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ORAL HISTORY TRANSCRIPT

FRANK H. SAMONSKI
INTERVIEWED BY JENNIFER ROSS-NAZZAL
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ROSS-NAZZAL: Today is December 30th, 2002. This oral history with Frank H. Samonski is being conducted for the Johnson Space Center Oral History Project in Houston, Texas. Jennifer Ross-Nazzal is the interviewer, and she is assisted by Sandra Johnson and Rebecca Wright.

Thank you for joining us today. I have a couple of questions for you about your career before you began working at NASA. Could you tell us how you developed an interest in engineering?

SAMONSKI: Okay. I guess it started in high school. I took a course in mechanical drawing, and I kind of was interested in that. So after I graduated from high school, I went to Trenton Technical Institute [Trenton, New Jersey], and they had a course there for mechanical draftsmen. It was a two-year curriculum, and I got a certificate from them. But before I was quite finished, I took a job with Allstates Engineering in Trenton, New Jersey. It was a contractor. They work for different people. General Electric was a big customer of theirs in making the electrical power switchgear equipment and the like, as an example. They also had an Atomic Energy Commission contract for the Savannah River Project.

Anyhow, I worked as a draftsman. One of the engineers that I worked with was a man by the name of Bill Clancy. I asked him how you got from an idea to a thing, a machine or something like that, and he told me that paper was patient, that man could do anything with a drawing and the like, which I thought was really pretty good.

But I was sort of the detail draftsman, and he was the designer, and I was kind of impressed with the job that he did. He was a mechanical engineering graduate from Brooklyn Polytech [Polytechnic University, Brooklyn, New York], and I decided I wanted to be a mechanical engineer.

The Korean War was on then, and I was eligible for the draft, but rather than wait, I went and volunteered for the draft and served during the Korean War, not in Korea but in Germany. While in Germany, I took the Scholastic Aptitude Test in Heidelberg, Germany, and made application to a number of universities, and decided when I got out in May of 1955 to go to Rutgers University [New Brunswick, New Jersey]. I started there in September of that year. So that's how I got to be interested in mechanical engineering.

ROSS-NAZZAL: You actually took a job with the Space Task Group after graduating.

SAMONSKI: Yes. The last summer, the summer before my senior year, I worked for the Naval Air Turbine Test Station in Trenton, New Jersey, a very sophisticated test facility. They test jet engines for naval aircraft. I was assigned as a test engineer's assistant to a project engineer who was conducting a test program on a Westinghouse jet engine. His name was Bob Babbington. He knew at that time that there was a lot of talk within, I guess, government and in industry, that there was going to be a man-in-space program. This was the summer of 1958. There were already rumors then was going to be an agency formed, a space agency, which then happened that fall, I guess in October or something, with the National [Aeronautics] and Space Act.

That next spring, when I was in my last semester of school, NASA Langley Research Center was one of the employers who came and interviewed at Rutgers, and I interviewed with them, and they subsequently made me a job offer as a research engineer in aeronautical instrumentation at Langley Research Center in Hampton, Virginia. It was my understanding that when I reported to work down there in early June 1959, that I would be working in a wind tunnel doing work on aerodynamics, air foils and the like.

Upon reporting for duty, the personnel man asked me if I would like to work on Project

Mercury, and I really didn't know. You know, the last few months of school, our first son was born in middle of April 1959, and graduation was on June the 2nd, and finals, and I really didn't know we had a man-in-space program, so I said, "Gee, it sounds interesting.

So he sent me over to the other side of the field. The main part of Langley Research Center is on what's like the west side of the field, all the wind tunnels and the like, and then over on the east side it's an older section of Langley, and that's where the Space Task Group was.

So I was greeted there by a fellow by the name of Chris [C.] Critzos, who was an engineer doing administrative kinds of things, and he sent me up to talk to three different people, to see what I would be interested in doing. One of those was Dick [Richard S.] Johnston, who turned out later to be my supervisor. Dick was responsible for the pressure suits, the integration of the man into the capsule, a number of other things, and the environmental control system. He was all by himself, so it sounded like it was going to be interesting. So I said I'd like to work on the environmental control system. So that's what I did.

ROSS-NAZZAL: How did you actually learn about creating an environmental control system? What had you known before this time?

SAMONSKI: Well, already McDonnell Aircraft Corporation, St. Louis, Missouri, was under contract to NASA's Space Task Group to produce the Mercury capsule, and they had as part of that contract AiResearch as a subcontractor for the environmental control system, and it was pretty well along there. I mean, there were schematics drawn and the like, but not quite much hardware produced yet. So I guess I kind of learned as things were being made. Yes, I guess so.

ROSS-NAZZAL: What interested you about the environmental control system?

SAMONSKI: Well, it's thermodynamics and heat transfer, and the sort of stuff I studied as a mechanical engineer in school, and it sounded like it would be interesting work and important work, too, providing all the life support functions and the like.

ROSS-NAZZAL: Why don't you tell us a little bit about the environmental control system for Mercury. Tell us about some of its components, how did it work.

SAMONSKI: Okay. The Mercury environmental control system was basically a pressure suit support system. The astronaut was in the very small capsule, fifty cubic feet, the capsule was, or so, in a pressure suit, and he was connected to the environmental control system that was kind of beneath his legs, underneath the couch that he sat in. He sat with his feet up sort of like this [lifts legs], and underneath there, that's where the environmental control system was. He was connected to it with the hoses, one inlet hose that went [in at] his waist, and then a return hose that came out of his [helmet].

So the gases leaving the [helmet] would be elevated in temperature and have some carbon dioxide added and depleted in oxygen, and also carry some body heat and the like. And so the system worked, like there were several components in series. The first one was a solids trap to catch any physical debris. There was a lot of concern about the astronaut heaving in the pressure suit. They even tested with, I guess, a can of Campbell's vegetable soup, I think, to see how effective the solids trap was.

Then there was two fans or compressors, only one of which operated, to move the air. Then there was a canister with a chemical, lithium hydroxide, that absorbs carbon dioxide. Then there was a heat exchanger that took out the heat that was added by the man and by the carbon dioxide reacting with the lithium hydroxide. It also condensed out the moisture that was there from the man and from the lithium hydroxide reaction. Downstream of the heat exchanger was a water separator to remove that water, and then that air was delivered back to the pressure suit with its inlet hose.

There were some sensors and the like that sensed pressure. As the pressure in this suit circuit decreased slightly—and I'm talking very small changes in pressure; it measured in inches of water—a thing called a demand regulator would resupply oxygen on demand. That demand regulator and another regulator in the cabin supplied the makeup oxygen. Both of those were fed by a couple of high-pressure, 7500 psi [pounds per square inch], tanks of oxygen through regulators.

That's basically how it worked. It was a single-gas system, although the capsule started off at launch at sea level pressure, maybe a little bit above sea level, because of making a leak test of the hatch when they closed them up. On launch, that pressure would begin to relieve. It relieved through a valve that measured differential pressure between the capsule and the outside environment, and when that differential pressure got to about six pounds per square inch, that valve sealed off, and then that's the pressure that was maintained in the cabin. As the astronaut breathed down [the suit circuit and as] the cabin [leaked down], these supply valves would resupply oxygen. And then on reentry, then, as the atmosphere pressure increased to a level greater than the cabin pressure of six or five or whatever it was then, then air from the outside would be let in to repressurize the cabin on descent. So that's kind of how that all worked.

ROSS-NAZZAL: You have a pretty good memory of the system. [Laughter] That's pretty good.

Why don't you tell us about some of your basic duties in working with the environmental control system. Were you in charge of certain components, or did you work with the contractors? What were some of those duties that you had?

SAMONSKI: Well, one of the earliest things that I did was to travel to St. Louis for what was called CST or capsule systems tests. This is what was done to the capsule to check it out, to see that it was ready for shipment to the Cape [Canaveral, Florida]. We ran leakage tests on different components and saw that the regulators operated at the right pressure range and the like.

This was not the first test performed on those individual components. There had been what was called an acceptance test or a predelivery acceptance test that AiResearch had performed on them prior to shipment to McDonnell. But this was a kind of a higher-level test to be sure that they still were doing what they were meant to do.

It was pretty crude at first, as a matter of fact. This little demand regulator that I mentioned to you that resupplied the oxygen to the suit circuit, by nature of its design, it had a little controlled bleed of gas, deliberately, like a leak, almost, to maintain pressure within the regulator, [by making up for any internal] case leakage. There was no way to turn that off. So in measuring how tight the rest of the system was, you had to really account for this little bit of a bleed that went out.

The first thing that they presented to us for testing that was a hose that they hooked onto the gas bottle, and it had a gauge on it that was like you would see—and it actually was—on a set of welding regulators, like the welder use, they have these gauges on the tanks. You've seen those? You know what I mean?

ROSS-NAZZAL: I think so.

SAMONSKI: They're about, [2 inches in diameter], this big around [gestures]. Well, this was a gauge that went from zero to 2,000 psi, and the needle was, oh, I don't know, it was a sixteenth of an inch wide or something. Anyway, the width of the needle was about as large as the leakage we were trying to measure in an hour. So it was really pretty crude. We had a little trouble getting with McDonnell in getting that straightened out, to get proper test equipment in order to be able to measure that leakage a little more accurately. We ended up with a nice precision gauge made by a company called Heise that was about probably fifteen inches in diameter or so, so you could really see what the pressure decay was.

So the capsule systems test was one of the first things that I remember that I did. I was the ECS [Environmental Control System] engineer from the Space Task Group on those tests at McDonnell, I remember, for Capsule 5, which was the one that the chimp, Ham, flew in. It was the first animal we put up as NASA. … That was [MR-2 (Mercury Redstone-2 on January 31, 1961]. It was the last flight before Alan [B.] Shepard's flight. Then I was also the test engineer on capsule [7] that was used for Alan Shepard's suborbital Freedom 7 flight. …

Also as a part of the Mercury Program, there was a special ECS test article that McDonnell built, and it had installed [in it] a complete production environmental control system, and the requirement upon McDonnell was that they perform a series of twelve manned tests of up to twenty-eight hours' duration, which was the design requirement of the Mercury capsule system originally. Those tests were conducted at McDonnell in the spring and summer of 1960.

During one of those, we learned almost a painful lesson. The test subject for most of the tests, although not all, was a fellow by the name of Bert [Gilbert B.] North, who was the McDonnell test pilot, and he was the subject. The way the test equipment was arranged, the ECS was installed in a boilerplate test vessel, and Bert North was inside that test vessel, and the hatch was closed. The test vessel itself was in an altitude chamber, and the altitude chamber was pumped down to where it was at a vacuum. But connected to the capsule by hard line, by several hard lines, were things like pressure gauges, so we could see what the pressure is rather than relying upon electronic transducers and the like. Then there was a gas-sampling system, a little pump that took a sample from within the suit circuit and pumped it outside the chamber and through gas analyzers for [analysis of] carbon dioxide and oxygen partial pressure, and then delivered it, [the sample], back [to the suit circuit].

Well, in the first manned test that was run, unbeknownst to anyone at the time, there was a leak in that gas-sampling system, we think. What was happening was, because the plumbing lines that came outside the chamber [were] at a lower pressure than the air around it, ambient air was leaking in. Of course, that's mostly nitrogen. So as Bert North breathed, he was breathing up the oxygen [the makeup that] was being resupplied with mostly nitrogen, and he finally kind of almost went to sleep. He had in there, [the altitude chamber], however, a suit technician watching him. I forget that fellow's name, but anyway, he noted that Bert was kind of drowsy, so called an end to the test. So we learned a lesson there about how insidious the lack of oxygen or hypoxia can be. After that, we cleaned up the procedures and the like, and completed the series of tests right up through all twelve tests.

I was the project engineer on that for the Space Task Group, and then we moved that test vessel and all the supporting equipment to an altitude chamber at the Navy's Air Crew Equipment Laboratory in Philadelphia [Pennsylvania], at the Philadelphia Navy Yard. They had a series of altitude chambers, also, and we set up a similar installation in their altitude chamber, and used that to expose the astronauts to what it was like to be supported by the environmental control system that would be in their flight capsules.

We had, I think, at least five of the astronauts of the original seven, and I think six. I don't have that documented. I don't believe John [H.] Glenn ever went up there for that [training] because of schedule pressures, but I think we took care of the rest of them up there. So that program ran from late 1960 on into January or so, 1961.

Later, when that program was done, we shipped that hardware down, back to Langley, where it lay in storage for a while, until we all moved to Houston, and then we set it up again here [in Houston at the Lane Wells Building] and used it for testing. So that ECS test vessel was another one of the projects that I was involved in [that took a] significant amount of time

I guess the next one would be the flight control duties. Back in the early days of the Space Task Group, the Operations Directorate, under Chuck [Charles W.] Mathews, was not very big, and they drew the flight controller expertise from the engineering divisions. I was in the division that was headed by Max [Maxime A.] Faget, the [Flight Systems] Division. So I was the ECS monitor, and my boss, Dick Johnston, he was an ECS monitor. He was to be the monitor at the Mercury Control Center at the Cape, and I was to be the monitor out at the Bermuda station, which was a backup to the Cape. If something happened to the Cape, Bermuda could take over control. It had its own computer and the like, which made it kind of like a mini Mercury Control Center.

Starting right after we finished up in Philadelphia, I started traveling out to Bermuda for missions and for simulations, which I did probably half a dozen times or more over that next year, went out there for the first orbital flight of the Mercury capsule, the one-orbit flight; I think it was MA-4 [Mercury Atlas-4]. And then the two-orbit flight that had the chimpanzee, Enos.

And then I was supposed to be out there for John Glenn's flight, but when we were preparing for that in, I guess it was either December of '61 or January '62, we scrubbed the launch for some booster reason or something like that, and we came back to Langley. We were still all at Langley yet.

I should say that Dick Johnston never really served as the flight controller at the Mercury Control Center at the Cape. It was another fellow that worked with me, by the name of Mort [Morton] Schler. Dick was just too busy with so many other things. He could never free up the time to be a controller.

So when I got back from Bermuda after that scrubbed flight, I got called in to Dick Johnston's office and got told that for the next go at John Glenn's launch I was going to be at the Mercury Control Center. I don't know, really, what all happened to cause that change to be made, but I was sent down there, and that's where I was as the capsule environment monitor for Glenn's flight and [M. Scott] Carpenter's flight, and [Walter M.] Schirra's flight and [L.] Gordon Cooper's flight.

ROSS-NAZZAL: Sounds like you had a very busy career during Mercury.

SAMONSKI: Yes, and that just gets you to 1963 or so, I guess.

ROSS-NAZZAL: I would like to go back and just ask a few questions on some things that you talked about.

SAMONSKI: Sure.

ROSS-NAZZAL: First, you mentioned that you actually traveled to St. Louis to meet with the McDonnell Aircraft Corporation. Can you tell us a little bit about your relationship with McDonnell and the AiResearch people, how the relationship evolved over time, who you worked with?

SAMONSKI: Yes. Of course, I was fresh out of school. I didn't have very much experience. But there seemed to be a perception on the part of the McDonnell run-of-the-mill guys, not all of them, but many of them, that really NASA was unnecessarily looking over their shoulder, and they could do the job, [without our help]. We even joked that the McDonnell people would say that "If they don't need to know, don't tell them," basically.

But there were some good guys that I worked with. There was a fellow by the name of George Rowe, who was an engineer in St. Louis, and he later moved down to the Cape as part of the McDonnell contingent at the Cape. There were two fellows that worked on this ECS test vessel program when it was at McDonnell, Bryce Keith, and Bill Wright, were both good guys. [I] got along well [with] them. There was a fellow that worked in the capsule systems test, by the name of Emmett Griffith, and he later went to the Cape also and worked. As a matter of fact, I think Emmett transferred to work for North American and worked on the Apollo Program later also down there.

At AiResearch I knew the program manager, a fellow by the name of Ed [Edward H.] Olling, and then a test engineer by the name of Joe Gillerman. He came to the Cape for the early launches, and he assisted me with the post-flight data reduction that we would always do in preparation to prepare the post-launch report.

Let's see. I guess that I ought to tell you about the suborbital flight before Al Shepard's flight, and that was [MR-2] with the chimp, Ham. We had a failure on that flight. In describing the environmental control system earlier, I did not mention to you, but there was a way through snorkel valves to get fresh air into the suit circuit after landing—an inlet snorkel valve and [an] outlet snorkel valve, and the inlet snorkel valve was connected directly to this pressure suit circuit.

Well, the chimp, Ham, of course, didn't wear a pressure suit. He was in a thing called a chimp couch, which was like an enclosed container, and he had some kind of a task thing that he was trained to do during the flight. The hoses were connected to that chimp couch the same way they were to the pressure suit, sort of a supply hose and a return hose.

Well, during launch, this inlet snorkel [valve] vibrated open, and that allowed, through another valve, that allowed all the cabin air to escape. So the cabin depressurized. It's a pretty serious kind of thing. And if not for a little check valve that separated this pressure suit circuit from this [duct that ran] up to the snorkel valve, we'd have lost that chimpanzee.

So we had a hard time figuring out just what the failure was. As it turned out, we determined what happened to the cabin pressure by seeing an instrument panel light come on, indicating that the environmental control system had gone into a post-landing mode. This happened just several seconds into flight, which really wasn't right, so that was a clue, and we started looking around and figured it out.

The fellow, Joe Gillerman, that I mentioned to you, he and I did that data analysis, and in looking at the film, [there was] a camera [in the capsule], it would've been over the astronaut's shoulder, looking at the instrument panel, it showed when that light came on, so we could then isolate the time when [the valve opened] and deduce just what had happened.

ROSS-NAZZAL: What sort of changes did you make after that flight to ensure that wouldn't happen again?

SAMONSKI: What really caused that valve to vibrate open was the fact that there was a mechanical linkage that was supposed to be connected to [it], so that for an astronaut or the person in the capsule, [if] the automatic function didn't work, he could pull a lever and open that valve. Well, because an astronaut was not going to be [on] that flight, for whatever reason that linkage was not connected. But the mass of the linkage, being unsupported, vibrated and caused that valve to open. So if the linkage had been connected, the failure wouldn't have occurred.

Everybody was pretty worried about that failure, and I talked to a lot of managers about it, ensuring them that it wasn't likely to happen on Alan Shepard's flight. When I got down to the Cape—we went down a couple days before the flight to prepare to do the data analysis—the guy who was the chief inspector for NASA—that's mostly contracted work now, but then they were NASA inspectors; it was a fellow by the name of Joe [Joseph M.] Bobik—he took me out to the pad, and we went up the gantry, went up into the capsule probably a day or two before Shepard's flight, and we looked at that valve, and we made sure that that linkage was connected, and we all felt better about that.

ROSS-NAZZAL: So when Al Shepard went up, what were you thinking? What were your thoughts?

SAMONSKI: Well, I was worried, of course, and it all went all right. As a matter of fact, when Al Shepard went up, I was outside of the Mercury Control Center with a pair of binoculars watching the flight. The next morning on the front page of the *Miami Herald*, there's this picture of a group of people standing there looking. There I am on the front row, with binoculars up.

But I felt pretty confident that that wouldn't happen again, because I think we understood what the failure was.

ROSS-NAZZAL: That's great. What a great memory of that mission.

SAMONSKI: Oh, yes.

ROSS-NAZZAL: You had also mentioned that the astronauts had actually tested the ECS before the flights, and I'm wondering if you can talk about the role the astronauts played in helping develop the environmental control system for the Mercury Project.

SAMONSKI: I don't think that they played a very big role. The program went very fast, and they were very busy, and they had a lot of territory to cover. They split up the work between them, like one guy oversaw the Atlas booster, and another one of the astronauts would worry about the Redstone booster. Another one was the guidance system. Another one was the reaction control. Wally Schirra was the environmental control system guy and pressure suits. So I worked pretty close with Wally. But, again, the number of changes that were made as a result of astronaut input in the environmental control system were sort of minimal.

I guess we'll come to the point where we talk about my bet with Wally. The reasons and what happened regarding that was not so much Wally's doing as the engineers' doing. But they kept in close contact with us, and they wanted to know how things were going, and briefings on how things worked. Wally would've been the one that could best explain schematically how the system worked to the other astronauts. I guess he got that knowledge partially from talking with me.

I guess maybe it's a good time to talk about the Wally Schirra thing.

ROSS-NAZZAL: Yes, let's talk about it.

SAMONSKI: Throughout not only Mercury, but on into Apollo and even into the Space Shuttle, one of our larger problems was heat rejection using a heat exchanger and water as the coolant by evaporating water. When water is exposed to a vacuum, it boils at a low temperature. Typically, it—without getting real technical, at about a tenth of a pound per square inch, water boils at about 35 degrees F, which makes it a very nice heat sink. But also, water freezes at 32 degrees. So you've got that very fine division between those two things.

The water that was supplied to the heat exchangers, both the one in the suit circuit, and there was a smaller one in the cabin, was by [way of] two little water control valves, comfort control valves, they were called, CCVs, and they were like little metering valves. They supplied water, and the water flow went drip, drip. I mean, it was really, really slow.

We had really no way to tell how well the flow rate of water was adjusted to the heat load on the heat exchanger. If you supplied too much water, there'd be insufficient heat to carry it away, and the water would freeze. [Also], if you supplied too much water, the pressure wouldn't get down low enough to where the temperature was an effective heat sink. So we might be boiling at 70 or 80 degrees for something, which wouldn't be effective in cooling the [astronaut]. So what we really needed was some way to control the water flow rate, have some intelligence into the control of the water flow rate.

The one measurement that we had was the temperature of the gas going into the pressure suit, but that had too much inertia in it between what you did with the water, and by the time it showed up in cooling the gas, something else was already going on within the heat exchanger. So we devised, based upon test work that we did in the laboratory at the Lane Wells Building after we moved here to Houston, we devised a temperature sensor mounted on the heat exchanger that actually measured the physical surface temperature of the heat exchanger, and so we could tell pretty close what the temperature of the water evaporating on the inside was.

When we hit upon that scheme, we were pretty happy, and both the two flights before Wally Schirra's flight, Glenn and Carpenter, both of those guys had some difficulty in keeping the temperature down to where they would prefer it.

So I assured Wally that we had that problem fixed, basically. He said, "You want to bet?" kind of, and so we bet fifty cents on that. We had a specific thing. I told him, I think, that the air out of the cabin heat exchanger would be down below 50 degrees or something like that. As it turned out, I was right, and Wally got a kick out of that. As a matter of fact, he said my name during that mission three times, saying that, "I guess Frank Samonski knows by now that I owe him fifty cents." After the flight, he had some guy go and get a fifty-cent piece, gold-plated, and he gave me that gold-plated fifty-cent piece, which I still have, of course.

ROSS-NAZZAL: Oh, that's wonderful. That's great.

SAMONSKI: Okay. What's next?

ROSS-NAZZAL: Why don't you talk a little bit about your work with Wally Schirra, since you mentioned that he worked as the ECS astronaut contact.

SAMONSKI: I can't remember much in the way of specifics. I do recall that when we had the test vessel up at ACEL [Air Crew Equipment Laboratory] in Philadelphia, and when Wally was up there as a test subject, somebody was making a film for NASA of what all was going on, and they sent a little team up to film what we were doing at ACEL. The guy that was sort of the director, he made up these little prompt cards that he was going to hold up behind the camera for Wally Schirra, and it was so funny. The first one was, "I am Wally Schirra." [Laughs] We got a big kick out of that. So I remember we got a photograph taken of Wally holding that card, and he signed it for me.

But I can't recall the specifics of the day-to-day stuff. For a while, I was in the same building with Wally and the other astronauts there at Langley, and I saw him on a daily basis, really, and it was just a casual working relationship. You know, he'd ask me, "What about that qualification test failure that we heard about?" or something, and I'd tell him. And that kind of a thing went on beyond Mercury and on into Apollo, when different guys would be assigned.

Bill [William A.] Anders was assigned as the ECS guy for a while in Apollo. And Ken [Thomas K.] Mattingly [II] was after him, and the like, but I guess we'll talk about Apollo later on.

Let me see if I've got anything down here that I meant to mention about Mercury. I made a few notes. [Samonski refers to notes.] Coming back to the sponge separator I told you about, that was kind of like a Rube Goldberg. If you can imagine, the heat exchanger's a little block of material that's got fins in it, very fine fins, and they're layered in passages, and between the passages are passages where the steam is evaporating and cooling. In the passages where the gas and the pressure suit is moving, moisture, humidity, condenses out and sort of wets those fins. And the idea was, the theory was that the gas being moved around by this suit compressor, this fan, would tear those droplets loose off of these wet surfaces, and carry them, entrained in the air stream into a sponge. Then this sponge was by a timer, periodically, like every thirty minutes, I believe, was squeezed, and would take out the water and push it through a check valve and down into a condensate storage tank, which all sounded pretty good and the like, except it turned out it didn't work that way.

We found out that by testing the heat exchanger inverted, where gravity was helping the droplets, even then, there was not sufficient velocity produced by the fan flow to tear those droplets loose. Of course, gravity is a fairly strong force. And in a weightless environment, without that presence, there was no way that the velocity imparted by the air from the fan could break the surface tension that was holding those droplets to the fins in the heat exchanger. So what the water was doing was really just running around the surface of the enclosure this sponge was in, and just going down the duct.

Liquids behave very strangely in a weightless environment. I don't know if you've ever seen movies of the Shuttle crews and the like when they throw globs of water around. It's really not an easy thing to have liquids go where you want them and to stay where you want them.

So we devised a thing called a condensate trap, that didn't fly until the last Mercury flight, Gordon Cooper's flight. This was a little cylinder that mounted in the hose that connected to the pressure suit, right over here in the waist [gestures], just right before it went into the suit. Inside this cylinder, we had like several wraps of a wicking material called Refrasil and then a plenum around that with a hose leading out. The water [bypassed] the sponge [would] just travel down the ductwork, and the hoses coming up to the suit. [There the water] would get caught in this wicking material, and because the pressure within the suit circuit was greater than where we were collecting it, it would come weeping out. And so Gordon Cooper had a little clamp there that he would open periodically, and he could watch the water come out of there, which was just proof positive that it wasn't going into the sponge separator; it was coming right down the duct [and] into the suit. So that was an interesting thing.

You asked me about this separator that [Robert E.] Smylie and a fellow by the name of Frank Collier and I have a patent on. That was to be mounted [at] the outlet of where this sponge plunger would shove the [water]. It would separate there. Well, as it turned out, we never flew that. We tested it and demonstrated it, but what we flew, because it was easier to integrate into the spacecraft, we flew the condensate trap, but we never applied for a patent on it. The patent is on a little more sophisticated device, but using same principle.

ROSS-NAZZAL: Interesting. Well, you talked a little bit about your job at the Mercury Control Center and at the Bermuda site. Could you tell us a about your job as a flight controller during that time? What were your duties during flight?

SAMONSKI: You know, before we start all that simulation stuff [and] flights and the like, you have a whole bunch of meetings where you develop a set of mission rules, and you try to think of just about everything that could happen that could go wrong, as unlikely or as improbable as the things might be, and decide upon what would be the most prudent course of action. Of course, you try and put some limits on where you have numbers. Like, if you're talking about a pressure, you know that a man can't live with the pressure that's much below a 5 psia [pounds per square inch absolute]. So if the cabin pressure gets below 5 psia and [if] the suit pressure (in Mercury [capsule]) looked like it was following it, [the cabin pressure], closely, then you'd better do an abort.

That's one of the more severe cases. But for everything, temperatures and pressures and expendable quantities and the like, we tried to develop these scenarios. The flight controller people today, still play the same—they call it the "what if" game. What if something happens? The logical consequences of what could go wrong.

So we worked quite a while in developing the mission rules, and then we just pretty much played by the book, basically. Before a mission, the flight control team would be deployed. Like for an orbital mission, it would be guys [going to] places all around the world. There were some fifteen or eighteen tracking stations [manned] around the world, and we'd do mission simulations. And there was a special group of guys that did the simulation. They would think up a scenario of what would be failures that might happen and the like, and they would inject them. They'd have a way to inject them into whatever was displaying the [data] on your console. They could have a way to make the cabin pressure look like it was falling down and down and down and down, and then you had to react to that.

We had basically two kinds of simulations. We had a lot of launch-phase simulations, which just involved us at the Control Center at the Cape and Bermuda, because by the time the spacecraft got near the end of Bermuda's range, it was in orbit already, so you really couldn't do much about it. So these were kind of launch-abort scenarios. Everything that could go wrong in the time-critical launch phase, we'd practice on. Then they had other things that were orbital simulations, trends that might develop over a longer period of time that you really might not deduce so much. You [would be] plotting the data hour by hour and [would] see which things were going to pot.

So that's kind of how it was. We did simulations and then you did the mission. The mission often seemed like a bland simulation, although sometimes we had problems.

ROSS-NAZZAL: Are there any missions that stand out in your mind where you had to deal with a number of issues?

SAMONSKI: Probably the most challenging problems that I had were in Wally Schirra's flight with temperature control, but we sort of worked that out. We violated a mission rule about the suit inlet temperature, because Wally couldn't get the right setting, not through any fault of his. As it turned out, there was an obstruction in that little comfort control valve that prevented him from getting as much water as he thought he was getting, even though it was a dribble kind of thing. But we finally got that under control. Kind of the way that went was that when we got to the red line on the suit inlet temperature, the flight director, Chris [Christopher C.] Kraft, spoke to the flight surgeon, [Dr. Charles A. Berry], and asked him, he said, basically, "What is Wally's physical condition? Do we have [time]?" Of course, he, [Dr. Barry], looked at the bioinstrumentation he [had], the deep body temperature, and could deduce physiologically what kind of condition Wally was in and whether we could kind of tough it out a little longer, because the mission rules weren't absolutely inviolate. They were really meant to be a guide.

So as it turned out, we got that straightened out. I guess Wally, in closing the valve and opening it back up—and this is mere speculation—probably dislodged whatever was there, and it got it under control, and we had a happy ending. [He] referred to his flight as a textbook kind of flight, and it pretty much was.

Some of the other missions were really—there were some scary things, like John Glenn's flight, when we thought the landing bag had deployed and the like. That was scary. I'm sure other people have told you about that.

ROSS-NAZZAL: Why don't we switch gears for a minute and talk about moving to Houston.

SAMONSKI: Okay.

ROSS-NAZZAL: Why don't you tell us what your impressions were of Houston at the time when you moved here from Virginia.

SAMONSKI: Okay. First of all, they had an arrangement set up where they would bring husbands and wives here on sort of a scouting mission to look for a place to live. The government provided that transportation and paid some per diem and motel and the like. For my wife, Joanie, and I, we came in early January of 1962. We owned a house in Newport News, Virginia, which we had just bought a year or so before, before we learned we were moving to Houston.

We came down with a planeload of people on a charter flight that the Space Task Group had arranged for, and we arrived at Hobby Field [Houston, Texas]. It was late at night, and they somehow—I forget, taxi, or limousine, or bus—not bus. Somehow we got to a motel on Telephone Road. I think it was the Sky Lane Motel. I don't know whether it's there or not, but

Joanie and I talked about the name of it. It was Stardust or Starlight, but I think it was Sky Lane. Anyway, that's where we all stayed.

The next morning, I'll tell you, that was really something to see. That was really honky-tonk. I mean, there were strip joints and bars, and really a seedy part of town. We figured, "Is this what we're coming to?"

So we had a couple of days. It was over a weekend. We had a couple of days to look for a place to live. I forget where all we looked. We looked at Fairmount Park, and we looked at a place that's now like Nassau Bay. There was a little subdivision there called Swan Lagoon, and maybe a couple—we looked someplace on the Gulf Freeway, too, Sun Meadow or something. Then we went out and looked at El Lago and Timber Cove, and we really liked the looks of El Lago. They had the concrete streets and curbs and sidewalks. It really looked like a neat subdivision, gas lamps and all.

Also there was a fellow there that was one of the builders, were active builders in El Lago was an outfit called Traditional Homes, and the builder's name was Jim Blackstone, and he was a nice fellow. We talked with him about how much it would cost us to get into El Lago, which we felt was one of the better subdivisions and the like. It seemed like a lot of money in those days, but we signed a contract, and wrote an earnest-money check for him to build us a home on Bayou View Drive in El Lago, Section One. And went back to Virginia to worry about putting our house on the market and selling it. But that all worked out okay.

The movers came while I was at the Cape for Scott Carpenter's flight in late May, and so my wife had to look after the movers taking care of everything and the like. When I got back to Virginia after the mission and the post-launch report and the like, there was no house to go back to. You know, we went to a motel, and we then almost right away started our trip down to Texas.

ROSS-NAZZAL: Could you compare working at the Manned Spacecraft Center with working at Langley?

SAMONSKI: First, let me say that when we arrived here, of course, there was no Manned Spacecraft Center. It was just a prairie, and it stayed that way for it seems like the better part of a year, because we drove by it. Going from El Lago, we drive up what is now NASA Road 1, was then a two-lane humpbacked gravel road that was Farm-to-Market 528. You'd see nothing out there where the Center is now, nothing at all. Because I think for the first several many months, I guess, they were working on the underground utility tunnels. But then once they started to put the steel work up, then things went pretty quick. But for a while, we didn't think there was ever going to be a Manned Spacecraft Center.

Our division was in the Lane Wells Building on Wayside Drive, and my wife worked for the Space Task Group also. She worked first in the Mercury Project office in support of a guy by the name of Jerome B. Hammack, who was kind of the lead guy on the Redstone Program, and there was a comparable guy for the Atlas. Then as Mercury wound down, she went to work for the Gemini Project office, in a secretarial job, executive kind of secretary.

Now, compare working at the Manned Spacecraft Center then in the early days, in the temporary buildings with the Space Task Group. Well, we were all sort of in one place back at Langley, but when we got here to Houston, everybody was spread out. I forget how many different temporary sites there were. There was probably ten or more. So a lot of the people that you had worked with, you didn't see very often anymore. You kind of just worked within your own division.

Langley was nice on the west side of the field, all those wind tunnels and facilities and the like. It was almost like a campus kind of atmosphere. It was sort of different.

ROSS-NAZZAL: Was there a different culture out here in Houston compared with Langley?

SAMONSKI: Well, it seems that we were growing so fast as an organization that there were reorganizations that were taking place all the time, and more and more new people, and more and more regulations, and almost from the time we got here to Houston, it was more like big business. Things were pretty loose back at Langley, kind of minimal supervision and multiple jobs to do. But it got more structured, I guess, as we got here in Houston.

ROSS-NAZZAL: We've talked about your position with the Mercury Project, and I'd like to start talking about the Apollo Program.

SAMONSKI: Okay.

ROSS-NAZZAL: How did you make that transition? Did you actually start working on Apollo while you were working on Mercury?

SAMONSKI: A little bit. I was very interested. I was inspired by [President John F.] Kennedy's speech in May of '61 about the man on the Moon, and I was anxious to get to work on the Apollo Program. I really wanted to be a part of that, really deeply wanted to be a part of that. But I was pretty well tied up in Mercury, and I really couldn't get free until after the last Mercury flight, Gordon Cooper, MA-9 in May of '63.

I did, however, serve on the proposal evaluation team that reviewed the proposals for the Apollo spacecraft that eventually was awarded to North American Aviation Space and Information Systems Division. That was done at Langley, of course, not on the field. We did that at a hotel that maybe is probably still there, right on the water. I think it's the Chamberlain Hotel at Old Point Comfort. We just took up a couple of floors of that, and that's where we did the proposal evaluation. So although I was still heavily, completely involved in the Mercury Program, I knew what the proposed Apollo spacecraft was going to look like, and something about the systems and the like, and what was proposed for the environmental control system and how it was going to be different than what was in Mercury.

Then in probably December of 1963 or so, there was an agreement made, as I understand it, between the Engineering Directorate, under Faget, and the Apollo Spacecraft Program Office, [that] was then headed by Dr. Joe [Joseph F.] Shea, who had not yet reported on duty to Houston from [NASA] Headquarters [Washington, D.C.], where he had been working. That agreement was that the Engineering [Directorate] was going to provide technical support to the program office and [in] the various technical disciplines. They came up with a thing called the Subsystem Manager Agreement, and this was a little document that was prepared that said Frank Samonski was responsible for the command and service module environmental control system, and then there was attached to that three or four pages of what all my duties and responsibilities were, cost and schedule. Those were new things for Apollo. They always give you responsibilities. They never say what your authority is, just responsibilities.

I was probably one of the first subsystem managers. They're still using that management technique now for the Shuttle Program, but that was a new thing for Apollo, and it was signed by Bob [Robert O.] Piland, who was the acting project manager for the Apollo Program office until Joe Shea came down, and Max Faget.

We just started in late December of '63 or January of '64, we started on the Apollo, and it just took all my time for the next five years or so.

ROSS-NAZZAL: Why don't you tell us how the environmental control system for the Apollo Program differed from the Mercury Program.

SAMONSKI: Okay. If you were to draw a schematic, kind of like a block diagram and the like, you could draw them so they look much the same. I mean, there are still the same jobs to do. You have to remove carbon dioxide, you have to resupply oxygen, you have to remove heat and humidity. But in details, the Apollo system was much more complex. It had many more components. For one thing, most of the heat rejection was done by cold plates that were slabs of metal that the black boxes, the electronics, would sit upon, and then a coolant would circulate through the passages in these plates, and they'd be arranged in a plumbing fashion in series and parallel groupings. That's how the heat would be collected.

Then the principal mode of heat rejection, whereas in Mercury it was a water boiler or two water boilers, for Apollo it was a space radiator, and this is a thing that's on the skin of the spacecraft that's basically like tubes, and when you point that at something in deep space, it looks very cold. So heat radiates from the surface of this radiator, and it cools the fluid that's passing through it, in the tubes and the like, and that's how you get rid of the heat. Sometimes it cools it to colder than you want it, because of the attitude the spacecraft's in, or because the peak load that you're operating at is not large, but for whatever reason. Then in that case, when the return temperature from the radiator is colder than what you would prefer, which is normally like 45 degrees Fahrenheit, then you bypass some of the fluid around the radiator, some of the hot fluid around the radiator, and mix it to get the temperature at the 45 degree temperature you want.

So principal heat rejection is done by radiator, but it's dependent upon attitude and also the environment you're in, like radiators probably don't work too good when you're in Earth orbit, because a good bit of them sees the Earth, and the Earth's effective temperature is much warmer than deep space. Like going to the Moon, though, translunar from Earth to the Moon, and the spacecraft is in a barbecue mode, it's rotated and the like to even out the effects of heat lost to space, radiators work very well. So that's one big difference, [radiators were] the principal heat rejection, [and the] collection of heat was done by a fluid pump.

And then the fluid that we used in the Apollo command service module was a mixture of water and ethylene glycol, the same kind of stuff that you have in the radiator in your car, in your cooling system, [water]-glycol. But it was a much higher percentage [of glycol] so that the freezing point would be much, much lower.

And because that heat collection is such a critical function, and all the electronics depend upon it and the like, we had two coolant loops, a primary and a secondary coolant loop, and they were so designed that if one should fail, you could safely complete the mission with the other.

So that was one big change. Of course, that adds a lot of complexity, a lot of components. I don't remember, if you were to count up components in the Mercury system, how many there were. I'm going to guess. I'm going to say maybe thirty-five or forty. I brought schematics. I could look and count them if we wanted. Maybe thirty-five components or so.

In preparing for this interview, I went back and counted. Just the equipment that AiResearch provided, there were eighty-eight components, and to that, of course, North American Aviation added, like all the cold plates were made by North American. I guess they're called Rockwell now. I'm not sure what. But anyway, North American, the radiators were made by North American. Most of the plumbing was made by North American, so there was just a whole lot more complexity, a whole lot more complex.

Another uniqueness about the Apollo system as compared to Mercury was that there was a requirement that the system be able to provide life support for the crew members in their pressure suits in the event there was a problem in the vicinity of the Moon. So the system had to work to provide life support all the way back from the Moon with the crew in pressure suits. That sounds all right, except that the tricky part was changing the lithium hydroxide cartridges. In Mercury, there was just one cartridge for the whole mission. In Apollo, we had to change cartridges. Each one was sized for, I think it was, one and a half man days.

So with three crew members, you went through two cartridges every twenty-four hours, and they changed them alternately. There was an A and a B, and they changed them alternately on twelve-hour intervals. But the thing that held the little cartridges of the chemical had to be such that you could open that up with the cabin at zero pressure and change the canisters. There had been to isolation valves and all that sort of stuff. It was a complex mechanism, kind of.

We tried to learn from our problems with the water boiler in Mercury. So [in] the Apollo command module, [the] environmental control system [had a] water boiler, sometimes they called it a glycol evaporator. It didn't evaporate glycol, but the coolant fluid was glycol, water-glycol, as I mentioned. It had a very sophisticated control system. Without getting into a lot of detail, but just a little bit so you can understand maybe some of the things I'll mention later, it was a device where the back pressure or the pressure within the evaporator, the thing that kind of determines at what temperature the water boils—remember, I said it was a function of the pressure—it was positively controlled. There was like a rubber boot valve that could squeeze and open in the duct leading to space vacuum. So you could basically shut off the water boiler. There wouldn't be any way for the vacuum to see the water boiler. Then by opening up that valve, then you allowed the pressure to decrease. By adjusting the position of that valve versus how much water was being boiled, you could actively control the pressure at which it was boiling, which was really a good idea.

And that part of it worked okay, but then we found the problem of controlling the water flow, and now we didn't have a crew member turning a water valve or something; we had automatic control for the water. That turned out to be a problem, because we had a difficult time determining where do you put a sensor in the water boiler to tell you sort of in the aggregate [how wet] the boiler is. If you put it one place, that's not necessarily representative of the rest of the boiler. So we had a fair amount of difficulty arriving at how and what kind of a sensor and where to control the wetness, and it ended up being called a wetness sensor. So the boiler, in itself, was a complex affair.

Then as luck would have it, this electronic controller that controlled these functions, the back pressure and the wetness, and when the thing turned on and turned off, AiResearch maybe didn't do such a great job in designing this black box, because they really weren't in that business, like some other companies were, in electronics and the like. So we had a lot of trouble with that. It was called the two-forty control, or 2.40. That was its, [schematic], item number, and sometimes you'll hear references to the two-forty controller.

As it turned out, after the Apollo fire, NASA management directed AiResearch to go to Honeywell to have that controller built. So for the manned lunar missions, that controller was built by Honeywell, Minneapolis Honeywell.

The radiator, the design of the radiator itself, you might think you just take this panel of metal and maybe braze on a bunch of tubes and flow the fluid through it, and that would do it, but it's not that easy. First of all, maybe one side of the spacecraft is exposed to deep space, and it's radiating heat away like crazy. But the other side of the spacecraft, where you have the other radiator, is maybe seeing the Earth, and it's not radiating away so much. So the side that's seeing deep space, the fluid within the radiator, because it's getting colder, it becomes more viscous, and the flow rate sort of slows down. The resistance of the passage on that radiator is higher, so more flow is diverted to [the Earth-side] one, where you don't want it, because it's not in a good environment for rejecting heat.

So we had to find a way to, first of all, balance the flow to the two panels on opposite sides of the spacecraft that would compensate for the environment that they were seeing. Then we had to figure out a way to, if the fluid got very cold and viscous within a particular radiator, how is it that you recover from that, that the flow doesn't stagnate, and then you can never get heat out there in order to thaw it to get it to go again?

So that was a pretty tricky job, and what we ended up with was a technique called selective stagnation. We had, I forget, maybe five or six parallel tubes in that radiator, and there was a manifold that supplied the fluid to those tubes, and one that collected it. By adjusting the restrictions in the manifolds for each tube, you could predict with some certainty what the temperature range would be in each of the tubes. So, the one furthest from the manifold would be the coldest, so you would want it to have the least pressure drop. Then come in one, and it would be a little more resistance maybe. So within certain ranges, the radiator could kind of stagnate and then recover itself. That took some testing, sophisticated testing, in our big chambers here, Chamber A, the space environment simulator, and the like, to get that all correct.

ROSS-NAZZAL: You mentioned that you were the first subsystem manager, or one of the first subsystem managers. Can you talk about your work with the contractor, with North American, with AiResearch?

SAMONSKI: Yes, I think so. Yes. It was a much bigger operation on Apollo than it was on Mercury. There were a lot more people involved. I think when I started working as a subsystem manager, there were probably 200 people at North American working on the environmental control system, different parts of it, and I don't know how many at AiResearch. We generally had good working relationships with both North American and with AiResearch. I was probably guilty of maybe violating management communication lines and contacting people from AiResearch when North American would have preferred me to kind of work through them, but as it turned out, they didn't always have the information that I needed or felt I needed to have. So I felt it was my job to find out, and so I tried to get a hold of the person that would have the best answer.

There was a lot of that that went on with the problems that we had with the water boiler, this glycol evaporator thing. We had lots and lots of meetings. As a matter of fact, when we got all the problems solved and the like, and it was behind us, which was pretty well into the program—I guess it was probably Apollo 11 or so before we had all those problems ironed out—all of us that were involved at North American and here and at AiResearch, we sort of formed a little thing we called the Boiler Debating Society, and we had a little certificate made up, and the guy at AiResearch, who was the chief engineer, Karl Jackson, he had some pins made up that were the letter *Q* for heat, it's the engineering term for heat, and behind it was like three Doric columns, like the Greek column, like for "debate" or something. There was probably twenty-five members of the Boiler Debating Society.

But we felt that with the subsystem manager agreement thing that we spent some time looking at costs and schedule and weights. There was more responsibility, and it turned out there was some authority, too. I mean, on more than one occasion I, being out there for meetings and problem-solving and the like, that I would call back here to the ranch and speak to the guy who was the project officer for the program office, that most of the time was a fellow by the name of Clint Taylor. He was the guy that really authorized that you could make a change. If I wanted to direct the contractor to make a change, I really needed his nod. If he said okay, then that sort of obligated the government for whatever it is that we said we were going to do, and we had to resort to that sometimes. And the way that worked was, you'd go ahead and you'd direct the change, and you'd make the change, and then sometime well in the future, the contractor comes back with the bill and, of course, you've got to pay the bill. But the schedule pressures and the like make you want to make changes at a particular time.

Early on, there was a thing called PERT. It was a computerized scheduling technique, stood for Performance Evaluation and Review Technique. What they did was, the contractor—and the subcontractors like AiResearch made input too—prepared this great big flowchart of events connected by lines of performance times so you could track. You know you had to do this, and it took three weeks, and then you had to do this, and that took five months, and then you had to do this, and then on something else you had to do this. So you had this complex network of bubbles of events connected by intervals of time to do things. This was all computerized so automatically you could find out what is the critical path in the program. What is the thing that is going to take the longest, the way we've got it? And that was the way for management to focus on where the trouble spots were.

So we spent a fair amount of time early, as the subsystem manager, kind of getting that PERT network to where it was representative of the program, that it had enough detail in to give you some management visibility, but that it was not so much detail, [not so] cumbersome that you got lost in the detail.

The cost stuff, AiResearch produced a very comprehensive monthly cost report, I mean more detail that you'd really care to look at, and we took that into account. Then North American produced a monthly 533 contractor cost report that was at a little bit of a higher level. But because of the depth of the AiResearch report, you could gain some insight into the North American report.

Can't think of anything else.

ROSS-NAZZAL: Well, you mentioned you moved into this management position, essentially. Did you have any duties as a flight controller in the Apollo Program?

SAMONSKI: None. None. By that time, the Operations [Directorate] had all their own people and they did all their own flight control.

ROSS-NAZZAL: Because our research had suggested that you might have been at Mission Control when the Apollo 1 fire occurred.

SAMONSKI: No, I was not. I noticed that in your list of questions. Actually, I was in my office in Building 7-A. It was late on a Friday afternoon. I knew we were running the so-called plugs out-test at the Cape. The guy that I frequently talked with at the Cape was the lead engineer for the North American guys down there, a fellow by the name of Sam Moody. I was talking to Sam, and I forget what we were talking about. It was not about the test that was going on, but we both knew the test were going. We were talking about something else. And he said, "Just a minute, Frank," [or] something. He went off someplace else, and he came back on, and he said, "Hey, we just had a problem out there at the pad." He says, "I've got to go." I think he may have told me that it was serious. He might have said it was a fire or something, but I knew we had a problem.

I was there pretty late that evening when we made that phone call. I think it was about five-thirty or quarter to six or so, local time, and I think the fire occurred about six-thirty-something Cape time. So I was on the phone with Sam when that happened. By the time I finished up what I was doing and got home, it was already on the news, and my wife told me that the three astronauts were killed in the fire.

Gus [Virgil I.] Grissom, I was probably the closest to him personally of all the original seven astronauts, although I worked with Wally Schirra quite some and spent a lot of time with Al Shepard in Bermuda and other places. Those are probably the three guys I knew, but Gus I knew the best. I visited Gus' home with my family.

So when I got home, I wasn't home long before I got a phone call that said that they wanted me to pack a suitcase and wanted me to go to the Cape that night. So I flew to the Cape that night on the NASA Gulfstream with Bob [Robert R.] Gilruth and my boss, Dick Johnston, and I forget who else. Seems to me that Joe Shea was on that airplane, too, but [he] had been at the Cape that afternoon, and I think that he had just gotten back to Houston when that had happened, and he turned around. But there were, as you might guess, a lot of very sad faces on that airplane that evening. Of course, we didn't have any real detail. We didn't know anything.

When we got to the Cape, it was probably one o'clock in the morning or something, and they put us up in the astronaut quarters at the Cape. That was a Friday night, and sometime early the next morning—they had impounded all the data, I guess, whoever it is that was in charge, and rightly so, had impounded everything till they could sort out what was going on. Then they released the data. So they led us into this room where there was all these records from strip chart recorders stuck up on the wall that showed all the data that was collected at the time of the incident. Then we started in on the evaluation, trying to figure out what happened. I was down at the Cape then for several days, and went back to Houston, and then came back down to the Cape. For the next probably couple of months, I spent a lot of time at the Cape trying to sort all that out.

Let's see. In your questions you mentioned that AiResearch and North American were sort of criticized about the environmental control system. You know, as far as I know, they never knew the exact cause of the fire, specific cause of the fire. Best thinking was that it was some kind of a short, that maybe someone stepped on a wire. I think the workmanship, as far as covering plumbing runs and most especially wire bundles and the like, could've been much better in the Apollo spacecraft. And workmen going in and out and doing things and all that, I imagine it's probably a situation where a wire or something got damaged, and we were in that oxygen atmosphere. You know, it's funny how those things happen. I don't really know where to begin with that [from] a historical perspective.

But the cabin was pressurized with oxygen at five pounds pressure above atmosphere. So it was 19.7 pounds per square inch of oxygen, pure oxygen. Almost anything will burn in that environment and burn vigorously. How it is we got to that situation where we did that as a procedural thing, I'll never know. The concern was that you have a need for the crew to de-nitrogenate before they go EVA [Extravehicular Activity], and I think there was concern that if the crew had to get out of the spacecraft in the space suit or something, shortly after launch—I don't know what kind of an emergency that would be—but if they had to do something like that, then they still have nitrogen in their body, and they would have experienced the bends, so the way to get around that, we thought, was to make it all pure oxygen, never once thinking that there might be a fire.

And then to compound it, then we did the hatch design so that the hatch opened inward, that is, so the pressure sealed the hatch to make it leak tight, right? That's the way an engineer would do it. Well, except when you've got five pounds pressure greater on the inside, then you can't get open the hatch until you bleed off that pressure.

So a number of things kind of piled up, really, that was the cause for that fire. Materials, control of nonmetallic materials, there virtually wasn't very much control, and there was just too many flammable things in the spacecraft, and that's another thing we fixed after the fire.

Some of the people who worked on the program, some of the higher-level managers, Chris Kraft, I think, was one. I've always respected Chris very highly. I think he said that if we hadn't had the fire, we probably would have never made it to the Moon. It was just a lesson that kind of had to be learned, and it was maybe just fortunate that we learned it early, and we had [learned] it on the ground.

All these components that make up the environmental control system in Apollo, because they all have to be tested, and when you say "testing," there's a process that they call certification. That's a program of testing that tries to cover all the bases as to what environments and the like components would be exposed to. So if there's eighty-eight components at AiResearch, each one of those components by itself had to be exposed to a test on a vibration table, had to be exposed to a test on a centrifuge for acceleration, exposed to a high-temperature test, low-temperature test, had to be exposed to an EMI test, electromagnetic interference, to see that no radio frequency waves would disturb its function. There was insulation-resistance tests to be sure that the thing is put together right and there's not some kind of a short-circuit path between the pins and connectors and the like.

… We had for a while oxygen and humidity tests where you'd put it in the chamber and raise the concentration of oxygen and then humidity to see if that would have an effect on the component. There's probably some others that I've forgotten, but each component had to receive that kind of a series of tests, and it had to be checked to see that it still performed [per] specification after [each] test.

Then they were assembled into functional packages the way they would be in the spacecraft, and they were tested at that level. Then they were assembled into a complete system and put it into a chamber, unmanned, and tested for a simulated mission cycle, 500 hours. Then they were disassembled, and they were subjected to post-landing shock tests to see that they didn't break apart after landing. So there was a whole lot of testing.

Well, just because of the sheer number of components and the number of tests, we had a lot of failures at AiResearch, and program management counts failures. So the AiResearch hardware was looked at as a trouble spot. But in retrospect, I don't believe it was so much a trouble spot as just a result of a numbers game. Each one, you forget sometimes, that each one is a unique, new design, and any product that you start making, when you make it the first time, there's going to be something that's not right, and you're going to have to modify it and the like, and that's really what we saw.

I don't believe that the environmental control system, from anything that I saw, was a direct cause for the fire. I don't believe anybody else thinks that. I think, because it carried oxygen and the like, that in the course of the conflagration, after the fire started, lines melted. This cold plate network that I describe was put together with aluminum tubing and soldered joints, and in that hot fire that occurred, some of those joints let go, and that glycol spilled out into the cabin during the course of the fire, and the oxygen lines that carried the high-pressure oxygen, some of those fittings melted, and more oxygen squirted in and the like, but those are all after the fact.

Enough on that one.

ROSS-NAZZAL: I think this would be a good time for us to change the tape, though.

[Tape change.]

ROSS-NAZZAL: Okay. We'll start back up again. When we last turned off the recorder, you were talking about the Apollo 1 fire. Could you talk to us about how the Apollo 1 fire changed the environmental control system, if at all?

SAMONSKI: Oh, sure. Mostly, I think, for us it was materials changes, like for a lot of other people in the spacecraft, other things in the spacecraft. More in particular, I was mentioning the cold plates were all aluminum, and the plumbing that connected all of them, and there's a lot of that. I mean, there was probably thirty-some cold plates in the command module. Remember, there were two coolant loops, and that [number] was kind of doubled. They were all aluminum, and the plumbing between them was aluminum, and they used what we thought was a very weight-effective technique for joining them, [it] was like a little solder sleeve. Well, the fire showed us that that kind of joining was not robust. So we came up with a thing called armoring. We made like a two-[part] sleeve that would fit over each of those little solder unions, and it would get smeared with an epoxy, and it was much longer than the joint, and we had a little tool. So every one of those unions would get clamped down with a heavier tube of aluminum that would be cemented to them, which made them more rigid.

We had probably between two and three hundred joints in the cold plate network like that, I believe, and every one of those got armored. I think every one got armored. Some of them were hard to get to, and there were meetings and arguments about whether we could armor this and one and that. But I think essentially all of them got armored.

Now, I should mention that most of the oxygen that we used in the command module for life support was stored in the service module in the tanks that provided the reactant for the fuel cells. As a matter of fact, it was one of those tanks that exploded during Apollo 13, one of those cryogenic tanks. So we had joint storage. So most of the oxygen in those tanks were for the fuel cells, but they were sized to have a certain amount for the environmental control system's needs. Through plumbing, that, [the oxygen], was brought into the command module through the pressure bulkhead from the service module, and that was like 900 psi, and the lines within the command module that ran to the AiResearch hardware where the valves were and all that, that was all aluminum before the fire. So, after the fire, that got changed to stainless steel. Those lines were all changed to stainless because it was felt it was more robust.

Likewise, the plumbing between the different valves and the like, anything over 20 psi was made out of stainless. We had [the supply] pressures at 900 [psi], then the regulated pressure was 100 psi, and then the pressure that was supplied to the water tanks was 20 psi. All those lines were changed to stainless steel.

The insulation materials on the heat exchangers and the like, before the fire, that was a polyurethane, like open cell, and it would just become saturated with oxygen so it would really burn bad. We did away with that and went to a Mylar-[covered] fiberglass. It's called TG-15000, high-efficiency insulation, kind of a wrap that basically doesn't burn.

We looked at every component that had nonmetallics in it, and made a separate assessment of the amount of material there by weight, grams or tenths of grams, and the amount of surface area exposed to oxygen and an assessment of just what the relative risks were and the like. You cannot do without nonmetallic materials. You know, you just have to have things made of rubber and silicon and polymers and the like, but you can look at them and see if you have any bad actors.

One of the things we changed was the high-pressure regulators that regulated this pressure from the cryotanks from 900 down to the 100 psi that got distributed within the system. We changed from a regulator that had a nonmetallic material as the seat, to a regulator that had a silver seat, [a] metal-to-metal seat.

That's kind of the highlights of the changes that got made in hardware.

We also went to a thing called air on the pad. We did away with purging the cabin with 100 percent oxygen and pressurizing it up 5 psi so it was 19.7 psi pure oxygen. We went to a mixture of oxygen and nitrogen, like an enriched air on the pad. We [added] a little pressure transducer that allowed the crew members to maintain the pressure in their suit circuits slightly above the cabin pressure so there was no in-leakage from the cabin. Their suit circuit was like at 100 percent oxygen, but the cabin around them was an enriched air: I guess they just bought into the physiological risk. If they have to do an EVA or something, that there might be an incidence of the bends.

Then after launch, we bled pure oxygen into the cabin so that over time the oxygen concentration increased and got enriched, so that by the time the command module was undocked and mated with a lunar module en route to the Moon, the cabin was up at, I forget what, some 90 percent oxygen or something.

ROSS-NAZZAL: You actually wrote a history of the Apollo environmental control system.

SAMONSKI: Wrote one of the Mercury one, too. More extensive, yes.

ROSS-NAZZAL: Yes. You noted some problems with things that you had talked about already, the cold plates, the evaporators, the radiators, the water system, and the waste management system. I'm wondering if you could talk not technically about them, but perhaps some of the problems that were encountered by the crews in flight.

SAMONSKI: Well, the crew didn't encounter problems with the cold plates in flight or the radiators. We had some problems with the water boiler temperature control in the earlier Apollo flights. They would tend to dry out, was a frequent problem. There would not be sufficient moisture there, and the boiler, for one reason or another, would just kind of shut down, and the crew would have to manually add water to the boiler and restart it. That happened on a couple of flights, I think, before Apollo 11. As I said, I think by Apollo 11 we had the water boiler problems pretty well squared away. But there were recovery procedures. We worked with the flight control guys at developing recovery procedures in case that should happen, because we knew that was a problem.

The waste management, that's kind of always hard to talk about. That's a sensitive kind of subject, but we'll give it a go anyway. There's, of course, two parts to that: there's urination and defecation. The urination thing we kind of solved pretty quickly. The way we did that was—at first we thought that we could basically collect it into a tank and dump it overboard, but I think we learned pretty quickly that it's pretty hard to do that, because there's no way to get hold of it to pump it into the tank. So what we did was, we developed a urinal, which was like a cup, about so big in diameter [gestures], four or five inches in diameter, and six or seven inches long. It is filled with like a honeycomb material, if you can imagine what a honeycomb looks like. The whole cup's filled with that. It's a stainless kind of material.

Then that was on the end of a hose and went through a valve, and then through spacecraft plumbing to an [orifice], it was called a urine dump nozzle, which was a block of aluminum, hemispherically shaped, mounted on the outside of the spacecraft, that had a very fine orifice in it, and built into a block of aluminum was an electrical heater. So the electrical heater would stay on all the time. It was fairly low wattage. I think it was like 5 watts or something, very nominal, but it would raise the temperature of that block of aluminum to something in excess of 100 degrees.

So the way the system would work would be, then, that the crew member would open the valve, and then there would be a flow of gas now from the cabin through this urinal, through the valve, and out through this dump nozzle. But it was a very small flow, because that's an orifice or a nozzle out there, so it's like a small hole. Then he would urinate into the cup, and the honeycomb would kind of hold the [urine]. Because of the surface tension of liquids in a weightless environment, the stuff wouldn't go floating everyplace and the like. Then the air, trying to move through the cup also, because of the differential pressure between the cabin and deep space, would kind of push that urine all the way through.

We had a temperature sensor on this dump nozzle out on the side of the spacecraft. If you watched the temperature, you could actually see in flight, you would know by watching the telemetry when the guys were using the bathroom. You'd see the temperature in the dump nozzle go tumbling on down. We'd say, "Hey, look. He's using the bathroom."

Gradually, the guy would finish urinating, and the cup would be full, and the gas from the cabin would gradually just push that urine ahead of it and on out, and would kind of dry the cup. The heater on the nozzle would keep it from freezing, because not only the heater, but also the fact that there was a small hole, and it was being driven by a pressure, so it had a high velocity, and it didn't spend sufficient time in the pipe so that it would freeze. But often at the end of—and we knew this from ground tests—at the end of urination, and when you finish with the system, there'd be some little residual urine in there.

Incidentally, we also dumped excess water overboard the same way. Because, the fuel cells make water, and that's used for drinking and the like, and for water for the water evaporator, supplemental heat rejection. But still, we usually made more than we needed—so does the Shuttle today—and we have to periodically dump water overboard, and it was dumped through the same kind of a dump nozzle. But after you're finished dumping and the like, then the last little few dribbles of water would tend to want to freeze, and the little heater there would keep that from freezing. A little chip of ice might form, but the heat would vaporize part of it and build up a little pressure, like a little steam pressure and kind of flick off that little ice cube, flick it off. But we have had instances where dump nozzles have frozen up, right up through the Shuttle Program. So that was the urine dump processor system that we had in Apollo.

The defecation was much more crude. There was really nothing except a bag with some adhesive tape that the crew member pasted to his buttocks, and just did the best he could. That's all. I imagine it was kind of like a camping experience or something. We tackled that problem for the Shuttle, and it took a while, but we ended up solving that by—let me talk about that later.

ROSS-NAZZAL: Let's talk about the flight of Apollo 11. Where were you when they landed on the Moon?

SAMONSKI: There used to be a thing called the Mission Evaluation Room over in Building 45 on the third floor that was operated by the project office. It was led by a guy by the name of Don [Donald D.] Arabian, who you probably have interviewed, and it was just a bunch of tables and a bunch of engineers from the Engineering Directorate, who would look at the data and act as the brain trust, if you will, for the flight controllers. If they had something they couldn't handle, they'd ask us, and we'd, by and large, figure it out. So we manned that around the clock, twenty-four hours a day with expertise, and that's where I was for Apollo 11. I was in the mission evaluation room.

By [then] I was not subsystem manager anymore; I had been made assistant branch chief. So another guy by the name of Elton [M.] Tucker, who was my co-author for the experience report on Apollo, he was doing the job that I had done, and he reported to me. He was on duty, and I was the team leader for that shift for our division. Another guy who worked with me all those years, whose name I haven't mentioned, Don [Donald F.] Hughes, who's deceased now, at least the three of us [were] there, I remember that, when we landed on the Moon. We were in the Mission Evaluation Room.

After our shift was over, it was before Neil [A. Armstrong] got out of the spacecraft, but we just hung around there. Incidentally, July 20th is our wedding anniversary, so it made a nice wedding anniversary present for the Mrs., [Joanie]. But, yes, we stayed, and we watched that and saw a very crude television picture of Neil coming down the ladder and saying historic words. You never forget something like that.

ROSS-NAZZAL: That's fantastic.

SAMONSKI: Yes.

ROSS-NAZZAL: What about the Apollo 13 mission? Were you at all involved in the efforts to save the crew?

SAMONSKI: Oh, yes. You know, they made a big thing about 13. They launched on April the [11]th, I believe it was, and the launch time was, I think, thirteen hours and thirteen minutes [Houston (Central Standard)] time. And they just were flaunting this "unlucky thirteen" thing.

I guess they were pretty well into the mission—I forget how long it was; I should remember, but I don't—when they had the problem. My oldest son's birthday is April 13th, and we took him out to dinner that night. It was several hours into the mission when the failure occurred. We had gone out to dinner and had come home into the house, and the phone was ringing. It turned out it was Elton Tucker calling me from the Mission Evaluation Room, saying, "Hey, Frank, we've got a problem."

So I went in to work and pretty much lived there for the next three days or so. We did a lot of plotting of data, really sharpened our pencils, because expendables were really critical—oxygen and the water. One of the bigger things was the lithium hydroxide for removing the carbon dioxide. Of course, you know about the little thing we worked on by taking the command module cartridges and adapting them for use in the lunar module.

You know, people probably ask, "Why would engineers do such a stupid thing as to make cartridges in the command module square and cartridges in the lunar module round?" But, as you might expect, there are some good reasons for that. Just maybe to touch on a few of them, engineers design to requirements, and you kind of sometimes put the blinders on when you do that. What drove the command module design was this ability, this requirement to be able to change out the cartridges if the cabin was depressurized by this mechanism and the like. It had to be a design where the pressure drop across the cartridge was minimized.

In the lunar module, as best I can recall, the driving force on the design of the canister was [that] the CO_2 production rate by the crew member would be higher because of the work level he was at. By making the canister cylindrical, the flow rate could go down the center and radially outward, so that the amount of surface area that was being exposed to the gas flow was much higher than the flow-through cartridge that we had in the command module. So I think those two different requirements is what decided the shape of the cartridges. Radial flow [in the] lunar module versus what's called axial flow, down the center, in the command module.

But as it turned out, we adapted somehow. We figured out that we could take out the command module cartridge, which was about seven inches square and maybe five inches deep, and take one of the cards from the flight plan that the crew was carrying, and a plastic bag and form kind of like an enclosure around one end with tape, and then tape the hose to that enclosure, and then hook it to the lunar module. So you would pull gas through the cartridge down this hose and into the lunar module ECS and remove the carbon dioxide that way, and it worked out great.

ROSS-NAZZAL: Did you help evaluate the environmental control system after Apollo 13 returned?

SAMONSKI: Well, see, no, we didn't really have a problem with the environmental control system. The problem was with the cryogenic tankage that supplied the fuel cells. So we really didn't have a change or anything to make after Apollo 13. The failure wasn't related to our [hardware].

ROSS-NAZZAL: Do you have any other memories about Apollo that you would like to talk about or any comments you'd like to make about the program?

SAMONSKI: Well, the family and I, we did go to the Cape and saw the launch of Apollo 16. I was happy to see at least one of the Apollo launches. But, no, I can't really think, at least this afternoon, sitting here, I can't think of anything else about Apollo. That was from December '63 until sometime in the spring of 1969, I was just full time Apollo and lived and breathed it. I was very, very proud of having—I don't mean past tense. I am very, very proud—present tense—of my involvement. Worked with a lot of very smart, very smart people. Like I said, that list of people that you have here who were participants in this program, I probably know 95 percent of those, and I'm pleased to say they know who I am.

ROSS-NAZZAL: Well, you mentioned spring of 1969. You kept working full time for Apollo. When did you start working on other programs?

SAMONSKI: Just about then, yes. I picked up additional duties. In the spring of 1969, they wanted to promote me to GS-15, and the job of system manager didn't support that. So they made me assistant branch chief for Apollo, Apollo support, in [the] Environmental Control Systems Branch. As it turned out, that didn't work either, so they made me branch chief a few months later, around the time of Apollo 11.

That was in July 1969. We had half the branch working on Apollo and the other half of the branch working on advanced programs. Then when I got made branch chief, then I picked up the responsibility for advanced programs also. I guess right then, that summer, we had going within the directorate some studies about a Space Station, because I think many people thought within the agency that after Apollo the next thing was going to be a Space Station. Well, as it turned out, we needed a transport vehicle first. In the matter of a year or two, it evolved into the Shuttle being the principal objective, and the Space Station got put on the back burner.

But for the Space Station, [that] kind of mission, long duration in space, larger crew sizes and the like, that gets to be a much tougher problem for environmental control because of the way things are done. You can no longer do things in an expendable fashion, like use a chemical to remove carbon dioxide or to store your oxygen as oxygen and/or to just use water as much as we use water. You have to find ways to reclaim and recycle those expendables, and that involves chemistry and electrochemistry and some more advanced techniques.

First you have to collect the carbon dioxide out of the atmosphere, and you have to do that while maintaining the partial pressure of carbon dioxide in the cabin at acceptable levels. You can't let that get too high. The higher the CO_2 level is, of course, the easier it is to collect it because of its concentration and the partial pressure that it has and the like. But problems occur when the CO_2 level gets too high. I guess the Navy [had] found that out early in their nuclear submarine programs where crews were in a sealed environment for many, many days, like ninety days or so. As I understand it—and this is kind of digressing a little bit, but it's kind of the basis for it, that when a crew would go out on a mission for ninety days, and they had CO_2 levels that were higher in the submarine, there would be changes, chemical changes that would occur in the blood, that the doctors didn't like very much. So the emphasis always, for us, was to maintain lower partial pressures of carbon dioxide. Of course, you practically can't maintain it at sea level equivalent, but you can allow it to go up to something like three millimeters or so, partial pressure.

Water is a big thing, also. We had to find ways to reclaim the water. Your body gives [it] off [as] sweat and as urine, also, and that's probably the biggest single thing that the body needs, and it needs to be recycled. So there are some formidable challenges to keeping people in a closed environment for longer periods of time in a weight-effective manner.

ROSS-NAZZAL: So you began studies, and did you contract out these studies?

SAMONSKI: Yes, yes. We, by and large, contracted out studies and hardware developments, and we had sort of two efforts. We had a large contract that was aimed at producing a prototype of a Space Station class environmental control system, and I think that contract was awarded toward the end of 1969 to Hamilton Standard of Windsor Locks, Connecticut, for what was called a Space Station prototype environmental control system. And that program went on for about three years, and there's a lot to talk about relative to that program. That was a multi-milliondollar effort.

Then there were separate contracts for specific areas that were troublesome in the past, that we wanted to fix, like the water boiler, and we had a contract with Ling-Temco-Vought in the Dallas area, I believe. They're in Grand Prairie, Texas. I don't know if they still have that corporate identity of LTV or not. But we conceived of an idea—and I mean "we," I mean we, the government—of a thing called a flash evaporator, where instead of having all [these] controls like we had in Apollo where we had a back-pressure valve and wetness sensors, and sensors to turn it on and off and the like, it would be so designed that you would just spray water into a chamber that was surrounded by the heat transfer fluid in some fashion, and it would be kind of self-regulating basically. We set that as our goal, and we developed a thing called a flash evaporator, and that's flying on the Shuttle today. It's not quite as simple as I described it, but basically that's what it does. It's [just] a water valve, and none of those electronic controls and the like.

Also, to get away from the high weight penalties of the lithium hydroxide, we set ourselves a goal of developing a regenerable technique for removing carbon dioxide. Based upon some work that the Navy had done, adapted from the Navy, we developed a thing called a regenerable carbon dioxide [removal] system, or RCRS, that is also flying on the Shuttle today. Instead of using lithium hydroxide, it uses an imine, it's a chemical thing, a very high molecular weight material, a polyethylene imine, PEI-18, it's called. It's a got a molecular weight of like 1,800 or something, that's coated on very small beads, I mean really small, tiny little things, so it's almost like a powder, but it's granular, like a sand, kind of, and it's proprietary how this stuff is applied and the like.

But it pours like sand, and it's poured into a multi-layer bed, kind of like a heat exchanger is made, and sealed in there, and then when you pass gas containing carbon dioxide and moisture over it, the moisture activates this coating and the carbon dioxide molecules kind of stick to the coating. It's not a chemical bond or not a strong chemical bond, and it can be broken by exposure to vacuum. So you have two beds. One bed is online collecting CO_2, and the other bed is desorbing to space vacuum. And then through a series of valves, you just kind of switch those beds, and you just dump the CO_2 overboard, in a cyclic kind of manner, and no more lithium hydroxide, and it works good. But you've got to have the vacuum then, so it don't work on the ground, and you have to be willing to throw away the water, also, that you collect. So it's not good for real long-term kinds of missions like the Space Station, but it's just right for the Shuttle. It saves all the weight of lithium hydroxide.

Those are probably the two bigger things that we worked on and brought to fruition that arose out of problems in the earlier Mercury, Gemini, and Apollo Programs.

This other program, this Space Station prototype program, as I said, it attacked the whole system. It had a design phase, and then a manufacturing phase, and then a test phase, and it produced a set of hardware that looked like what would be in a Space Station, sized to support six people. It collected carbon dioxide using an electrochemical process, a process called EDC, electrochemically depolarized cell. It's kind of like a fuel cell device. [It] collects the carbon dioxide, and then the carbon dioxide in the second step is reduced in a reactor to methane and water vapor. Then the methane is vented overboard, and the water is then electrolyzed back into oxygen. [The] third step is then either water that you get from this reaction, this so-called Sabatier reaction that changes carbon dioxide in hydrogen into methane and water. That water is condensed, and it's electrolyzed back into oxygen and hydrogen. The oxygen is supplied to the crew to breathe, and the hydrogen you use to collect the CO_2, and it just kind of goes around in a circle, and you add in electrical power, basically.

So we built those three subsystems that would recycle the CO_2 to reclaim the oxygen. Then we built a trace contaminant control subsystem that would remove trace impurities in the air, and then we built a water reclamation subsystem that would, through a distillation process, reclaim potable water from urine. We would distill it, basically, at a low temperature and produce water of potable quality, although psychologically it's not very appealing to think about drinking reclaimed urine, but physiologically it's acceptable

As I think I said, the program ran probably about three years, and the hardware was delivered to us here in Houston, and we set up a laboratory in Building 7, and we tested that hardware pretty rigorously—all unmanned. It [was] all unmanned testing.

ROSS-NAZZAL: That's pretty impressive. What happened with the study? Was it just shelved after the three-year study?

SAMONSKI: Yes, pretty much. All the emphasis at that time was on Space Shuttle, and so sort of what we learned or [were] learning was just kind of put on the shelf until just this more recent interest in the Space Station program. Now, I really don't know, because the Marshall Space Flight Center [Huntsville, Alabama] is responsible for the environmental control system on the Space Station, I really don't know how much of that technology is employed in the Space Station. I've been away from it too long.

ROSS-NAZZAL: So in addition to doing these advanced studies, were you involved at all with any of the other programs that were going on at the time?

SAMONSKI: Very little. The ASTP [Apollo-Soyuz Test] Project, the docking module hardware, which were just some valves and the like, those were done within my branch, but I didn't have very much direct involvement. I think my attention was kind of diverted more toward the advanced program stuff.

There were any number of other advanced development contracts, smaller ones, on specific areas of improvements and problems and the like. We had a contract with the Boeing Company, looking at a technique called incipient fault detection, where you could measure the vibrations coming from a piece of rotating machinery, like a fan or a pump, and be able to detect that a failure was going to occur. We brought that to where we could have put it on a spacecraft, I guess. The Boeing Company, I think, had some commercial applications in the petrochemical industry for that technology, in oil refineries and the like, where they would use that to avoid having an unplanned shutdown of some refining process.

We worked a fair amount with electrochemistry, different techniques for electrolyzing the water, [and] water vapor. There was a process, water vapor electrolysis that would electrolyze water directly from the humidity in the air. I know there were a number of different contracts that we had.

ROSS-NAZZAL: My research also indicates that you worked on the source board for the Shuttle EMU [Extravehicular Mobility Unit]. Could you talk about that a little bit?

SAMONSKI: No, I didn't work on the source board for the Shuttle EMU. I was the chairman of the source board for the Shuttle portable oxygen system, which was the emergency breathing pack that was carried in the Shuttle in the event there was some kind of a problem with the Shuttle. At one time, it was thought that if you had an Orbiter that had a problem on orbit, that you would send up another Orbiter, and you would transfer the crew between the two orbiters. While they were being transferred, this portable oxygen system is what they would use to keep them alive, basically. As I say, I was the chairman for that source board.

ROSS-NAZZAL: Could you describe that unit to us?

SAMONSKI: It was pretty simple. It was just basically an oxygen tank and a regulator, and mask, breathing mask, and a breathing regulator. I think that was about it. Really pretty simple.

ROSS-NAZZAL: I'd like to have you compare and contrast the Apollo environmental control system with the Space Shuttle design, the environmental control and life support system, if you could.

SAMONSKI: Okay. Let's see. There's different ways to approach that. The Apollo environmental control system was basically a pressure suit support kind of system like Mercury was, although the crew did get out of the suits when they were translunar and the like. For launch and entry they were in pressure suits. The principal processing components were within the pressure suit circuit. It also was a reduced pressure system. It operated at 5 pounds per square inch absolute, which was about a third of an atmosphere, and it was a single gas system. It was 100 percent oxygen when you were on orbit and things were stabilized and the like.

The Space Shuttle is, first of all, a shirt-sleeves environment. Although the crew does wear launch and entry pressure suits for launch and entry, the primary mode of operation is with them in flight coveralls in the cabin, in what's called a shirt-sleeves environment.

Then also the pressure is a sea level [equivalent]. It's 14.7 psia, just like we have here at sea level, and it's a two-gas mixture. It's oxygen and nitrogen, and the composition is much the same as we have. It's slightly enriched over what we have on Earth, but just very little. By having a two-gas atmosphere, then that implies certain control functions you have to do. You have to maintain not only the total pressure at 14.7, but also the partial pressures of the oxygen and the nitrogen and supply them each separately, makeups and the like, which is a little trickier. It means you have to sense partial pressure of oxygen.

The gas distribution system within the Orbiter is much more extensive than in the Apollo spacecraft. You've got all these ducts running everyplace, because the volume is so much larger. As I recall, the command module volume was in the neighborhood of something under 200 cubic feet. The Space Shuttle, the Orbiter volume is, I think, 2,500 cubic feet or something. It's much, much larger. It has the airlock function, and you've got to be able to provide for crewmen going in there and isolating themselves from the cabin, and then being able to get out into space. To do that in Apollo, you had to depressurize the cabin, like we did for some of the later Apollo missions, which were called the J missions, when they had an experiment package back on the service module, they would depressurize the cabin, and one crew member would go out in an EMU, and go retrieve that experiment. But for the Shuttle, that's got to be routine. The rest of the cabin functions normally, and the EVA crew members get into the airlock and depressurize it and go out [into] the payload bay.

Typically, the Apollo spacecraft was a single-use mission, one mission kind of thing, where the Shuttle has a hundred-mission requirement. All the rotating machinery [such as] the pumps that pump the coolant around, and the fans or compressors that move the air around, they have a 20,000-hour life requirement on them, and we had to do some testing to ensure ourselves that they would meet or exceed that lifetime requirement.

Those seem to be maybe the major differences between Apollo and the Space Shuttle. Yes, I think so.

ROSS-NAZZAL: Well, you had mentioned that the Apollo capsule was much smaller than the Space Shuttle. How were you able to keep oxygen levels consistent and cabin pressure consistent, given the size of the Space Shuttle? How did you work that out?

SAMONSKI: Well, gas fills the volume that it's in, so there's really no problem maintaining the pressure if you have a device that will add gas as a function of pressure. A thing called an aneroid works very well, basically what's in a wall barometer. You know where you've seen wall barometers, sometimes you'll see a little gold disc or something. It expands and contracts, and that's the functioning element within the regulators that can control the pressure and the like.

But then in a weightless environment, of course, you have stratification that will occur, and you need to have forced mixing and circulation and the like. The bigger the volume, of course, the more important that is. There's concerns or there were concerns, and maybe still are in the Orbiter, for instance, like in the sleep stations that if you didn't have a little bit of an air flow to each sleep station that the crew member asleep, exhaling carbon dioxide, would just build up a bubble of CO_2 around themselves, and that it might not be good. Now, I don't think we ever had that problem, but that's conceivably what could happen. We do have, as I said, a good distribution system within the Orbiter, ventilation ducts running different locations and the like.

ROSS-NAZZAL: Were there any major problems that occurred in any of the earlier Space Shuttle missions with the Shuttle ECLSS [Environmental Control and Life Support System]?

SAMONSKI: I think we were pretty fortunate relative to problems with the Shuttle ECS. We did have some problems early with the waste collection system, and we thought that would be a tricky problem, and it was. Again, this is the solid waste collection. It's hard to test for that condition on the ground. You know, you have the KC-135 flights that produce a short period of weightless conditions, some thirty seconds or so, but that's kind of a short time period. It's really not compatible with trying to evaluate going to the bathroom, if you will. So we had to wait until we got into orbit to find out really what the problems were.

We had some bathroom problems early, and it took us a little while to get those squared away. It's a funny thing. It makes me think—about the time of Apollo 11, my Aunt Lucy and Uncle Art came to visit us. They [lived] in New Jersey. That's where Joanie and I are both from. My Aunt Lucy was a schoolteacher. She [was] a pretty sharp old gal. Of course, the people were along in years. Uncle Art served in the First World War. He had he's been through quite a few of the battles, Belleau Wood, I think, and the like, some of the bigger ones, had been gassed, mustard gassed. Anyhow, my Aunt Lucy, [what] she really wanted to know, she [said] "Tell me the truth, Frank, how do they go to the bathroom in space?" Everybody wants to know that.

So it took a few flights to get that kind of sorted out, but I think we've got a pretty good handle on it now.

ROSS-NAZZAL: Was that actually contracted out at the beginning of the Space Shuttle Program? I know it's contracted out today, the waste management system.

SAMONSKI: To build it, you mean?

ROSS-NAZZAL: To build it and to also take care of it during the in-flight problems.

SAMONSKI: We had a contractor to build it. That was Hamilton Standard, who built several of the other parts of the Shuttle environmental control system. If you mean the servicing of the waste collection system between missions, cleaning it up for the next one and the like, well, yes, that's done at the Cape by—I guess it's the United Space Alliance now, whoever the company is that processes the Shuttle between missions does all of that. Used to be, I guess, Lockheed, but now it's a bigger conglomerate.

But the formative work was kind of done under NASA's direction. We wrote the specifications for the hardware and had it built and concepts-tested, and I think, long before the Shuttle was a program. We had like three separate contracts looking at different aspects of waste collection in a weightless environment. Also, I didn't mention, but the urine collection for females is not as easy as it is for a male, and that was a separate problem that I guess we've managed to solve somehow. I'm not sure I'm really answering your question about the waste collection.

ROSS-NAZZAL: You have answered it. I'd like to move on to *Challenger*.

SAMONSKI: Okay. Not much to say about *Challenger*. That was in January of '86. I was still branch chief, but over a different kind of branch.

I'm going to back up a little bit. The Space Station Program management within NASA was broken up into different work packages, and we here at the Johnson Space Center were what was called Work Package 2, and Marshall Space Flight Center was Work Package 1, and I forget who the other two work packages were, which Center. Goddard [Space Flight Center, Greenbelt, Maryland] was one of them.

The management here at Johnson also wanted what they call the Level 2 responsibility, which is the integration of the work packages, almost like the chief function of them. And I think that what our management did, in their zeal to get that Level 2 responsibility, they gave away the life support responsibility on Space Station to the Marshall Space Flight Center. So all the work we were doing in-house, getting ready for the Space Station with these advanced techniques and the like, they were somebody else's responsibility now. I was very upset about that. I was really very upset about that. I thought it was purely a political decision, and I guess it kind of opened my eyes as to the way things happen in the government.

Anyway, we sort of reorganized within the division and put less emphasis on life support and more emphasis on the EMU kinds of things, advanced space suits, and my branch got reorganized, and we redirected our resources at looking at improving the EMU and its many functions.

So, part of that EMU-related stuff was the airlock that would be used on the Space Station, and the equipment within the airlock that would be used to service and check out the EMU between EVAs, rather than like the Shuttle now, [where] you don't have but a couple EVAs, and then the thing comes back to Earth, and it can be all checked out on the ground. [For] Space Station, the EMU is going to be on orbit for months, and be subjected to many, many EVAs, and so you needed some different method for checking it out and servicing it between EVAs. So that was kind of part of what we were looking at and the like.

I was at an airlock-related meeting in Building 1 with representatives from Marshall and other people, and it was the day of the *Challenger* launch, and we sort of stopped the meeting to watch the launch on the tube that was in the conference room. You've seen the pictures. You could tell—anybody could tell something was not right about what you were seeing in the sky.

I don't know how to describe that. That was just so shocking. It just took a while to digest. I got back to Building 7, and they replayed those tapes over and over again, and they had different camera angles and the like, and then you could begin to see maybe what had happened and the like.

Of course, things changed quite a bit then after that. There was a long down period. I guess it was almost the better part of two years.

But anyway, I was in the airlock meeting at Building 1 when it happened.

ROSS-NAZZAL: What did you do during that down period?

SAMONSKI: It didn't affect us directly that much. I think we just went on. We used that opportunity to review all of our areas of responsibility for safety and what was not right, and what could be improved, and that sort of stuff, but there was no big activity within our division that I can recall.

ROSS-NAZZAL: Well, in 1988, you actually took a new job managing the laboratory operations.

SAMONSKI: Yes, I think it was in October of 1987 that the fellow who was on the division staff and was responsible for laboratory operations left the division to go some other place, and the division chief asked me if I would move down from my branch chief's job to work on his staff in the job of laboratory operations. We have a pretty extensive laboratory in that division, Crew and Thermal Systems Division. Some of our more important tasks are to conduct crew training of the astronauts in the use of the EMU, and they do that through two separate kinds of tests in two different facilities.

There's what we call the eleven-foot chamber where each astronaut who's going to be an [EVA] crewman goes through what's called an EMU fam, or EMU familiarization run, where he's in an altitude chamber. The EMU provides his life support, and he is on the treadmill, and he exercises and gets his metabolic rate up higher and the like, and there's a weight suspension system that takes some of the weight of the EMU off of him—of course, it's a fairly substantial weight—so he doesn't have that burden, as part of the training.

He's in the chamber at a hard vacuum, and so that's a hazardous kind of testing, and it's a big deal to test like that. Before each of those tests, you do have a thing called a Test Readiness

Review Board, and I generally chaired those, where we would look at all the procedures and the hardware and the pedigree on the hardware and everything it's been exposed to, and safety people would have their day in court and speak, and quality and all that. You try and come to a rational conclusion that you were ready to perform the test.

Also, being sure that the facility was ready and all the maintenance was performed and all the calibrations were done. They are only good for a certain period of time and have to be repeated periodically, and making sure all that paperwork was straight was all part of that job.

So we did this EMU fam [familiarization] run and all the stuff that went with it for each of the EVA crew members. Then there was another separate test in another facility that is a larger chamber. … [It is called the ETA (ECS Test Article.)] It's a pressure vessel. On the inside is a replica of the Space Shuttle pressurized compartment. I mean, it's got two floors, and it's kind of shaped like the Shuttle. It's got the Shuttle environmental control system installed in it, [and] the ductwork and all that, and it has an airlock, and it has all the airlock controls in there. Then there is a way, while the crew member is in the airlock, to vent the airlock to hard vacuum so that for functional purposes the crew members are like they are going out into the payload bay through the airlock. We call that particular series of runs a crew training run. That's got all the same kinds of rigor with test preparations and the like, and culminating in a Test Readiness Review Board meeting to see that you're really ready and safe for the test.

From a functional standpoint, the environment that the crew member is in and is exposed to is virtually identical to what you [would] see in the Orbiter. It might not look like it to him from the inside, but functionally, for engineering purposes, it's identical.

ROSS-NAZZAL: Later you became the manager for the Space Shuttle, for the Crew and Thermal Systems Division. How did your position change? What were your duties then?

SAMONSKI: We got a new division chief, and he asked me to help. Because we were doing so much Shuttle work, he asked me to take on [the] responsibility for the Shuttle work that was being done within the division. That was, to a large degree, a resources management kind of job. Not in the bean-counter sense, but our Shuttle budget was $30 million a year or so, and the program managers within the programs, Level 2 and Level 3, through whom those resources came, we had to go justify to them what it is we were spending the money on, and what our budget needs were, and then, too, after you've nailed down your budget for the year, to make sure you delivered what you promised to deliver for that budget. Just working with the people within the division, and moving resources from here to there to get work done where you have a little more cushion, and [to] areas where there's a little more difficulty.

ROSS-NAZZAL: Your last position was as engineer for the Manufacturing Materials and Process Technology Division.

SAMONSKI: No. No. That's my son. My son Frank Samonski. He's still over there in Building 15. Now, I retired in May, early May of 1994, and I didn't do any more work.

ROSS-NAZZAL: I wonder if you could tell us, you worked for the Crew Systems Division for quite a long time.

SAMONSKI: My whole career.

ROSS-NAZZAL: And I'm wondering, could you tell us how the division itself changed over time?

SAMONSKI: Sure. Going way back to the Space Task Group, okay? When I went to work for Dick Johnston, Dick was part of the nucleus of what would become the Crew Systems Division. I'm glad you raised this, because I did want to talk a little bit about the organization at the Space Task Group. When I went to work there in early June 1959, there was probably about 100 people [in] all. Most of the people were juggling several balls, and there were three divisions.

There was the one under Max Faget, which was the Flight Systems Division, and within that division there were a couple or three branches, but the branch I worked in was called Onboard Systems, which is pretty all inclusive, and that was under a guy by the name of Harry Ricker.

Some of the people I saw on your list, Tom [Thomas V.] Chambers worked there in that branch. Tom Chambers and Dick Johnston, Phil [Philip M.] Deans, John [B.] Lee. Just about all the different things that went into the Mercury spacecraft, there was somebody within that branch working on it.

Maybe another branch in Faget's division, I believe, was Guidance and Control under Bob [Robert G.] Chilton.

But there were only three divisions. Let me go back. Then there was the Operations Division under Chuck Mathews, and I think Chris Kraft was his deputy or special assistant for flight control or something like that. Within the Operations Division, there was a little contingent of medical people. There was Dr. Stan [Stanley C.] White, Dr. Bill [William S.] Augerson, Dr. Jim [James P.] Henry, and a couple of engineers, couple or three engineers—Gerald [J.] Pesman and Jack Grames and Jim Hires. Those people came, got combined with Dick Johnston and myself and a fellow that had come on board to help us with the suits, a fellow by the name of Lee [N.] McMillion, and we [were] made a small branch in Faget's division, called Life Systems Branch, and Dr. Stan White was the branch chief, and Dick Johnston was the deputy branch chief.

We kind of stayed like that as the Life Systems Branch in Faget's division until we moved to Houston. Right about that time—of course, we were growing all the time and getting more people. Joe [Harold J.] McMann came to work, and he worked in the little group that I had, and this fellow Don [Donald F.] Hughes came, and another fellow by the name of Ted [Theodore B.] Leech, and Mort Schler. We grew all the while.

So about the time we moved to Houston, we became Crew Systems Division, and Dick Johnston—I don't think Stan [Stanley C.] White ever came—yes, yes, he did. He came. When we were at the Lane Wells Building, Stan White was our division chief for a while, but not very long. Then I think he went back to the military, and Dick Johnston became the division chief.

I don't know what size we were at that time, at the Lane Wells Building. I do remember that I was the section head. It was the Systems Evaluation and Project Support Section that I had, and I was in a branch that Ed Smylie was the branch chief, head of an ECS Branch, I guess it was called. I had, I believe it was like seventeen people reporting to me, but some of those were technicians. There was about a fifty-fifty split of technicians and engineers.

I think we stayed, the Crew Systems Division, like that, including the medical aspects, for maybe a year or so. When we moved out to the site from the temporary building, which was in April of 1964. Our Building 7 was not ready yet, was not even constructed yet. We were in Building 4, which was the astronauts' building, and we were all still one division, medical and engineering sort of stuff. But sometime before we moved into Building 7, which was probably about a year later or so, the medical people had split off and had formed some other organizational element. I guess that later became part of the Space and Life Sciences Directorate.

Then just the engineering people stayed in the Crew Systems Division. It stayed that way, basically, the division [being responsible] for environmental control systems and space suits and portable life systems, and a collection of crew equipment survival gear and the like for quite a few years. Then somebody got the bright idea that we did a fair amount of thermal work and it would be a good idea if we expanded the scope of our name to include "thermal," so it became the Crew and Thermal Systems Division, which it still is today.

ROSS-NAZZAL: That's a nice organizational history. Thank you. So you retired in 1994 from NASA.

SAMONSKI: Yes.

ROSS-NAZZAL: I have to ask. Perhaps this is your son. Did you work as a contractor?

SAMONSKI: No, I never worked as a contractor.

ROSS-NAZZAL: All right. Just wanted to verify.

SAMONSKI: He does.

ROSS-NAZZAL: So why did you choose to retire from NASA?

SAMONSKI: You know, I'd had a good life and I had done most of the things that I wanted to do, and some of the things, the way things were going at the Center, resources were getting tighter, and it seemed regulations were getting stricter. It just seemed like a good idea.

ROSS-NAZZAL: What do you think was the most challenging part of your career at NASA?

SAMONSKI: I don't know. I saw that on your list of questions. That's kind of hard to say. It seems, in looking back, it seems I spent an awful lot of time trying to make water boilers work. There's something I neglected to tell you. I had a pretty good rapport with Dr. Joe Shea, who was the program manager. He's a very smart guy, and I could tell you a fair number of stories about that. But we were having problems with the water boiler working in the early period of my involvement, probably like '64 or '65, and it had to do at that time with the way we were sensing the wetness of the steam that was coming out of the boiler, and thereby how we resupplied water.

I was of the firm opinion, me—stubborn me—that we should test that boiler in an inverted attitude. Based upon what we'd seen in Mercury, I'd thought that if you could demonstrate that gravity did not affect the way the device functioned, then you could be somewhat more certain that in the absence of gravity, the thing would work as you would expect it. That's not 100 percent true, but I believed it gave you—I felt strongly it gave you confidence.

Joe Shea wasn't having any part of that. I think that maybe North American management got to him, or what, but he thought it was just plain foolish that we should test a piece of hardware in an upside-down condition when it was never going to see upside-down operation, and that that was just adding complexity and unrealism to the test program and resources, too. So I fought that tooth and nail. I really did.

As it turned out, not too long after that, we flew one of the early unmanned Apollo spacecraft, Apollo—it was either spacecraft 9 or spacecraft 11, which was on the smaller Saturn, Saturn AS-201 or 202. I forget just which. And the water boiler stopped working. It dried out. I think from the day we knew pretty well it dried out. And "So I told you, Joe."

So after that, we tested it upside down. Still, it took us a while to resolve how to properly sense for water content in the boiler. And I think in my paper you'll read, there's a little section of the sponge, and there's a sensor embedded in the wick. What was causing some of the latter problems was the fact that the sponge was over top of those sensors, and the wet sponge was causing the thing to sense incorrectly, so we ended up finally cutting away the sponge around the sensors, and that seemed to do the trick, and we didn't have any more problems.

But there were several other problems associated with that water boiler, after the problems we had with Mercury. Then we had just had them one right after the other on Apollo, right up until Apollo 11. So, yes, that probably sticks out in my memory as one of the more significant things in my career.

ROSS-NAZZAL: What do you think has been your most proud accomplishment?

SAMONSKI: I think that would have to just be the aggregate of it all, having been involved from the very beginning, like I was, right up through the Space Shuttle Program. I couldn't point to any really one thing. I think just having been at the right place at the right time to become involved in something like what I was involved in. I guess that's what I'm most proud of.

ROSS-NAZZAL: It's a great program.

SAMONSKI: Yes.

ROSS-NAZZAL: Let me ask Rebecca and Sandra if they have any questions. Rebecca?

WRIGHT: I just had one, if you could share briefly with us something we had talked about at one of the breaks is about weight. How much factor was the weight when you were designing and developing everything that you did within your division?

SAMONSKI: Weight was a parameter all the time, and people kept track of the weight and balance of the spacecraft, the people within the program office. It was their full-time job to keep track of weight and balance. Because, where the weight is in the spacecraft is important, too, for the center of gravity and the like. Periodically had to give reports, but by and large, I don't think it was too big of a problem for me, for the environmental control system. I think the engineers doing the detailed design at AiResearch and at North American did good work, and we didn't have any significant weight increases.

ROSS-NAZZAL: Do you have any questions, Sandra?

JOHNSON: No.

ROSS-NAZZAL: Is there anything else you would like to talk about before we close today?

SAMONSKI: I'm sure there's a number of things that I'll think of, but right now I can't think of anything else.

ROSS-NAZZAL: All right. Well, thank you for coming today. I enjoyed it.

SAMONSKI: You're welcome.

[End of interview]