of the caliber that was there before. It, quite frankly, was this. Now you've got a guy like myself, who tells them they're going to have a problem with the Space Shuttle—this goes back to the first time when the Shuttle engines were being considered, who's going to get the contract. They misinterpreted the data on which the design was based. The work on the bearing technology was done here by Bill Anderson and Herb [Herbert W.] Scibbe.

I said they're going to have a problem—and so did Bill Anderson, by the way. I wasn't the only one. Bill did the pioneering work in here, it wasn't me. People took umbrage at that, people took umbrage with that. When the *Challenger* went down, it really bothered me. I was watching it on television. I went in my office and I cried. I couldn't get myself under control because I thought the cause was the bearing. I think the *Challenger* went down in '85?

JOHNSON: 1986.

ZARETSKY: For close to 15 years' time, they had a problem. Not only were there calls here at the laboratory—in fact, I've been told the vice president of Rocketdyne called and said, "Get the guy with the Russian name off our backs," because I was telling them that. When I used to go to the meetings over at Rocketdyne in California, the guys who were working there said, "We can't tell them what you're saying. You've got to tell them this"—they understood—"because we're going to lose our job."

The people at [NASA Marshall Space Flight Center] Huntsville [Alabama] were culprits in this also, although there was guys down there [saying this], too. The guys at my level, we knew, but there was suppression. The environment wasn't good. I could yell out, "You're going to lose a Shuttle."

"Well, who are you to say?" In fact that was asked of me when we [once] had a yelling match down in Huntsville at one time.

I said, "You're going to lose the Shuttle, you're going to lose the Shuttle, you're going to lose the Shuttle." Of course when this happened, I was devastated. They called me up afterwards and they said, "We'll talk to you."

It turns out that they had a problem that no one considered. What I was concerned about was a rub in the oxygen turbo pump because of bearing [wear]—there was a wear problem. The stresses at which the bearing was run were higher than the test data upon which they based the design on. This [work] was done here at the laboratory. It wasn't me who did it, but the stresses were higher [than they should be]. It turns out that the method of lubrication was such that the lubricant, which is really a Teflon [polytetrafluoroethylene] transfer film, couldn't handle stresses beyond a certain amount. The film would then plastically deform and get out of the [ball-race] contact. You would have metal to metal [contact and] wear.

[I was told that after the *Challenger* was lost,] they had two [oxygen turbo pump] rubs: one was on the ground, and one was actually in the Shuttle flight. They didn't lose the Shuttle. It turns out the reason for [the oxygen turbo pump not exploding based on subsequent testing was that] they had what they call single-phase flow, which means it was all liquid [oxygen]. If they had two-phase flow, which would be liquid and gas, they would have exploded the engine.

After the *Challenger* went down, they [discovered that they] had a [bearing] stress corrosion problem that no one really anticipated, including myself, because they did testing on stress corrosion. The testing that was done was limited to six months of conditions, simulating what would be in the Shuttle, assuming that each engine would not be on the ground for more than six months before being fired. [When] they did [the work] there was no indication [of any problem].

Was it the *Discovery* [orbiter] that went up right after the *Challenger*? The bearing [inner ring] had actually fractured in two places. One fracture was straight through [the ring], and [the second fracture] was 60 degrees apart, two-thirds of the way [through the ring]. If I recall the information, and had it gone a little further they would have lost the Shuttle. We went to Rocketdyne to look at this [problem]. We discounted the [cause of the race fractures being wear or lubrication].

We're at Rocketdyne and we're looking at this and I said, "Why did this happen now? How long was the bearing on there?" It was 14 months, if I have the dates correct, between the time it was assembled and the time they flew the next Shuttle, the *Discovery*. One of the guys from Rocketdyne—I wish I knew his name because the guy was really good. After the NASA contract was over, he kept taking data. [When] I went to Rocketdyne he was still taking the [stress-corrosion] data, as far as I know. He kept taking data and he kept the chart. The chart was probably from one end of this room to the other, showing all the data and the time [to fracture]. It was a timeline.

I said, "Wait a second." I'm looking at this stuff and I said, "Here's 14 months. This is what it shows you at the stress level, you predict this." The next Shuttle was supposed to be going up in, I think, nine months. Nine months is right out of this timeline. I said, "You can't run the next Shuttle, you can't run the next Shuttle." At this particular point, I have no trust in anybody. [When] I went back [to Lewis] and I sent out memorandums and made calls and everything else, and they changed the Shuttle turbo pumps with new bearings. That took care of the issue.

If they kept that under six months, it'd have been fine, kept the stresses down, which they needed to do. Then I called Pratt & Whitney, who was designing the alternate turbo pump at the time. I knew the party who was in charge, Bill [William] Poole. I said, "Bill, you've got a problem there. Let me tell you the problem."

He said, "Good thing you told us, because we were designing the same thing into it." So you change your design.

That was another interesting issue about [the Shuttle that] I got involved with. I got a call on the post-*Columbia* [STS-107 accident investigation], same thing. Not with bearings, it was the [body flap and rudder speed-break] actuators. The last paper [I wrote on the subject] just went out. We used Weibull analysis to determine the probability of a failure of the rudder speed-break actuators in the Shuttle. It turns out—they planned these things [to last] for 100 flights each in the 1970s. They planned 100 flights for each Shuttle over a 10-year period of time.

The 1970s—how many is it, 30 years?

JOHNSON: It was 1981 when it [Shuttle program] started.

ZARETSKY: 1981 to 2011, how many years is that?

WRIGHT: 30 years.

ZARETSKY: Not too bad. They didn't run 100 flights, but they had as many as [30 or 31] flights for the *Discovery*, I think. I don't recall exactly. They didn't run 100 flights for each Shuttle, but they never did any maintenance on any of the actuators [that I was aware of]. We did a probabilistic analysis. They sent me a [photograph of a male spline of one of the actuators. I said,], "Gee, this looks terrible." I asked, "What's the history of this?" They told me what the history is, and I forgot how many flights it had, but it had gross wear. I said, "Well, how does the inside [of the actuator] look?"

They said, "We never looked at the inside."

This is 2003, am I right? We tried to get them to open it up and look at these [actuators], and they refused to do it. It really was bad. Now, we've got emails. I'm saturating everybody with emails. Déjà-vu all over again. One of the bad things that I think was a real problem was [NASA] subcontracting responsibility that should be in-house within NASA. We have the responsibility; we shouldn't be subcontracting this outside. The people on the outside, their motivation is bonuses and fees and making profit.

They didn't want to take these things down because it was going to interfere with the [Shuttle] Return to Flight. I kept saying, "You got to do it." They consented to taking [one actuator off a Shuttle] and x-raying it. It turns out that the x-rays showed that the actuator they x-rayed was improperly assembled. It was assembled in reverse. Then they agreed to take the others apart, and it turns out that [they all] had gross wear and corrosion and a lot of other problems.

We did a whole program here, we did it with people at Marshall. We did the probabilistic analysis using the Weibull stuff. That's where the connection comes in. We have [our last] publication [on this subject] coming out now, which [work] was done 12 years ago, it sat on my desk.

WRIGHT: I'm glad you didn't give up on it.

ZARETSKY: That's one of the things I wanted to get out of it. We have about eight [technical] papers [published] related to this [problem]. And test for lubricant—they had never tested the lubricant, they never did anything. If you ran your car like this, you wouldn't be able to run your car. There was no maintenance, no inspection. There was total disregard for safety and maintainability. We say this in the paper, by the way, because it's going to be repeated again. People are going to make the same mistakes.

All the things that we had learned—we did the bearing analysis for this, we did gear analysis. I started the gear program, and also fracture analysis. Lubricant, gears—all the things that we picked up in the basic research in the 60's and 70's, even the 80's, we were able to apply to these particular problems. I think there's also a good probability that there could have been a [third] Shuttle that could have been lost as a result. That's just a little history, but it's a continued story, from the beginning.

WRIGHT: Amazing history. One of the questions I was going to ask you originally, which you certainly have answered—you continued doing research on bearings and made such a difference in the program.

ZARETSKY: It was gears, too. We started the gear work. That gear work started, just to give someone else credit—we talked about Battelle. There was a fellow named C. Malcolm [Mal] Allen who worked at Battelle. He actually was a section head at Battelle. He was a hell of a guy. He made the suggestion, "Why don't you expand a lot of the work you're doing on bearings, that's also applicable to gears?" It's the same technology, "So why don't you do gears?" [This was I think in 1964.]

We put together a gear program and got that going in 1969. That [early gear work was] related to the [Space Shuttle] actuators [30 years later]. We [designed and had built four] gear test machines. In fact, the fellow I recruited from another section of the laboratory, Dennis [P.] Townsend, was put in charge of that. I worked with Dennis. [As with the bearings, the plurality of data published in the open literature has come from these gear testers.] Then we went from gears—the Army put a laboratory here and we went to helicopter transmissions. [John [J.] Coy was put in charge.] We've had a significant impact on [the life and reliability of both military and civilian helicopter] transmissions. Then we were doing the traction work—I told you about Wernitz and traction [drives].

In the early 70's, there was a move to do a Chrysler [Corporation automotive gas] turbine engine. I don't know if anyone's ever mentioned this to you or not. It was DOE [Department of Energy]-sponsored, and they needed a speed reducer. The turbine was going a lot faster than your normal car engine would be turning, so they had to reduce it. The gear technology, at the time at least, was not sufficient to handle it, so we proposed a traction drive.

The traction drive came from Dr. Algirdis [L.] Nasvytis who was himself a mechanical genius. [He had one of the best minds in the field of Mechanical Engineering.] He worked at TRW then left, formed his own company. He came to see us [in 1969] because he wanted [to propose] the traction drive as a concept for a high speed bearing, the 3,000,000 DN bearing. I told him that it was a lousy idea for a bearing but it would make a great [drive] for the Chrysler engine, and we did work here. We had a program going in that particular area. [Stuart [H.] Loewenthal was put in charge. I consider Stu among the brightest people who have worked at this Lab.] These are the types of things that happened as a result of the synergism that occurred between people [both within and outside of NASA. The young people today are not getting the opportunity to be on committees, to meet the more senior people, to interface with them.]

One of the things, I think, is telling the interaction between what occurs very early in someone's career – who you meet, how much you're mentored, what opportunities you have – the trust that's involved between the people that you work with. Had I not had the opportunities to interact with people in my early career, like I've told you, a lot of [positive] things that happened later on, that impacted the space program and impacted safety [may never have happened]. There's other stories I haven't told you about our helicopter work, that if we have some time I'll go into.

There's two things in the world that you're never forgiven for. One is to give unsolicited advice, and second, to be right. This has happened [to me] with the Shuttle and with the helicopter transmission. Unfortunately, in the helicopter area, a lot of people lost their [lives]. It's an interesting story.

There was one person who now is deceased—he was director of research at Marlin-Rockwell Corporation, which is now [part of] SKF—Arthur S. Irwin. Great guy, a personality. I used to introduce him as the biggest man in the bearing industry. He was 6'5". Just a teddy bear of a guy. In my younger days I got to know him. If I had a problem, and I didn't know quite what to do and there was no one here who knew the answer, I would call Art up. "Art," I said, "I have a problem."

He said, "Oh, yes. Back in 1943, at [Glenn L.] Martin Aircraft [Co.] we had the same problem. This is what we did."

These types of relationships are important, and they're important for the people to have because you're insulated here. You have what you have, and that's it. I've talked to some of the younger people, and they don't give papers. I said, "Why don't you give papers?" "Oh, I offer a paper, then I can't go to the meeting or I can't get approval." This is not really healthy for the organization, it really isn't. The more exposure and the more interaction they have with people external to the organization in the technical area, the better the organization is over the long haul—assuming these people stay. If they don't stay that's another story, but even so, it's important for them.

The other thing I want to talk about—I mentioned the helicopter transmission. That was a program that was done basically with the interaction between Headquarters and myself, initially. Bill Anderson was also involved. This is where we proposed a program, and to be candid with you, the management here [at the lab] was not in favor of it. They didn't say no, they just put me down at the bottom of the list. We had a situation where we were working with the Army and we were very successful [but with little support from NASA].

Incidentally, there's some films—I don't know how much you're interested in movies, documentaries, made about some of these things. I had some CDs [compact discs] made from the original tapes. Actually it was real film, and then it was put on the tapes, and then I had some CDs made. I think it's worthwhile, if you're going to have it, and we'll talk about that perhaps later on if you'd like. There's at least three that talk about some of this stuff.

There was a material [steel] that we had tested, and we were working with the Army. We had a very close relationship with the Army, specifically in St. Louis [Missouri]. The Army Materials Directorate, I think that's what they were called, Ed [Edward] Holman was the person who was in charge. Terrific guy. Very, very competent, very concerned, and very supportive.

The Army was doing a program, a heavy-lift helicopter [HLH], and I was on the Technical Evaluation Committee for the Army. One of the materials they were proposing was a new material called Vasco X-2. Boeing got the program for this, and we started to do testing of the material here at the laboratories, on Army support. We began doing supporting [research] work for the Army [beginning] in the 1971 timeframe.

There's a long story, but we found that the material was subject to brittle fracture if not properly heat-treated. We didn't know what the proper heat treatment was because Boeing had developed a heat treat for the material [and considered it proprietary]. There was also a commercial heat treat [in the open literature]. We were using the commercial heat treat, and we were getting fracture. I met with the Boeing people, we told them this, and they said, "Well, you're not using our heat treat."

"Well, we didn't know what your heat treat was."

They said, "Well, we'll heat treat it for you, but we can't tell you how we heat treated it."

They heat treated it and we tested it, and it was good. It didn't fracture, but on the other hand, if anyone else were to use the material that wasn't Boeing's [heat treat], they were going to have fractured [gears].

They were proposing to put this into the heavy-lift helicopter, which subsequently was canceled. Thank goodness it was canceled, "Don't have to worry about this." New chapter, worry about other things. It turns out that Boeing then took the results of the proposal to upgrade the [Boeing] CH-47 [Chinook] helicopter. To be candid with you, I went ballistic with this. I said, "You can't do it because if you go on contract, the lowest bidder is going to get to do the heat treatment, and if they screw up the heat treatment, you're going to lose helicopters." No one listened to me.

Not only did no one listen to me, but the colonel, who I knew, [and thought of as] a nice guy, came here to the laboratory and he told the people at the laboratory, "Get Zaretsky off my back, he's going to prevent me from being a general." I got called in—this is 1979—to the [front] office and told to not make an issue of this anymore. Not to say anything, don't talk to Boeing, don't talk to the Army about this.

I don't say anything more, but Dennis Townsend was working with me on this and he said, "What do we do?"

I said, "We publish this, put this in the [open] literature." It's like the Shuttle again, it was déjà-vu over again, same psychology. He asked, "What do you do about this?"

I said, "We can't do anything." First of all, how do you prove a negative? We're assuming someone's going to make a mistake, do an improper heat treatment. [We had already published a preliminary NASA paper on the steel in 1977. We also] published a NASA TP

[Technical Paper] [in 1980]. It's on the list of publications [that I provided to you, you will find three papers listed].

Then, [October] 1984 comes, and it's [NASA management's] revenge time. At that time I [headed] the Bearing, [Gearing, and Transmission] Section. It's in my résumé. All of a sudden, I'm called in. They took the section from me, and they made me what they call in Japan a "window man." This was before the *Challenger*. There was an edict that they will not do any more bearing [or traction drive] work at this laboratory, and that I was without a group. [Also, much of the helicopter] transmission work [was cancelled]. We had a lot of projects going, a lot of programs going. Things were [nearly all] canceled. A lot of the transmission work was canceled. All the bearing work was gone, and I didn't have a job. The other thing with regard to this Vasco X-2 material—because I still had contact with the Army, I said, "You're going to lose a helicopter."

The last time I went down [to Huntsville] before the *Challenger*, there was a yelling match going on at the meeting. I can remember someone with a [thick] German accent [yelled at me], "What right have you to tell us this?"

I was as arrogant as I could be [when I replied]. I said, "Because I know more about the subject than anyone sitting in this room. You're going to lose a Shuttle."

What happened was they got rid of me, basically, and I didn't have a job. I went up to see the Director of Aeronautics. He was an acquaintance, he wasn't a friend. I was actually offered a job outside [in the private sector], but I didn't want to disrupt my family. My wife was working, kids are happy, we're living a good life. I said to him, "Hey, listen, I'll take my salary and if they don't want me to work, I'll sit around and I won't work," if that's how they want it. I said, "If you think I'm going to retire or get another job"—I think that's when he smiled, like acknowledging—"I am not going to do so. I suggest perhaps you find another position for me here," which he did.

I went from having a group of 15 people, including myself, to zero. No bearing work was supposed to be done anymore. I told you, I've always worked from behind—I have a file drawer full of data. What they didn't do is take me off the committees for the Shuttle. [They must have forgot to.] I still went down to Huntsville, I got invitations. When the *Challenger* went down, I got called for the *Challenger*.

No one told them [at Marshall] we weren't doing any more bearing work. I was the sole guy here for bearings and lubrication—well, not quite for lubrication, there was lubrication work—but no work done on bearings. I was made chief engineer of the Structures [and Acoustics] Division. I wrote two books subsequent to this, and this gave me a little more time to write the books.

Ed Bisson [in the early 1970s] retired. But Ed was very active [in technical society affairs]. He was [former] president of STLE. I didn't know this at the time. He contacted STLE, he said, "Have Zaretsky write a book." He suggested the title of the book, and they invited me [to write it]. I never knew that until years later. Ed, even though he was no longer here or affiliated with the laboratory, was still looking after me. It was really nice. You feel emotional about this. I ended up writing this book, [which was published in 1992]. Of course, I got involved with the post-*Challenger* Return to Flight, which would save the Shuttle. Now we flip ahead 10 years, to 1997. The exact date is the week that Princess [of Wales] Diana was killed in [Paris, France]. There is an international tribology conference, sponsored by the Institution of Mechanical Engineers in Great Britain. I'm giving a paper there. In fact, when we got there, the funeral was going on for Princess Diana. My wife and I watched the funeral, we were staying in a hotel near the church.

They have a [conference] dinner—the British do [things] very well, their dinners are pretty formal—an evening dinner, and my wife's with me. We're sitting at this [round banquet] table, and there's a gentleman to my right. Older man, older than I was at the time. I introduced myself to him, and he said, "I know who you are." I can't remember his name, by the way. I should, but I can't remember his name. He said, "I used your work."

I said, "What work is that?"

"You know, on the Vasco X-2, your [1981] ASME paper."

I said, "You did? How did you use it?"

He said, "I was an expert witness on the crash of a Boeing helicopter in the North Sea. Sixteen people were killed, and it was like you predicted in your paper."

I hear this. All of a sudden, [when I get back to the United States,] there are Army helicopter crashes. This was on the national news. The helicopters, the CH-47s, were crashing. What happened is that there was a company that Boeing subsequently bought out that was manufacturing the gears here in southern Ohio. They improperly heat treated the steel, and they started to have [gear] fractures, and they started to have helicopter crashes as a result. This made national news. There was a whistleblower down there who blew the whistle, and I ended up a government witness as a result of the [ASME] publication.

[I have a newspaper clipping from the [Cleveland] *Plain Dealer*. They wanted to interview me. The NASA management wouldn't let them talk to me.]

Just an interesting side, from [our 1981 ASME publication date], we're talking [over 16] years later. Interesting how things happen with time. I don't know if they're still using the material or not—I haven't heard of any recent incidences. There were subpoenas [issued]. The Justice Department was involved. All the people who were involved here and at the Army and people who were trying to cover up were all subpoenaed. I don't think that there were any criminal [indictments] for this—I don't know if it was criminal or not—but it was certainly negligence.

[Basically, what happened to me in 1984 launched me on a second NASA career. My individual research moved ahead. It actually was a professional opportunity. In 1989 I received the NASA Award of Merit in recognition of my support of the manned flight program and more specifically my post-Challenger work. In 1990 I received the NASA Medal for Exceptional Engineering Achievement. I was appointed to the Senior Scientific and Professional Corp. with the Civil Service Grade of ST in 1998. This is the highest grade attainable by a Federal Engineer or Scientist. In the aftermath of the Columbia tragedy, I received the "Silver Snoopy Award," the Astronauts' Personal Achievement Award in 2004. Also in 2004, I received the NESC Director's Award for my work in advocating and repair of the Shuttle's Rudder/Speed Brake Actuators.

In 1992 my first book came out. In 1997 my second book was published, "Tribology for Aerospace Applications." In it] we got everything together that was done for the previous 50 years and the history of how we got into lubrication [and mechanical component research from NACA to] NASA and the transition. I had gotten [information and history from] Bisson and Anderson and a lot of other people. I put in the book about the brittleness of the [helicopter gears and] the stress corrosion problem [with the Shuttle bearings], just in case that would go away, or that someone would forget about it. At least it's in print. That's just a little background. Robert [S. Arrighi] has the book because it has the history in the Preface of the book.

In 2012 I was honored by the STLE and received the Society's highest award and honor, the STLE International Award for "Lifetime of Contributions to the Field of Tribology.]

Now I want to talk about Joe Savino. Joe was a very [competent] guy. [He had a PhD]. He had a terrific sense of humor, a nice guy. Joe had this idea that our country needed to conserve energy. He was a little bit of an idealist, by the way, just to give you a little background. This was in the late 1960s. Of course, we were headed that way. The Chrysler turbine car [engine] was being [worked on at the Lab]. Joe proposed a wind turbine program. As I recall it—this is my memory, now—the people who were running the Lab, especially this energy program, wanted to do solar energy instead of a wind energy [program]. Joe was pushing—not with the blessing of the [management], but on his own—[to do Wind Energy Research at Lewis].

[Joe succeeded in getting] a Wind Energy Program here [under DOE sponsorship]. In fact, my group sized the first gearbox for that. What happened was that they didn't have enough money to buy the gearbox, so we figured out how to get a cheaper gearbox off the commercial market. I think the gearbox would have cost, at that time—we're talking, like, 1970—I think it was $60,000. I had a friend who worked for Horsburgh & Scott [Company], and we got a similar gearbox that would work for $15,000. My philosophy is don't pay retail if you can buy it wholesale.

Joe was successful on that. When they did it, there was Mod-0 [modification] and I think there is Mod-1. Had it not been for his pushing and his advocating the wind turbine, there wouldn't have been a Wind Turbine Program. Even with that, I don't think we would have any wind turbines in this country today, or perhaps in the world, had he not been instrumental in doing what he did. No good deed goes unpunished. He was here in 1955, and I don't think that he ever got to be a GS-15. Joe recruited me for this laboratory and I used to kid around with him, I said, "Your major contribution to the Lab was recruiting me." I said it tongue-in-cheek, I didn't mean that, but Joe was a good guy. He did the work, and he's a guy who was a hero.

Joe was not recognized, [to the best of my knowledge] for his contribution, and that to me is an injustice. I don't know if anyone's left who worked on the Wind Turbine Program [here at the Lab]. He never got to be in a manager position for the NASA Wind Turbine Program. I read his obituary when he died. I didn't go to his funeral because I found out about it after the fact—but I read his obituary, and there was nothing said about [his contribution to NASA or the Country. He] was one of the guys who made an impact and a difference.

A lot of the projects and programs, certainly in the 50's, in the 60's, into the early 80's up till about 1979 were [started and advocated by individual researchers like Joe, but the] complexion of the Agency changed in '79. The old NACA was gone. By the late 70's, NACA was history. It was history [both in fact] and memory, as far as I can tell. By the early 1980's, all the old timers had retired. The people who were in positions of leadership, who could make things happen, were no longer there. We got into a new era. The era was politically motivated. We brought in outside people who were not familiar with the Agency. Their backgrounds weren't the same, the culture wasn't the same, and it affected the way things were done.

I had no support when these things came up with the Shuttle in the 80's and with the helicopter transmission. I had absolutely no support, I had zero support. If I was in a private company, I would have been fired, there's no question about that. The fact that I stayed on – it had a positive effect, both on the technology and on the people. There was a [personal] responsibility, and I feel good about discharging it.

I got credit for what I did. I got [awards and] rewards, you see it there. I got recognition, but no one said, "You were right." There was another guy who was a hero. I didn't mention his name, but I don't want to take all the credit. There was a fellow named Fred [J.] Dolan. He was my counterpart at Marshall, and Fred used to tell me that he can't say the things that I'm saying. He's been telling them about [the bearing problems on the Shuttle turbo pumps] on a quiet basis, but I made it an issue. I would yell back. I would scream back if I were yelled at. He couldn't do it, but he was a supporter. He recognized the problems and issues. [For a number of years] we couldn't penetrate the people who were making decisions [at NASA] and it was unfortunate.

[Fortunately], the original problem turned out to be a [benign] problem, but only in the sense that there was no explosion, and the problem that we didn't recognize, at first, was a problem [that had Fred] and I not waved the flag, we could have lost a Shuttle [after the *Challenger*]. I think that was another thing. There are relationships between [people] and this is important – the early work and the experiences that you have and the people that you meet, and what you do subsequently.

WRIGHT: You did decide to leave in 2009, is that correct? In January was when you actually retired?

ZARETSKY: Yes, January. Actually, if I retired a year before my pension would be 5 percent higher today because there was a COLA [cost of living adjustment] raise that came in 2009. January 2, 2009, and I stayed because my total time—by the way, just for purpose of the record—is 51 years, 6 months, 1 hour, and 15 minutes. I never got paid for the hour and 15 minutes. Walked out the door at 6:15.

WRIGHT: Well, we're glad you came in today to talk to us. We appreciate that.

ZARETSKY: I still have the office here. They just don't want to clean my office out. I still am turning out work that's related. I finish up, that's my responsibility. We get it taken care of, then I can know I did what needed to be done.

WRIGHT: Those papers are important, and documenting that information is important. Proves over and over and over again, for anyone doing any type of substantial research.

ZARETSKY: Yes, I think there's a lot of people involved. I named some who I thought were key people, were influential, but there's a lot more people. Most of them are gone now, they're no longer alive. I get a call periodically, someone died, and that's what's going to happen here. There were a lot of people who, I think, made some major contributions over the decades that are forgotten, and they made things happen.

I'll say this. If I were starting out from scratch, just like I told you from the beginning, I don't think in today's environment I would be able to accomplish anywhere near what I accomplished. Let me talk about the 3,000,000 DN bearing. There's no 3,000,000 DN engine out today, and I don't think there's going to be a 3,000,000 DN engine, certainly in our lifetime, because other parts in the engine are life-limited because of the speed.

You still are running a higher speed than we had before, at least 50 percent higher, maybe even as much as twice as high. The life of the bearings is potentially 400 times that of 1950, and 80 times more than 1960, and that's the result of the work we did here. We didn't do all the work—we had the GEs, the Pratt & Whitneys do work, some of the bearing companies—but the primary mover on this whole area was ourselves. Myself, my colleagues, Bill Anderson, and we were enabled to do this by the people we worked with [and for].

I'll give you an example of what happened, because this talks about Abe Silverstein. Abe Silverstein was a forceful leader. You either liked Abe or you didn't like Abe, because he held you accountable. If you made a commitment, you better keep the commitment, because next time you're not going to get a chance. That's how it was. People knew this. When we were proposing to do—this is the mid-60's, like '64—the 3,000,000 DN program, when we were going out to GE on a substantial amount of money—of course today it seems like a little bit, but it was for about 15 years. I think we averaged about $125,000 a year. Some years more, some years less. That was big money.

Abe called around—this was the SST, "What are we doing for the Supersonic Transport?" One of the things we were doing was the high temperature. This was not necessarily a 3,000,000 DN bearing *per se*, but it was a high-speed bearing with the 3,000,000 DN bearing put in there. He would go around the table, and everybody who was working on a key component for the engine—if you're going to be a propulsion laboratory, you have to have people who are experts in each of the areas for which there are components, and which technology improvements need to be made.

He'd go around the table, and he comes to me. It's Anderson and Zaretsky, A to Z. He says, "Anderson, Zaretsky," just like that, "What do you need?" Bill looks at me, he says, "Okay, tell him." I told him how long it would take, what we thought it would take at the time. We told him it was several years, we would need an average of $X a year, like $100,000, maybe more, and we'd have to get test rigs built. That's a big upfront money.

He said, "How long do you think it would take?" We told him an estimate, that it would take, like, two or three years [for the test rigs] if we were lucky. He said, "All right, I want you to have a purchase request ready to be signed in two weeks, and I want to contract out with the next number of months. If anyone gives you a hard time, tell them Abe wants it." Just like that, "If there's any problems, let me know." That's how it worked. Things got done. "Abe wants it."

Do you have a little more time?

WRIGHT: Yes, we could probably do one more story.

ZARETSKY: Okay. This is a side story. One of the things that I told you is that we built our test rig, that five-ball test rig, in three months. We had a shop here, and the people in the shop were really craftsmen. They could do things here because they weren't only here to make money. They would experiment and they would do things from a standpoint of—forming titanium was one of the things that they developed here at the laboratory. They developed some methodology for doing [things] that were unique.

The first Mercury capsule was here. I don't know if it's this building or the next building over. The prototype of the first Mercury Capsule was built here, 1959. One of the people who ran the shop was a fellow named Henry Meltzer. Henry was from Hungary, and he talked with a little bit of an accent. We would go, "Henry, I need to get this part done." He said, "When do you need it by? Can you wait till after lunch?"

WRIGHT: Amazing.

ZARETSKY: "Can you pick it up in the morning?" That's how we were able to turn things over. I got to know Henry real well, so over the years, when we're building up test rigs or we have issues or a rig is down, I would go see Henry. Henry would look at me from across the shop and says, "My cousin, my cousin." He was saying this just in jest, there was no relationship, but everyone thought I was Henry's cousin.

If I needed something done, I would get priority service. A lot of the other guys I worked with, they always said, "How do you get things done so fast, and we got to wait a week or two weeks?"

I said, "I don't know, they must like me." That was because of Henry. If things needed to be done fast, I would run it over there with a purchase request or work order, and ask Henry if he would help expedite it for me. That's just a little side story. A lot of that was done here.

What I want to say, there was a lot of camaraderie. There used to be social functions here on Friday nights sometimes. They had movies—this is before television was a big thing. A lot of social functions at the laboratory. During the holidays they would have, for the small children, Santa Claus-type things, which was nice. We got the families in. People—and I think you're going to get this from Sol Weiss and Cal [Calvin W.] Weiss and some of the other people that you'll be talking to—there was a lot of camaraderie, there was a lot of good feelings.

It's a little sad when I think of these people, and then realize they're no longer with us. I think that made the basis for the success both of this laboratory and the Agency. A lot of the people went from here to Washington. I think George [M.] Low was from here. Abe Silverstein brought George in, I think. This was before my time, and I didn't know George Low personally. I knew Abe personally, got to know him.

There's another little story about Abe, which I think is a really interesting story, should be from a historian standpoint. Abe got the Guggenheim Award [Guggenheim Fellowship]—I don't know if you're familiar with this—and the Crawford Auto [-Aviation] [Collection] museum, that's where the award was given. It's a nice facility, by the way. I was there, it was a big crowd. I don't remember who sponsored the event. Abe was there, obviously, because Abe has been retired for quite a while.

It was buffet style, in terms of serving. They didn't serve it in individual tables. It's semi-formal, but everyone's in line. I'm in line and I hear a voice behind me, saying, "Zaretsky, still writing papers?" I turned around, it was Abe Silverstein. I said, "Yes, Abe, still writing papers. I'm going to have a problem."

He said, "What's your problem?"

I said, "I don't know what I'm going to do with my office when I retire." You know how many years ago this is? "I've got these papers and these files and everything else, and I can't bring them to my house." I don't remember if I was living in my old house or my new house, doesn't make any difference which house.

He said, "You think you have a problem? You should see my house and my basement. I have wall-to-wall boxes of everything. I have no place to move it to." We [finished going through the line, finished] dinner, and [Abe got] the award [and gave a speech]. It was a nice evening.

I don't know if you knew or know Bonnie Smith? Do you know Bonnie Smith?

WRIGHT: No.

ZARETSKY: She was hired here as a contractor in the 80's. She had Robert's job, she was a [Glenn Research Center] historian. She [subsequently] went to [The] Aerospace Corporation. I haven't talked to her in a number of years. Bonnie is a very nice lady, and she shared an office with the Report Control [Office]. I get over to Report Control, and they introduced me to Bonnie. I talked with Bonnie, and she was telling me they're trying to accumulate [historical NACA/NASA] papers. I told her the story about Abe. I think Abe had passed away at the time. They contacted Abe's family and they were able to get all the archived pictures, historical pictures [and documents]. It went back for a long time. Just a little incident that happened at the dinner, and I told her the story. I said, "See if you can get it, that would be a lot of historical, archival material."

WRIGHT: That's great. Such a richness in that legacy that tends to get lost when people throw boxes away.

ZARETSKY: Yes, exactly right. They don't have to worry about me, but I've got to give this all to Robert. He's going to have to take care of it.

WRIGHT: Yes, I think they're ready to start processing.

ZARETSKY: I've run out of stories, I think.

WRIGHT: I bet you haven't, but we probably have run out of time, so we'll probably need to close up now. We certainly thank you for spending as much time with us as you did.

ZARETSKY: Yes, it was interesting. I enjoyed it. It was a good catharsis, too.

WRIGHT: Good.

[End of interview]