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WALTER G. VINCENTI
INTERVIEWED BY REBECCA WRIGHT
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WRIGHT: Today is July 15, 2014. This oral history session is being conducted with Walter Vincenti at his home in Palo Alto, California, 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 for letting us come to your home today.

VINCENTI: My pleasure.

WRIGHT: We'd like for you to start today by telling us about your years before you came to NACA. I understand that as a child, you moved to Pasadena, California, and you were so close to Caltech [California Institute of Technology]. How were you influenced by your move here to California, and how did that bring you to NACA?

VINCENTI: As you say, I grew up three blocks from Caltech, but I was not anxious to go to college three blocks from home. I had two older brothers who had gone to Stanford [University, California], and so I came to Stanford. I got interested in aeronautics in a somewhat surprising way. When I was 10 years old in 1927, I was at a Saturday afternoon movie for children, which they had in those days. In those days, during the movie, if there was something important in the world, they would come on the screen with a printed slide, telling you what had happened. In

the middle of this Saturday afternoon movie, a printed slide came on the screen saying, "Charles Lindbergh has just landed in Paris," and we all stood up and cheered. Then, I got interested in building model airplanes. From the time I was 10 years old, I built model airplanes and got more and more interested in aeronautics. When I came to Stanford, Stanford had an aeronautics option in mechanical engineering, which I took as a graduate student. I had two years of graduate work for an engineer's degree.

I didn't have and I don't have a Ph.D., but I had two years of graduate work. There's one thing that was very interesting. In those days, Moffett Field was the home of the [USS] *Macon* [ZRS-5] dirigible, and the *Macon* carried five small biplane airplanes in it to increase its range of searching. At a Saturday afternoon football game, Stanford versus USC [University of Southern California] in the Stanford Stadium, at mid-time, the *Macon* dirigible from Moffett Field flew over the stadium and stopped. The doors opened up in the bottom of the dirigible, and you could see the five airplanes up there. They let one of the airplanes down on the launching trapeze and let go of it, and it got up to flight and flew off, and then the dirigible left. If I needed anything, as a freshman student, to increase my interest in aviation, you couldn't have done anything better.

WRIGHT: What were your plans at that time?

VINCENTI: My plans were to study what Stanford had to offer in the way of aeronautical engineering. Stanford, in those days, was the home of a very great man, William [F.] Durand, after whom a building is named at Stanford now. He was one of Stanford's greatest engineers ever, and was very much into aeronautical matters. I knew that Stanford, at the graduate level,

not as an undergraduate, but at the graduate level, had a specialty in mechanical engineering, a specialty in aeronautics, and that was my plan. I stayed for the two years of graduate work to get that.

WRIGHT: About that time, they were beginning to formalize the plans for the Ames Aeronautical Laboratory [now Ames Research Center, Moffett Field, California].

VINCENTI: Yes. When I was finishing my second [graduate] year, was about a few months away from my degree, Russell [G.] Robinson was out here and turned the first spade of dirt for the Ames Laboratory. It just happened that Russell Robinson was a graduate of Stanford University, in the class of 1928. I was class of 1938. Robinson had been coming to Stanford, and we students got to know him. He offered me and a classmate of mine, Charles [W.] Frick, a job at the Ames Laboratory. We accepted. We had to report for work on June 1, which was two weeks before commencement, but we did that. We arranged that with our professor.

Frick and I were the fourth and fifth engineers on the staff at the newly being constructed Ames Research Laboratory. When we went to work there on June 1, the Ames Laboratory consisted of a construction shack, a half-built flight hangar, and surveys and excavations for a couple of wind tunnels, and a lot of dirt roads. The first winter, there was a lot of mud, so talk about being in the right place at the right time. Frick and I were there, and we grew with the laboratory and got right in on literally, the very beginning.

WRIGHT: When Russell Robinson talked to you about coming to work at Ames, what did he tell you that you and Mr. Frick would be doing at the new laboratory?

VINCENTI: I don't remember. We had heard a great deal about the NACA. Our aeronautics professor at Stanford at that time, [Elliot G. Reid] had worked at Langley Field [Langley Aeronautical Laboratory, now named Langley Research Center, Hampton, Virginia] and he came to Stanford in 1927 from Langley Field, and was one of their stars. He had talked to us about the NACA. William Durand, who was a retired professor, had been the [second] chairman of the NACA, so we had been exposed at Stanford to the existence of the NACA and it seemed like a very, very natural thing to do. I remember it very well. Once, when we were working in the Stanford wind tunnel, we had a visit from the great Theodore von Kármán, the great man from Caltech, who asked me if I wanted to come to Caltech and study with him for a Ph.D., but I didn't want to do that, and so I didn't. I went to work for NACA, and it was the right thing to do.

WRIGHT: You were no longer a student and you were newly employed. Where did you live and how did you become more integrated to the NACA? Were there places for new employees to live nearby?

VINCENTI: My parents had moved up here from Pasadena when my younger sister, who was the youngest of the five siblings in our family, went to Stanford. I lived at home with my father and mother for the first years that I was at NACA. They lived in Los Altos, which is just a few miles from Moffett Field.

WRIGHT: I understand you helped inspect some of the new buildings that were coming up, is that true, as they were being built?

VINCENTI: Yes.

WRIGHT: Could you share with us what that entailed?

VINCENTI: It entailed work for which Frick and I were not qualified, really. We went out and watched what the people were doing in actually building the buildings. I remember once, I went out and watched the contractors laying the concrete for the parking areas for the aircraft in front of the flight hangar, something I had no experience at, no qualifications at, but I was supposed to okay what the people were doing there, in building those. Especially, the things that were most interesting were the work we were doing on the first two wind tunnels. Frick worked on the 7 x 10 [Foot] Low-Speed [Wind] Tunnel and I worked on the 16-Foot High-Speed [Wind] Tunnel. We worked on the drawings for the construction and all kinds of things involved in the construction of the wind tunnels. We got in on the building of the laboratory, literally.

WRIGHT: You really did know the buildings inside and out, didn't you?

VINCENTI: Yes.

WRIGHT: You actually worked on the design of the Supersonic Wind Tunnel, is that correct?

VINCENTI: Yes. After the laboratory got started, I was assigned to a high-speed research division under the direction of the great [Harvey (Harry)] Julian Allen. We got into the small Subsonic [Wind] Tunnel, and then we could see that the knowledge of supersonic flow was something we needed for the future. Allen got us to work on making a model of the supersonic tunnel, and then on designing one of the United States' first two supersonic wind tunnels; ours and the one at the [U.S.] Naval Research Laboratory, back in [Washington, DC], were being built about the same time.

We had to do a lot of work, and the only documents we had to go on were some reports from Germany. The Germans were the leaders in the field, so we spent a lot of time digging out German references and studying them, and we then designed and built one of the country's first two supersonic wind tunnels, which was a 1 x 3 [foot]. The test section was 1 foot wide and 3 feet high, and I was put in charge of the wind tunnel. After we got going, which that would have been just about the beginning of the war, I was put in charge. I was a section head for the wind tunnel, and I had about 20 engineers working my staff, and about 8 or 10 young women who did the calculating in those days, when we didn't have electronic calculators. They did it on hand-driven [calculators]; they were our calculating machine. I was in charge of this group. Should I say something about the group?

WRIGHT: Sure.

VINCENTI: It turned out it was a very remarkable group. I had four project engineers who were in charge of individual projects, and we'd have a meeting every Friday afternoon to discuss what had been going on that week. I had four people. I was the oldest; I was 27 years old. I was the

oldest. I had two years of graduate work; I didn't have a Ph.D. Engineers, in those days, didn't get Ph.D.s. Of those other four in our group of five, there was one with one year of graduate work, and the other three had bachelor's degrees. The one with the one year of graduate work was a man named Jack [N.] Nielsen, and the others with bachelor's degrees were Milton [D.] Van Dyke, and oh, my goodness, I have a terrible problem with names nowadays. We had, as I say, very little specialized graduate training. Nobody had run a supersonic wind tunnel before, so the boss, Julian Allen, our boss, and his boss, knew less about our problem than we did, so we were left completely alone. The people in Washington didn't know anything. They couldn't give us orders, so it was just wonderful. We made up our own research program and worked at it, and set in the results purely on our own.

It just happened, I don't know how it happened, but those four men were a remarkable group of people. Of the four, three of them and I became members of the National Academy of Engineering, and the last one, the fifth one, he became a venture capitalist and made more money than the other four of us put together. The three that joined me in becoming members of the National Academy, when the war was over, they went back to Caltech and all three of them got Ph.D.s. They became very prominent people in the profession. Again, we were all in the right place at the right time, and made quite a reputation for ourselves. I was told later by a man who was at the Washington office of the NACA at the time that our group was turning out the most valuable research work of any group in the NACA. We were very fortunate, and I was there for quite a few.

There's something that's sort of interesting, stop me if I'm going too far in this, after the war was over, I had an interest not only in experimental aerodynamics but in theoretical aerodynamics. I had done work in both, and when the war was over, after a couple of years, I

got tired of being in charge of the wind tunnel and I wanted to go back to doing some theoretical work. If I gave up being in charge of the wind tunnel, they would have had to reduce my salary because it was required to have a certain salary, and you must have responsibility for people below you. I didn't want to take a salary cut, so one day, I happened to find myself in the company of Hugh [L.] Dryden—you know his name, I suppose—and I told him about that dilemma and asked him, "Is there any possibility, Dr. Dryden, that you could get around this restriction?"

He said, "I'll see what I can do." So he went and a couple months later, I found that this requirement had been eliminated. They could let people go into work, which didn't require people working for them, and so they took me out of being in charge of the wind tunnel and put one of the other people in, the one who became a venture capitalist, and they put him in and put me back on theoretical work. I was able to do some theory, which was new, and I was able to make the first calculations of the drag of an airfoil through the transonic speed range.

I was very fortunate because while I was at Ames in the mid-1950s, John D. Rockefeller, Jr. gave money to the federal government that could buy employees the equivalent of a sabbatical year, and government agencies could recommend someone. If the [Rockefeller Public Service Award] committee decided that this was right, they would give that person a year's salary plus extra expenses to spend a sabbatical year away from the government job. I was fortunate in that the NACA decided that I was the person to nominate, and I was able to get it.

I went to Cambridge, England, to the University of Cambridge, and was able to spend a year there studying the physics and chemistry of high-temperature gases. We could see that high-temperature aerodynamics was coming along with reentry bodies coming back into the atmosphere. The air becomes a chemically reacting mixture of nitrogen [and oxygen]. I spent

the year attending lectures at the University of Cambridge and reading books about the physics and chemistry of high temperature gases, chemically reacting gases.

When I came back in 1956 from Cambridge, it just happened Stanford was forming its new aeronautical engineering department in the School of Engineering, which had not been a separate department before. Five aircraft companies, at the request of some of their engineers who had graduated from Stanford, saw that the Stanford professors that did the aeronautics teaching in the mechanical engineering department were about to retire. There was danger that Stanford would close that activity because they didn't think it was worthwhile continuing. The [graduates] in the industry said, "We mustn't let this happen," and they got to work, and each one persuaded their company to give Stanford $5,000 per year for five years to form a new department. That doesn't sound like a lot of money today, but in those days, that was $25,000 a year for five years. It persuaded Stanford to start a new department.

When I came back from Cambridge, in England, Stanford offered me a professorship. I'd be one of the two professors to start the new department. The other one was the man that I knew who was a professor, was the head of the department of aeronautics at Brooklyn Polytechnic [Polytechnic Institute of Brooklyn, now named New York University Polytechnic School of Engineering]. He had offered me a job at Brooklyn Polytechnic three years earlier, and I declined. He was a Stanford graduate who had been a graduate student at the same time that I was. He was a Hungarian, and had come to this country. He was 10 years older than I, but they were offering him and me to be the first professors in the new department at Stanford. He was a structures man, and I was in aerodynamics, and we were to build up that part of the department over a period of years.

We got to work together there in September of 1957. In October of 1957, the Russians put up Sputnik [satellite], and Washington came and pushed money at us. We didn't have to go and ask for it. They came and said, "How much can you use?" We exploded. Within three years, we had one of the biggest research budgets of any aeronautics department in the country, and had increased our faculty much faster. Again, in the right place at the right time.

I should have said, when I came back from England and Stanford offered me the job, I debated. I was very happy with my work at the Ames Research Center, and they had, after all, given me this opportunity to go study, so I didn't want Smith [J.] DeFrance, the Director of the laboratory, to think that I was in any way unhappy with things at the laboratory, if I took the job at Stanford. I went to see him at his house one Sunday afternoon, and I said, "Smitty, Stanford has offered me this job, and I don't know whether to take it or not. I'm not the least bit unhappy—I'm very happy with the work at Ames, and you've been very good to me."

He said, "Well, Walter, I think you should take the job at Stanford. You can probably be, in the long run, more use to us by educating new people that we can give jobs to as you could by working at it." Interestingly enough, one of my first Ph.D. students, Dale [L.] Compton, became Director of the Ames Laboratory. Unfortunately, Smitty DeFrance was not living by that time, and I couldn't show him how right he'd been, that one of his successors had been my Ph.D. student.

WRIGHT: When you were talking about Mr. DeFrance, you referred to him as "Smitty," even though he was the Center Director. Talk to us about the type of organization and the atmosphere that was NACA while you were there.

VINCENTI: I don't think we called him Smitty to his face, but there was something very interesting about DeFrance. He had come here from Langley Field to head up the laboratory. Back in the 1920s, at Langley Field, he had been in an airplane. He was a pilot, and he'd flown in an airplane that crashed. He survived the crash, which was quite remarkable, so his wife made him promise he would never fly again, which he didn't. Here he was, working for NACA at Langley Field, and never went up in an airplane again, as a passenger or a pilot.

When he was Director of the laboratory here, they couldn't fly him to Washington to take orders, so the laboratory here had an independence of Washington that it wouldn't otherwise have had, thanks to what his wife had required. Smitty was so much wanted for the job, they took him anyway. If he needed to go to Washington, he needed to go by train, and he would do that maybe once or twice a year. That was all. We got less direction from Washington than Langley Field did, where people from Washington could go down there in one day's drive.

WRIGHT: Talk about his management style. Did he give you direction, or were you let alone to come up with your own research projects throughout your years there?

VINCENTI: He didn't give us much direction. He gave more direction to the people who were doing research in the subsonic wind tunnels because that was his field that he'd grown up in, but he didn't know anything about supersonic flow. Harvey Allen was the one who gave us directions. Allen was very, very flexible, and a very unusual person that we were related to. There's one set of circumstances about the Ames Laboratory that I don't believe is recorded anywhere, that maybe is unique.

We were, during the first couple years of the war, deferred in the draft. About halfway through the war, or a little more than halfway, the draft boards changed their policy. The work you happened to be doing did not qualify for deferment, so we were up to be drafted. But, the military wanted us, those of us at Ames, to continue the work we were doing. We were doing research which was fundamental to the development of military aircraft. The draft boards wouldn't recognize this. Since the Navy wanted them, and the Navy had the Naval Air Station at Moffett Field, the Navy decided, after we were drafted, that we would be put in the Navy. The Navy would assign us to Moffett Field, and the captain at Moffett Field, the commanding officer at Moffett Field, would assign us to the Ames Laboratory during the day. We'd be doing the same work we were doing but at night, the rest of the time, we'd be Naval.

Most of us at Ames, as Naval officers, qualified to be commissioned officers. I and a few others, because of my eyesight, I didn't qualify to be a commissioned officer. I qualified to be a noncommissioned officer. I was made a chief petty officer. In the daytime, when I was heading the [Supersonic] Wind Tunnel at the laboratory, of the 20 or so people who were working for me, there were [mostly] commissioned officers. In the daytime, I, as a noncommissioned officer, was giving orders to commissioned officers, as high as Naval first lieutenants. The highest officer—I've forgotten what his rank was—in the Navy contingent was one of my people in my staff. So, in the daytime, I was giving directions to the commanding officer of our detachment, but at night, as I did, occasionally, on shore patrol duty, he might be giving me orders. That was the kind of thing that went on. It led to some interesting things.

I remember one day, I was in my office at the wind tunnel, and I just came in there in the morning, and I took off my jacket, which had my rank indications on it, and took off my necktie, so that I was ready to do my day's work. I had no indication of my rank. Lo and behold, a

knock came on the door, and Smitty DeFrance came in with an admiral. The commanding admiral of the Naval Air Detachment Corps, or whatever they called it. They sat down and they wanted me to tell them about the work we were doing. I figured since I don't look like I'm a Naval [person,] I won't call him "sir." We just went on for about an hour or so, and I didn't give him any indication of the fact that he was my commanding officer. DeFrance told me, a day or two later, that after they left my office and they were going back to his office, the admiral said, "Is that young man in the Navy?" Smitty told him that I was a chief petty officer, and all the admiral said was, "Well, I'll be damned."

One other thing I must tell you, which is we had in our group a very unconventional fellow named Marty Klein. When you were first being in the Navy, you weren't commissioned yet. You spent the first six or eight weeks as just an ordinary sailor, to get broken in. Marty didn't very often wear the uniform that he should. One day, he was picked up by the Shore Patrol in downtown Palo Alto because he was out of [proper] uniform. He had on part of his uniform, but not the other part. He was called up at the captain's mast to get properly disciplined by the commanding officer of Moffett Field, and so he was all properly dressed. We said to him afterwards, "Well, Marty, for once in your life, I'll bet you were properly dressed." He said, "I was wearing argyle socks." That's the kind of thing that was going on at the Ames Laboratory during the war. Those are the things that are not anywhere in print that I know of.

WRIGHT: Thank you for sharing that. You mentioned that you were telling the admiral what kind of work you were doing. What types of projects were you working on, then, for the Navy or for the military?

VINCENTI: We were doing, at that time, very fundamental research on the properties of aircraft wings at supersonic speeds. There weren't any supersonic aircraft in those days, but the aircraft companies were beginning to think about designing them. If the war went on long enough, there might be supersonic aircraft. We were doing the first research on finite-aspect-ratio wings for supersonic aircraft. Plus, some fundamental research of other kinds, but that was typical of what made the military, the Army Air Corps and the Navy, interested in what we were doing.

WRIGHT: When you were in your group, looking to design your wind tunnel, you mentioned that you did research on what the Germans had done for their wind tunnels. How were you able to gather that research? Where were those materials that you were able to read, and how did you get those?

VINCENTI: A lot of it had been in published reports before the war. The Germans had done this prior to the war, so it was being published in the reports that were available, but nobody in the United States had paid much attention to them at the time the Germans were doing it. The Germans had a very great tradition of aerodynamic research, going back to a man by the name of Ludwig Prandtl, who was the man who came up with the basic idea of the boundary layer, and was one of the great men of all time in aerodynamics. They had a man who'd been a student of his by the name of Adolf Busemann, who was one of their earliest people in supersonic aerodynamics. We had to dig into those reports and learn, and had to do it to educate ourselves as we were working.

WRIGHT: As the war began to close, how did the jobs and the work that you were doing at the Center change? Did you continue some of those projects, like the supersonic work?

VINCENTI: No, it just progressed and became more refined, and just like typical research, we were learning things which raised an interest in new questions, and we learned more and more. We didn't find anything that became of practical use before the war ended. We were one of the leaders.

WRIGHT: Talk about the publishing process you went through, and how often did you publish your works and your findings to share with others in your field?

VINCENTI: Before the war, the NACA published technical reports, which were published on a project, the technical report, and at the end of the year, they'd be assembled into a thick volume of the NACA reports, which were definitive works. Besides the printed reports, they published some technical notes which were typewritten reports, typewritten and reproduced in numbers and made available to people, but they weren't considered as definitive as the ones they'd put in the printed reports. We would write those reports and they would be edited and published. During the war, though, those reports were classified. They were still made in reports, but they went to a select audience who had reasons important to the government to have them.

That's about all I can think of to tell you. A lot of the history, of course, of Ames, books have been written about the known history, so if I told you anything about the regular things that went on, it'd be repeating what, in the books that Glenn Bugos and others have written.

WRIGHT: Could you talk to us some about the tools that you used. When you and your group were designing the wind tunnels or doing your research, you mentioned you had the group of women that helped process your math, but were there other tools, maybe, that you developed, or other instruments that you used, or pieces of innovation that helped you do your work?

VINCENTI: Nothing that helped with the calculations. All the numerical calculating was done on Marchant calculators, which were things that sat on the desk and you pressed in the numbers, and you'd do multiplication, division, addition, subtraction, on these calculators. In the early ones, you had to drive them by turning a crank. As time went on, they became motorized, electric motors, but they still had to be operated by hand. I made the first transonic airfoil calculations, and it took three young women about one year to do the necessary calculation, and the only reason they assigned them that much time, was because this was considered important enough. Those calculations could probably be done today [by electronic calculators] in less than a minute. To think it took three young ladies one year, and now, we do things like that without giving it a second thought.

WRIGHT: Share with us some of your personal information as you spent those 17 years at Ames, you mentioned that you had lived with your parents for the first year or so. Did you live close by Moffett Field and the Ames Center after you worked there a few years?

VINCENTI: Yes. I lived in Los Altos, if you know, which is about four or five miles from Moffett Field. There were other people that lived out there, and we had a ride group that would go there every morning and bring us home, and take turns. I would drive a week, then one of the

other people would drive for a week, and so forth. That kind of thing went on. That was during the war as well as when I was working at Ames during the 1940s, but I got married in 1947. My wife and I lived together. She's no longer living—it's why I'm here alone. She died a year ago.

WRIGHT: Sorry to hear that.

VINCENTI: The paintings there are her work, in the other room, and that's a self-portrait of her. She was an artist.

WRIGHT: Was she local? Was she from this area when you met her?

VINCENTI: I met her on a railroad train in the freight yards at Council Bluffs, Iowa, when the train was stalled by the floods. She was living in Southern California. We met on the railroad train, and three months later, we got married. We were married for 66 years. She was a very accomplished abstract artist, and the large painting, Sandra can see it there, over my bed, is one of my favorites of her paintings, and the others are hers, too.

JOHNSON: That is beautiful. The colors are just beautiful.

VINCENTI: Yes, she had a wonderful sense of color. There's another one you can't see, back in that corner, which is more colorful. She sold paintings and had several shows in San Francisco.

JOHNSON: That's quite an accomplishment, especially around here.

VINCENTI: Yes, yes. San Francisco is a very active place.

WRIGHT: You mentioned you had brothers that were engineers—did they work at Ames as well?

VINCENTI: No, no, they weren't engineers. They went to Stanford, but one was a lawyer and one was a businessman. The one that was a lawyer became the president of the Tournament of Roses in Pasadena.

WRIGHT: Your father must have been very proud he made the choice to move you all to California.

VINCENTI: My father and mother were born in Italy. My father came to the United States as a boy of 16, with very little money, and an eighth grade education. He let his children know he came to the United States so they could amount to something. My siblings, I had two brothers and two sisters. We all worked hard. Our mother had a sixth grade education. It's a classic story of the first part of the 20th century, people coming from Europe and raising families in the United States.

JOHNSON: What did your sisters do? You mentioned that one of them was going to Stanford, the youngest one.

VINCENTI: My sister was a teacher of romance languages, and then, during the war, she went into the Navy. She was a Naval, what did they call them?

JOHNSON: WAVES [Women Accepted for Volunteer Emergency Service]?

VINCENTI: She was a WAVE, and spent most of the war in Hawaii, doing work for the WAVES. After the war, she went into the early days of the CIA [Central Intelligence Agency], and was sent abroad in Southeast Asia.

JOHNSON: That's an interesting area to be in at that time in the history of the world, too.

VINCENTI: Yes, she did a lot of interesting things. After the war, she was assigned to the embassy in London [England], but she was doing work in a group that was studying what they should do about Fidel Castro in Cuba. They didn't want to have this group in Washington for fear that it would be discovered. They wanted this group to work by itself without any notice, so they sent them to London. She was involved in that, and she went through a couple of marriages and then she became, after the war, in the CIA, and spent quite a few years in their office in Chile. She did all kinds of things.

JOHNSON: Amazing woman. For the time period, that's just amazing, what she did.

VINCENTI: During the war, before she went into the WAVES, she applied for a job at Douglas Aircraft. She was living in Southern California. She wasn't getting anywhere, so she sat down

one evening and wrote a letter to Donald Douglas, the president of the company, in Spanish, and sent it to him. Two days later, she got a phone call from Douglas' secretary, who said, "Come on in." That was one way to get a job—to go around the Navy red tape.

WRIGHT: You mentioned earlier that you had a student that later became the Center Director of NASA at Ames, and you also mentioned that when you were a student, your professors had told you about NACA, so what did you share with your students about the qualities of NACA?

VINCENTI: He was already employed at Ames, and when he was my student, he was given time off from his work at Ames to do his Ph.D. work. At that time, he and another of his colleagues from Ames were both my students, and they both did their Ph.D. research in related fields, so they both used to come to my office, the two of them together. Ames was closely involved, and another man that we hired very quickly after Washington started pushing money at us was Milton Van Dyke. We had quite a few. Ames was sending people over to us to take advanced training. We were creating quite a service for Ames. We had some excellent students as a result.

WRIGHT: What lessons or what do you believe you learned while you worked at NACA that you could share with your students as valuable lessons to take to their jobs?

VINCENTI: Just talk very positively about Ames, as my professor had done, who had been, gosh, why can't I remember his name? I did my dissertation under him.

WRIGHT: We'll have to see if we can find that for you.

VINCENTI: It'll come to me, probably after you leave. He had worked for 10 years at Langley Field before he came to Stanford and had talked very positively about NACA, before Ames came into existence. Russell Robinson, who had graduated from Stanford in aeronautics 10 years before I did, had been his student. This professor became a full professor at Stanford at the age of 27. He was the youngest full professor, I think, at Stanford, maybe in any department, but at least certainly in the School of Engineering. He was quite a brilliant man, and he had written a book which was very important, which we used. So our professor, who was such a distinguished person in the field, had worked for NACA. That itself was testimony we didn't have to hear about, we just knew. William Durand was, I think, the [second] chairman of NACA, and he was one of the people that had to do with the formation of NACA. NACA and Stanford had a close connection.

WRIGHT: Did you have any regrets of going to Stanford at the time that the space age was beginning? You could have had an instrumental part.

VINCENTI: No, I felt very fortunate that I had done that because people needed this kind of education we were giving them. The country needed a department like ours, and we very quickly became, along with MIT [Massachusetts Institute of Technology, Cambridge] and Caltech, the three top departments in the United States—and still are. Those were the three outstanding aeronautical departments. A Stanford graduate was Brad [Bradford] Parkinson, who was the key man in putting up the global positioning satellite [GPS] system.

WRIGHT: The list is long, isn't it?

VINCENTI: After he got his doctor's degree at Stanford, he went into the military, and then after he got out of the military, he came back and was a professor at Stanford. There has been that kind of thing going on.

JOHNSON: Was it Elliott Reid [professor at Stanford]?

VINCENTI: Elliott Reid, where did you get that?

JOHNSON: Out of this article right here.

VINCENTI: Yes, Elliott Reid, good heavens. I know that as well as I know my own name. What is that?

JOHNSON: It's an article that was from *Stanford Aeronautics and Astronautics*. It's a little history. I found it online.

VINCENTI: Is it about Reid?

JOHNSON: They mention him, but it talks about Durand and Reid and you and different people in the aeronautics department. It's the history of the founding of that department.

WRIGHT: Do you have any questions?

JOHNSON: When they made you part of the Navy, when you couldn't get the deferment and then you became a chief petty officer and you had people working under you that were higher ranked, during that time, did you still receive the same pay as you were before? Or did it revert to Navy pay?

VINCENTI: Navy pay. For the first six months or so, I lived there on the Naval base in the Navy dormitories. We were required to, and we went through boot camp. We were part of the Navy, for sure.

JOHNSON: I was just wondering, since you had already been established at a federal level with Ames, and then you have to go into the Navy, was that a cut in pay?

VINCENTI: Yes. Definitely.

JOHNSON: Then people working under you were making, I would assume, more money than you because they were commissioned.

VINCENTI: Yes, that's right, but we didn't think about that because the war, after all, there was a war going on. We were right here in California, worried about the Japanese. At Ames, the dirigible *Macon* was gone, but Moffett Field, the Navy station there was the base for blimps that

went out over the Pacific here, patrolling, looking for Japanese submarines. There were 13 blimps in that hangar, and one day, in order to take some photographs, they put all 13 blimps in the air at the same time. They were all flying close to each other over Moffett Field, so that they could take a photograph of all 13 of them in one photograph. A fellow that shared the office with me, looking out the window, said, "Now, I know what it feels like to live in the bottom of a goldfish bowl." Those blimps were all around us, day and night, they were always on patrol out here.

JOHNSON: Did they beef up security at Ames during the war? Was it any different?

VINCENTI: Yes.

JOHNSON: Did you have, like now, when we go through the gates, we have to show a badge and that sort of thing because of security. Was it similar when you were there?

VINCENTI: Yes. I've forgotten the details, but yes. It was very much a Navy base.

WRIGHT: Mr. Vincenti, were you working the day of Pearl Harbor [Naval Base, Hawaii, attack by the Imperial Japanese Navy, December 7, 1941]? Were you actually at work? I know it was a Sunday morning, but I don't know if you were working.

VINCENTI: It was a Sunday morning?

WRIGHT: I believe so.

VINCENTI: No. I was playing tennis with one of my fellow workers from Ames, Carl [Carlton] Bioletti, who had been one of the three people working there when I was hired. I heard a car drive—we were playing tennis on a tennis court in the Stanford campus, and there was a street that went by—a car went by and I heard something about the Japanese bombing Pearl Harbor. It had happened on a Sunday, as you say. I yelled across the net to Bioletti, who was on the other side, farther from the street, "Carl, the Japanese have just bombed Pearl Harbor."

He said, "That's Orson Welles again." Orson Welles had put on a show on the radio about some fictitious military happening [1938 *War of the Worlds* broadcast]. Afterwards, we found out it wasn't Orson Welles. That evening, we were having dinner in the home of Carl Bioletti's sister, in the hills back of Redwood City. On that evening, that woman was serving Japanese food for dinner, so there we were. We figured if word got around about that, they'll come and raid us.

WRIGHT: The years that you were able to do your research and your work at NACA, you were always seeking new challenges. How did you know what areas that you wanted to go into? How did you know what theoretical work you wanted to research?

VINCENTI: It was just our own instincts, really. I picked out the problems I wanted to work on, Harvey Allen, our division chief, was very good. We would have a luncheon. There was a cafeteria under the Administration Building at Ames, where we would go for lunch. The four or five of us, with Allen, we'd sit around and have our lunch together. A lot of very serious work

was done at those luncheon tables, of getting ideas, of not doing the work itself, but getting ideas about what to work on by bouncing ideas off of each other.

The fact that we had these luncheons together was very important in the operations of the laboratory because we would be people from different departments of the laboratory, might be different people in these luncheons but they were usually always around Allen. He used to invite us over to his house after work, sometimes. He was a great chess player, and we'd go to Harvey's house and play chess in the evenings. We had a social as well as a research and business relationship. We felt like Allen was a friend of ours.

WRIGHT: What do you think is what you consider to be the most significant accomplishment of your time at the NACA?

VINCENTI: There are two things that I would rank equally. One was with the supersonic wind tunnel, the definitive work we did on the effect of variations in plan form on the performance of wings at supersonic speeds. We did the first work of that kind, and it led to a number of reports, about four or five reports, that were basic for that. The other was the theoretical work I did with these three young ladies. There was a very interesting thing that happened with that work. About the time I finished these calculations, or my women finished them—laying out the calculations was quite sophisticated. It took some very sophisticated basic mathematics.

This was about 1947. We gave a meeting at the laboratory at Ames for professors, and we had people from all over the country coming, and we're bringing them up to date on all the work going on at Ames. We gave papers on it, and I gave a paper about this theoretical work of this airfoil. When I was finished, a man jumped up in the audience in great excitement. I found

out later it was Hans [W.] Liepmann, the great professor at Caltech, saying, "I have a student who is just completing experimental measurements on this same airfoil. This is wonderful."

He invited me down, I went down two weeks later to Caltech, and gave my paper over again. We went out into the wind tunnel, where Arthur [E.] Bryson, his Ph.D. student, had just a week or two before finished measurements at the same time that I finished the theory, and we plotted his experimental points on my theoretical curve, and they agreed. You couldn't have planned it any better.

Those experiments are just the experiments you would have wanted to run if you knew the theory was going on. It's just incredible how these two things fit together, by sheer chance. When we published, then we referred to each other's work. And Bryson, this Ph.D. student is now a professor in our department at Stanford. I have lunch with him every Friday. He's a great man in his field, he's one of the leaders, and he's the winner of the Daniel Guggenheim Medal [engineering award]. It's the darndest coincidence you can possibly imagine. I got to know Hans Liepmann, who was a great man. These fields are full of interesting things that happened. Some of the things I'm telling you I don't think are in print anywhere.

WRIGHT: I'm glad we're collecting these. I think what you just said, something I was reading you had said about engineering being such a communal field, of sharing that, is that an example of what you're talking about, of being able to share that knowledge and do that?

VINCENTI: Yes. Engineers learn a lot from each other. I'm glad you found the name Elliott Reid because that would be troubling me the rest of the day.

JOHNSON: I'm glad we could provide that for you.

VINCENTI: That's one thing I find, that in getting old, the memory for names—because names are arbitrary. They don't logically connect for anything else. I find it very difficult to remember names, some of the times, of people that I know them and I have to track them down in some way.

WRIGHT: It's good that it's easier to track down now. We have more tools to find things.

VINCENTI: Yes, Google is a big help. I've used that to sit there at that thing [pointing to computer].

WRIGHT: Are there other stories or experiences that you want to share with us today? Any other pieces of information that maybe haven't been?

VINCENTI: No. I don't think of anything else that pertains to—I'm trying to think if there's anything more from Ames.

WRIGHT: Any experiences in the wind tunnels that maybe you haven't shared before, talked about?

VINCENTI: No. When I first went to work there, I worked on the design of the wind tunnels and when the 40 x 80 Full Scale Wind Tunnel was being built, they put me to work on the design of

the doors that open above the test section of the wind tunnel. I didn't have anything else to do with the 40 x 80, but I did design those doors. If you ever go to Ames and see the 40 x 80 doors open, those are my doors.

When they were designing the laboratory in Cleveland of the NACA, what's the name of that?

WRIGHT: Lewis Research Center [now Glenn Research Center].

VINCENTI: Yes, the Lewis Laboratory. We designed their refrigeration wind tunnel. Carl Bioletti was in charge of it and I was working with him. Their wind tunnel was a high speed, pressurized tunnel, but it was also a cool tunnel. I'm not quite sure why, but they wanted to run these at very cold temperatures. When the tunnel is running at cold temperatures, the shell of the tunnel tends to shrink and pull away from the reinforcing rings on the outside. Very fortunately, one of my top professors at Stanford was the great Stephen Timoshenko. Is that a name you've ever run into before?

WRIGHT: Yes, when I was reading that history, I was reading about him.

VINCENTI: I had taken all of his courses in the design of material—not aerodynamics, had nothing to do with aerodynamics—but because it was the great Timoshenko teaching them, I knew how to tackle that kind of problem because the shell pulling away inward would put special thermal stresses on the shell, and that in turn would put thermal stresses on the reinforcing rings. I worked out a scheme for doing that. I just happened to have had the right

education to do that particular job. I'm probably the only person in the NACA who happened to know about that kind of thing. When I finished my method of working it out, I went to see Timoshenko at his home one evening and showed it to him. Here, I got the consulting of one of the world's greatest people in that field free of charge.

WRIGHT: I'm sure he was proud of his student.

VINCENTI: I worked for him as a graduate student on one of his books because he was a Russian and he wrote his books in pencil, longhand, in English. In Russian, there are no definite and indefinite articles, so he had me take his pencil manuscript and go through it page by page, putting in definite and indefinite articles. In Timoshenko's *Theory of Plates and Shells*, the definite and indefinite articles were my work.

When I became a professor at Stanford, his wife died. He was emeritus, and he was going back to Germany to live with his daughter, who was living in Germany. He came into my office to say goodbye to me. As he was leaving, he went to the door and he turned around and he said, "You know Vincenti, this is the first time in 50 years I haven't been writing a book." The man just wrote one definitive book after another, and they were the defining books in his field.

I was very fortunate; I had excellent professors at Stanford. Elliott Reid, in aerodynamics, Stephen Timoshenko in solid mechanics. I learned how you use mathematics to solve problems, and I had a wonderful mathematics teacher, Harold [M.] Bacon, for freshman mathematics.

I doubt if I could have gotten as good an undergraduate education if I had gone to Caltech. Caltech looked upon itself as a science school, not an engineering school, whereas at Stanford, engineering was engineering. We had people like Durand and others. I did a little work for Durand when I was a graduate student on a government report. He was chairman of a three-man governmental committee that was studying the possible effect of the Grand Coulee and Bonneville Dams on the migration of salmon in the Columbia River. They were the ones that recommended the building of those steps that the salmon could go up.

I got to know the great Durand. He lived to five months short of 100, and he was completely active his whole lifetime, up until his middle eighties. His first engineering job as an officer in the U.S. Navy, after his graduation from Annapolis [Naval Academy, Maryland], was as engineering officer on a wooden-hulled sailing ship with auxiliary steam power. His final job as an engineer was as chairman of the U.S. Government Committee Fostering the Development of Jet Engines. This man, in one career, went from wooden sailing ships, to jet airplane engines, and was involved in all of it. If I were a young professor again, I'd write that man's biography because his biography would be at the same time, the story of a fundamental development in American engineering from a cut and try operation to an analytically based operation. He was one of the people guiding and promoting that development, as a professor at Cornell [University, Ithaca, New York] and then at Stanford.

WRIGHT: What a lifetime.

VINCENTI: Stanford was fortunate because when he was at Cornell, he was a Naval architect and had done the basic work on ship's propellers. His wife was taken ill and the doctor said, "Look,

get her away from these severe winters in Ithaca and go to California," and so, he applied to Stanford for a job at Stanford, and got it, fortunately. The NACA, during the First World War, needed to work on aircraft propellers. Durand was the recognized world authority on ship's propellers, so he was the one to logically take—and he effortlessly was on the faculty at Stanford.

Durand had come to Stanford by that time, went to work and was given—it was either the NACA's first or second research contract with a university. I think maybe MIT, under Jerome [C.] Hunsaker, had gotten the first one, but Durand was given a contract to do work on aircraft propellers, which was the start of 10 years of work that he and [Everett P.] Lesley did. The first work, they designed the wind tunnel, built and tested the propellers, wrote a report which was published, all in 13 months. It was just incredible what these men did. Part of that 13 months, Durand was in Washington, DC, doing government work, and Lesley was running the tests here at Stanford alone. He would write a long letter to Durand every week about what had been done that week. Durand would read it and write a letter back. The letters, of course, went by train in those days, so they would be crossing in the mail.

I've written in a book, there's a chapter about the Durand-Lesley Propeller Test, which I wrote, and I was doing research for this chapter. A friend of mine, or a fellow I just knew casually, heard that I was writing this book. He said the government archives have these letters from Durand, and he had copies made of those letters and sent them to me. There I was, right in the middle, week by week, of the work going on. It was wonderful. Talk about things flowing in over your head—the thing that you wouldn't think to look for, because I didn't know it was done that way, so I refer to those letters in my chapter.

Everett Lesley, I got to know. He was still a professor. He was younger than Durand. These propeller tests by Durand, Nicholas [J.] Hoff, my friend who was the other professor with me, that started the new department at Stanford, he was from Hungary and he'd been an aircraft designer in Hungary for 10 years. He had used the Durand-Lesley Propeller results to pick out propellers for the airplanes he was designing in Hungary. These things get around.

WRIGHT: They do. That's very interesting. Thank you for giving us your afternoon.

VINCENTI: Thank you for listening.

WRIGHT: It was fascinating.

VINCENTI: It's fun to remember these things. I miss it, by the way.

Having mentioned Nicholas Hoff, I'll tell you something. He was about in his early thirties when he came from Hungary to study under the great Timoshenko. He was going to study and get his doctor's degree and then go back to Hungary, but the war broke out. There was a young woman, Vivian Church, who was secretary of the Dean of Men at Stanford when I was a student in the 1930s. I was a freshman student at Stanford 80 years ago. She ran the Dean of Men's office to the extent that we referred to her, the students referred to her as "Dean Vivian." She was a graduate of Palo Alto High School. He came here and was studying under Timoshenko, and Vivian Church was well known by every undergraduate male student. She was just a figure.

Lo and behold, one weekend, this glamorous Hungarian eloped with Vivian Church to Reno [Nevada], where you could get married and you could take out a marriage license and be married the next day, and here, you had to wait a month. Here, this glamorous Hungarian had stolen Dean Vivian and gotten married. She was older than he was, but she was a very well known person. There is now a Vivian Church Hoff endowed professorship at Stanford. Her husband, she died before he did, so he gave the money. He made a fairly good sum of money in his lifetime, although it was purely as a consulting engineer. He gave the money to Stanford—they didn't have any children—to endow. The head of the department at Stanford now is the Vivian Church Hoff professor. Interesting history.

WRIGHT: That's pretty neat. We all leave our marks—that's nice that she's got that. Thank you.

[End of interview]