NASA MSFC Oral History Interview
Steve Johnson Interviews – Apollo/Saturn Program

Len Worlund
Interviewed by Steve Johnson
Huntsville, Alabama – Unknown, Circa 2012

Steve Johnson: I am talking to Armis Lenall Worlund, known to most of his friends at Marshall Space Flight Center as Len Worlund. He worked at Marshall and NASA [National Aeronautics and Space Administration] from 1961 until August 2003. Len, would you talk about your education? What prepared you do be in the space program?

Len Worlund: I am not sure my education was specifically for the space program. I went to school at Auburn [University] and got a B.S. [Bachelor of Science] degree down there in 1957. I had a girlfriend there that I was interest in, so I stayed around and went to graduate school and got a master's degree in 1959. What really happened, since I was in mechanical engineering, you worked a lot of mechanical processes and that type of thing here. That was before the days of the aerospace specialization. I got a good education. I thought when I got out that I would probably be working in structures or something of that nature. I was interested in doing that.

I first came back to Huntsville [Alabama] to work for Chrysler before I went to work for NASA. I asked for a job in structures and they did not have a job available. They said looking at my background that I could handle thermal, fluid, and propulsion related stuff. They said, "How about you give it a try and if it does not work out, when we get an opening in structures, we will take you over there." It never happened. I stayed in propulsion. I liked it, it was interesting. I do not know that I had a special education that prepared me for it.

Johnson: You started out and were thinking structures. What got you into the space program?

Worlund: I came out of Auburn and went to work with what used to be Kim Strand, they had a plant in Decatur [Alabama], had another plant in Pensacola [Florida]. I was sent to the Pensacola plant and put into a development group. In that development group, they basically had two branches here. The guy I worked for, he was losing the battles. The other guys were getting all the work and our group really did not have much to do. I am impatient. I do not sit around well, it bothers me, frustrates me. I stayed there about a year, came back to Huntsville, and ran into a guy I knew down in Auburn and he was working for Chrysler. We got to talking and he said I should come back up here and go to work. I told him I was not smart enough to work in the aerospace industry. He said, "If I can work here, you can work here." I eventually made an application, came back up here when they offered me a job, and stayed here. There was no magic to it. I happened to meet somebody that said he would get me an application.

Johnson: From Chrysler, how did you end up at NASA?

Worlund: I worked for Chrysler for about an month, maybe slightly longer. My boss came in one day and said they had a request from NASA who said they wanted some people to come and work out there. He said, "We showed them your resume and they think you might fit in and they would like to interview you and see if you would go out there." They sent me to an interview. I interviewed with [Robert R.] Bob Head, [Charles C.] Charlie Wood, Harvey Connell, they were all in the heat transfer, fluid flow group out there in the Structures and Propulsion Laboratory. I went to work for Charles Wood in the Fluid Flow and Heat Transfer Group working internal flow.

Johnson: Working heat flow, what does that mean?

Worlund: That means that you are working the processes that takes place on the inside of the vehicle. You have heat that comes into the vehicle and that heat is absorbed by propellant, mostly. When that heat comes into propellant, then the propellant, if it is LOX [Liquid Oxygen], it is at minus 270 degrees Fahrenheit, is warmer than almost everything you are dealing with. Heat is sucked into the propellant and where does it go? What is the temperature of the propellant? When you have to pressurize the propellant, then you pressurized it with warm gas so you can have the lightest pressurization system you can, but it is losing heat to the propellant, losing heat to the tank side walls. How much heat does it lose? How fast does it lose it? How do you size the pressurization systems to maintain pressure in the tanks?

Johnson: How important was this in the rocket business?

Worlund: In the rocket business, if you cannot maintain pressure in the tank, then pressure inside the tanks helps stabilize the tanks. If you want the tanks to be lightweight, you would like to have a lot of pressure in that tank, but you have to know the pressures there. If you depend on pressure being there and it is not, then the tank collapses. If you cannot depend on it, then you have to make the tank stronger. If the tanks weigh more, the payload goes down. The other side of that is over on the propulsion side. The propulsion guys have an engine and an engine pumps operate in a region of what they call above the minimum net positive suction head. That is the pressure that is above the vapor pressure of the fluid that allows the propellants to be pumped without it cavitating, turning to vapor.

Johnson: Temperatures are very important.

Worlund: Temperatures are a very important part of that. Pressures are a very important part of that. Temperature and pressure go together. The higher the temperature goes, the higher the pressure you have to have to suppress the boiling.

Johnson: What were the main technical challenges in that field, doing what you were doing, during the Saturn days?

Worlund: I think the primary thing was how do you maintain a pressure schedule that minimizes the structural weight impacts, minimizes the propulsion system weight impacts, and assures you have adequate pressure so the propulsion system, primarily the pumps, work properly. It is a balancing and an integration of things that provide flow of things that heat the propellant up. How do you get the temperatures there? How do you make the heat exchangers work? How big do you make the heat exchangers, how do you control them? Our problem was primarily how do you integrate all of that stuff because they interact and they have to work together. How do you get it all done and how do you get it done so that you know that it is going to be there.

Johnson: Was there a lot of testing to do what you just said?

Worlund: Yes, a lot of testing.

Johnson: How did you test?

Worlund: We tested in all sorts of ways. On a pressurization problem, we would start out on a small tank. We would pressurize a small tank, we would take data on it, we would do an analytical model of it, we would match that data. Then we would scale it up and go to a bigger tank. We started out with small-scale tanks on a Saturn S-IC stage, we went to a third scale tank, I believe, and then we eventually went to a full-scale vehicle. Each time you do the analysis, do the test data, and then you use that as a building block to gain confidence that you know what is going on.

Johnson: Did you do this testing on the test stands? Did you do it in heat chambers? Where did you do it?

Worlund: Mostly on test stands.

Johnson: You would fire rockets?

Worlund: In some cases early on, when we had the smaller things here, since we were working with the processes that were going on inside the tank, we did not have to fire the rocket. Once the propellant left the tank, we did not have to go and burn the propellants, we could just pressurize the system and discharge the tank into another tank.

Johnson: Was this the kind of testing, kind of work, that required you to develop new tools, new materials, or new test equipment? Did you have to come up with new things to answer all the questions you had?

Worlund: We did a lot of work in the process. We had done new insulations. If you go back and look at some of the things we did in this area of work, we had to insulate the tanks. Early on with the Saturn V, the S-IC stage, we did not insulate LOX tanks, that is liquid oxygen. It is at about minus 297, if it remember correctly. What it would do if you let that tank sit out there, moisture would condense on it, you would build up a layer of frost on it. Once you got the layer of frost on it, frost is a pretty good insulator. We would not insulate those tanks and just let the frost insulate it. You have seen the Saturn V photographs as they lift off and you can see all these sheaths of ice and frost falling off of it. As soon as it vibrated, it started shaking off, but while it sat on the pad, that was pretty good insulation.

Johnson: That was your new material, ice. (Both Laugh)

Worlund: Yes, it helped us out. When you went to some of the other stages, the upper stages, where you had liquid hydrogen in those tanks, now the moisture that condense on there, there is such a difference in temperature, it will freeze. Now you have to have an insulator in there and you cannot let the water get to the surface because then it will not shake off. What we really did, as you probably have heard, on the second stage, the S-II, we developed an insulation that we flew early in the program that was a honeycomb. We filled the honeycomb with a foam, we put a sheet on it, and we glued it onto the tank and put a sheet over it to keep the air out. When you would fill the tank with the liquid hydrogen, the air that was in the foam would basically precipitate out and freeze, creating a vacuum in that thing in there. The moisture was now solid. You were carrying the weight of the moisture, but it was a very, very good kind of insulation. It was kind of fragile, so we eventually went to a spray-on foam insulation. Materials guys are the guys who would go off and do most of that development work.

We used that and it helped us decide and configure our systems. As far as my group goes, we did not actually build the insulation.

Johnson: Yours is more figuring out temperatures and what happens to the temperatures during a mission.

Worlund: We would tell you we needed a better insulator and we need it to be about this good. If you get it this good, we can change something else. I worked most of the things in what I like to think of as a systems integration area. We try to get all the technical disciplines to play together so that we end up with either the most efficient or the lightest weight, or the highest performing, depending on whatever you wanted out of the system. We are basically in the business of making sure all the structures, the insulation, the fluids, and all the pumps and everything worked together. We were basically what you would call a systems engineering group.

Johnson: The time you were working on the Saturn, we were under the calendar, we had a presidential promise we would launch at a certain time, within the decade. The work you did, how much pressure was there for you to figure these things out quickly?

Worlund: A lot of the pressure was self-imposed. You had to keep up with whatever else the other people were doing. What they really did in those days, we had a lot of money. If we had an issue that we needed to do some work on, we might pursue more than one solution for that item. We may have off to the side here, if we need to make an improvement in an area, in the insulation, for example, we may have two or three different insulations that were out there in tests. As they matured, we ended up taking which one was best at the time we had to finally make the decision.

Johnson: Let me make that straight, money was not anything you guys had to worry about, at least in your work.

Worlund: That is right. Money was not an issue we had to worry about. If we went to them and said we had a problem, then they would give us dollars and we would go off and have more than one solution in work. If we thought there was a risk to it, we would have a second one going along in parallel.

Johnson: Did you have to work long days? Were there a lot of hours put in to do the work you did?

Worlund: Yes, early on in the days, we were working ten hour days, five days a week. Then we would probably work eight hours on Saturday. If you needed to come in on Sunday, then you would do that. Mostly, Sunday work was when you had a problem, failures or something like that, that had to be worked. The standard rule at that time for most of the time was a ten hour day with an eight hour Saturday.

Johnson: How about the work environment? Was it a good environment? Did you have fun?

Worlund: Yes, you had fun because there were things that were interesting, it needed to be done. You felt an urgency to get it done, but you also could see where it was going. You could see the program and the hardware develop as you went along. Every day was fun to go to work.

Johnson: You were testing performance more than you were building something or coming up with a design for hardware. If I am understanding correctly, you were testing other people's designs or they had to find something, a material, that would accomplish the requirements you felt were needed to make the rocket successful. Was that okay? Did you ever wish you could build some hardware?

Worlund: We built hardware, but I did not personally build it.

Johnson: They built hardware because of the work your group did.

Worlund: Yes, but like I said, we were a systems integrating group and we were an analytical group. We did not build hardware, but other people built hardware. We would tell them we need a piece of hardware that would do this, or I need a control valve to control my flow and I need it to have this type of response time on it. It needs to have this type of resistance when it is open. It has these types of failure characteristics, but I did not build that hardware. Somebody else built that hardware. We were kind of analyzers and hardware requirement suppliers. We did not actually have to build the hardware.

Johnson: Any dead ends you reached any time you were trying to do something and you could not make it do what you wanted it to do?

Worlund: No, I do not think so. You found some things that you worked on that as you went along, you were carrying another one in parallel, or somebody else was carrying another one in parallel. You might end up deciding that solution is better than this one, but one of them worked. I do not remember an occasion. There were occasions that were not directly a part of that early on in the F-1 Engine Program. The F-1 Engine Program, the combustion was unstable and the flame oscillated and created pressures and this type of thing. Some other people may have told you about it, but everything you tried to do, you tried to change the acoustics, you tried to baffle the injector, you tried to change the resistances, and you tried to put in acoustic cavities, and all of those things were generally less than totally successful.

Johnson: In the work you were doing, were there ever any surprises?

Worlund: There were things that came up that we did not anticipate. There was hardware that failed that you may have to go back and redesign, or things of that nature. As we were in the Apollo Program, we had a problem that came up that the Air Force first found out about, a pogo problem in the Titan Program. It is a thrust oscillation of the engine that couples with the frequency of the structure. The thing acts like a pogo stick and it vibrates if it is unstable and keeps going bigger and bigger until it falls apart.

Johnson: This was not a new problem with the Saturn?

Worlund: It was a new problem in the Titan Program, we read about it in *Aviation Week*. Titan has this problem and our configuration is kind of like Titan. Maybe we ought to find out if we could have this problem as well. We went off and started working the problem. We went to the Titan people. The Titan people were very gracious and very helpful to us. They gave us information about it. Their solution was to put accumulators, little gas bubbles, down close to the propellant inlet. That does not change the operation of the pump, but it does change the frequency on the feed system on the stage. If they are coalescing and causing a problem, then you put the accumulator in there, it separates the frequencies, and the problem goes away. We knew what they did. We went off and did a lot of ground tests to try and determine what our frequencies were and what the frequency of the engine was. Analytically, we showed the problem was one dB [Decibel] stable.

Johnson: dB stable? What does that mean?

Worlund: Decibel is a measure on a log scale. Six dB is a factor of two. It is on a log scale. What the guys did is they gave us a requirement and they wanted to be six dB stable. We were one. We were not sure the analysis was any good. One dB stable, maybe it is bigger than that, maybe it is less than that. We did not know how good our analyses were. Prediction wise, we had to put a fix on this thing of some nature. We went off and started looking at a fix to put on it. We were trying to change the frequency by putting some gas in the feed line. We did not do anything with that, we kept having that as a solution until the second flight of Saturn. The first flight, we saw a little bit of a problem, but it did not get big. The second flight, it got a whole lot bigger, but we did not change anything. We just said those were variations that would normally occur in a system. That is the reason somebody asked for more margin. We wanted a factor of two to take care of the variations that occur in engine structure, because each one of them is a little bit different.

After the second flight, we now had to implement something and we have to do it without causing a slip or a significant slip in the next flight. We got a group together and looked at whether or not we could use this gas injection. Somebody came up with the idea of the prevalve sitting above the engine inlet. That prevalve has a visor and we could fill that cavity with gas and make an accumulator out of it. The light came on, we put a little boss on the valve, put a little helium into it, and bingo, we had an accumulator. We ran ground tests on it, we ran the analysis on it, and we committed that to the next flight. It worked like a charm.

Johnson: When you describe it, it sounds like an easy fix, but there was some concern about the pogo problem at Marshall.

Worlund: It was a big concern. It turns out that our fix was simple, but to get to that simple solution, we went a long way. We looked at different types of things, and at the end of the day, we were able to find a simple solution.

Johnson: When the pogo problem cropped up, we were under a sort of time deadline at that point. Things were moving right along and they needed to. Did you feel pressure to solve this?

Worlund: Yes, we knew we had to solve it. I am not sure I should tell the story, but I will tell it anyway.

Johnson: This is for history. (Laughs)

Worlund: This is for history, and everybody knows it, maybe. It is kind of like an elephant, we grab it at a different spot. We were on the second flight of the Saturn V rocket. We had a pogo problem on the first stage. We had an ASI [Augmented Spark Ignitor] line failure on the second stage that cut one engine off. When that engine cut off, we had mis-wired the prevalves so that when the first engine went down, it shut the prevalves on the second engine. When the second engine went down, it went back and shut the prevalves on the first engine, which was already shutting down, and we had two engines out on the second stage. We did not think you could fly with two engines out, but we did, probably because we had built enough margins in the various systems at the time we were designing them. On the third stage, we had an ASI line on that engine too because it was the same engine on the second stage as it was on the third stage. It turns out that that ASI line also failed. It ran alright during the first stage, but we needed that ASI line to light the engine back for the second burn. Of course, the line had failed, so we could not restart the first stage. On the way up, one of the slaw[?] panels fell off of the stage above the third stage, it was on the payload. Every stage had some problem with it.

There was a big harangue. Dr. [William] Lucas happened to be lab director over in propulsion. He has had structure and propulsion. The problem on the first stage was his. The problem on the second stage was his. The problem on the third stage was his. He had three people out working three of these problems all at the same time. They all had to be solved because that was the second stage launch. We were waiting and had a third one already scheduled. There was a lot of pressure to get each one of those problems solved. The group I was working with, we were mostly working on the first stage, the pogo. Some other people were working with the ASI lines. The way I remember that is we were having a big meeting. The meeting was scheduled, I think, on Sunday, and Lucas had all of his groups. He was reviewing us. We would make our charts, go through them, and he would look at them, hear our presentations, and send us back to go get more data or to change it. I think I left there about eleven o'clock on Saturday night. Other people were still working when I left.

Our presentation, we had sent it back for the final wrap-up on charts. We were going to talk and explain the structure charts, why we had this problem, and I was to sit in the third row at the big meeting in case somebody asked a question. I liked Paul Harvey [Aurandt], so I had a little portable radio. I was walking in that morning listening to the portable radio because Paul Harvey had about got to the rest of the story. When I got out and started to walk into the building, a guy met me at the front of the building and said, "Lucas wants to see you." I said, "Okay." He said, "Lucas wants to know if you can make the presentation on pogo today." I said, "I will go talk to him."

I went up there and he said, "We planned to talk structures and all this, but what they are really going to be more interested in is what we are going to do to fix it. Take some charts out of the structures presentation and put it with the charts on how we were working to try to fix the stage. When you do the presentation, we want to talk fixes rather than define the problem." I said, "If you want me to, I can do it." He said, "When you do it, when you get through one chart, make sure you move right on to the next chart. Do not give them much time to ask questions. When you get to the end of your presentation, introduce the next speaker." I was prepped, I went up there, a guy went off and got the charts, and I went and sat down in the meeting. The charts were going to show up when my time on the agenda came. When I got to making the presentation, turns out I am pretty much a motor-mouth and was going right on through. When I got to the end, I introduced the next speaker, and Dr. [Arthur] Rudolph jumped up and said, "Wait, wait." I thought this was going to be a question I certainly was not going to be able to answer. He said, "I think it is time to go to lunch." (Both Laugh) That was the biggest relief I think I had ever had. It was a big deal. There were big people up there to do it. We were changing as we needed to address the issues that were coming up. You went off and did whatever you had to do to keep moving.

Johnson: This would be a good time to ask about the involvement of Dr. [Wernher] von Braun in your work. I would imagine the pogo problem, he was vitally interested in solving that. Did he visit your lab or visit your area? Did he talk to you guys?

Worlund: No, he did not. Dr. von Braun really had confidence in the people he had assigned to those jobs. He did not come down. We may have briefed him a few times, but I do not remember him ever coming down. I do have one story with Dr. von Braun. I do not remember exactly which launch it was, one of the early launches where pogo was known about, but we had not had too much. We still had the analysis that showed we had about one degree of stability. We were going down. I was not going to make the presentation. Somebody else was going to make the presentation. Bob Marshall [Spelling?], I think, was to make the presentation. We would take the NASA plane down and Dr. von Braun liked to sit in the copilot seat and take the plane off.

When we got up and leveled off, he got back in the back and sat in the right seat in the back and asked to see the charts. He was sitting back there, flipping through the charts. Finally, he said, "This chart will never do." We had a chart where we had the stability line, below the line you were stable, above the line, you were unstable. We showed up in time, and about 100 seconds into the mission, we were predicted very close to the neutral stability line. They wanted two dB, which we did not have. He said, "I want you to change the chart and cross out this area here below the line and we will say this argument is that this is the band of where we think our things will operate in. This upper limit here is the worst case, and it is probably much better than that. What we will do, when we get to the Cape [Canaveral, Florida], I will filibuster the meeting for thirty minutes and allow you time to change that chart."

We went into the meeting. He filibustered the meeting. I had the job to go and have the chart changed. I got the chart changed and got it back there. In those days, they had Vugraphs and you slipped them into the stack, our presenter was there, and I slipped the charts into where it was to go. When that chart came up, Dr. von Braun was right. It hit the fan. Everybody in the room jumped up and hollered and screamed about the lack of margin in that type of thing. It turned out they stopped the meeting, huddled around, and wanted us to go and bring in all the data that goes with this analysis. Somebody wanted to know where the frequencies came from, and I gave them the frequencies. They said, "We want to see them." I said, "I do not have them with me. I do not have them in my briefcase." The guy said, "You have a guy still at work at Marshall?" I said, "Yes, sir." He said, "Get on the phone and tell him to send it to us."

I went up there, got on the phone, talked to the guy. I started flipping through my briefcase and low and behold, I still had that chart in there. I had that basic data in my briefcase and I did not find it the first time I was looking through it. I went back in there and they looked at the chart. I think it was George Mueller, he looked at the chart. I showed him the data and said, "Here is where I think the frequency is." He looked at it and said, "No, I do not think that is right. I think that frequency is over here." He moved that line over about half a cycle per second. He said, "Go back and have your guys up there rerun the analysis tonight. Bring them in from wherever you have to bring them in." I had to call a guy in and he was duck hunting. (Both Laugh) He came in and ran the computers that night.

I was at the table in the cafeteria the next morning replotting that chart. As we replotted the chart that next morning, George Hage came in and said, "What does your chart look like?" I showed him what the chart looked like and he said, "Good. That will work. We could not let the chart you had yesterday get into the record." What that meant was we were ready to fly. We were willing to take the risk, but we had to have a clean paper trail. We flew and there were not any problems.

Johnson: That said, were you a little bit concerned?

Worlund: Yes, I was a little bit concerned. (Laughs) You have to understand my philosophy on life. My philosophy on life is that I think my job is to tell you exactly what I do with as much clarity as I can and give you the information and a recommendation. You are not required to take my recommendation. I am not the only input into your decision equation. There are other things that go into your decision equation. After I have told you what I think and everything I know and I think you understand what I have told you, it is up to you, you decide now. I am not worried anymore because I have done all I can do.

Johnson: Let us talk about the integration of all the different parts, systems. You were kind of working to see how things worked together to get the heat where it needed to be and solve whatever problems with fuel flow, pogo. How did it all fit together in the final analysis? Did it fit together about like you thought it would or better?

Worlund: In nearly every case, I think it went together pretty well like what we thought it would look like. What we did in most cases was we tried to build enough margin in the system such that if we were not quite right, then we would have some margin there. For example, early on, I was working the fuel pressurization system on the S-IC stage. We had a ladder valve, and you open one valve. We were supplying pressure out of a tank, so the tank had a constant value at 3,000 psi [Pounds Per Square Inch], and you are getting a certain flow rate when you open a valve. When the pressure gets down to 2,500, to keep the same flow rate up, I now have to have a bigger valve. You open a second valve, and when the pressure comes on down, I opened a third valve. Then there is a fourth valve. I put in a fifth valve because it does not do me any good to leave any pressure in that storage tank. I thought four valves would be plenty enough, but I put a fifth valve in anyway. That was my margin.

Johnson: Just to make sure.

Worlund: Just to make sure. What that did was say I take everything I have, put it into place where it needs to be, and maximize my probability of success. I think if you go back and look at what other people do, particularly back in the Saturn Program, everybody had some extra margin that he put into the system here because he was not sure exactly how good his analysis was, so he put a little more in. In lots of things, we were fortunate, we were lucky, probably, but at the same time, I think the engineers put margin in places they needed. Everybody had a little something, a little margin in there.

Johnson: How did you feel when the Saturn V finally flew with no pogo problems, heat transfer working like it was supposed to, even human beings aboard? How did you feel about that?

Worlund: I think there was a sense of satisfaction and a sense of pride that we had gotten that job done. I am not sure I am the best guy to philosophically answer that type of thing.

Johnson: But it felt good?

Worlund: Yes, it felt good and I felt confident it was going to work.

Johnson: Did you know when you were doing the work you did, like everyone else working in the Saturn V days, that you were making history?

Worlund: I did not think about making history, to be perfectly honest. We had a job to do. You go off and get outside the job and you talk to people. I go down and talk to my father-in-law, who worked heavy machinery in the Birmingham [Alabama] area, and they think it is a great thing. Every day, we were just working the problems. I did not have any particular feeling that we were great. We were working hard, we were working with a lot of really smart people all the way across the country, but I did not think we were doing anything other than the job.

Johnson: You got to do the job on several projects after the Saturn V. You worked on Skylab and you worked on some satellites. Generally speaking, you were working on heat transfer, heat flow, and how these particular pieces of hardware would handle the high temperatures?

Worlund: Actually, the satellite work was kind of fill-in work. It was not a large, extensive program. It was a small program. Skylab, we were doing some work with the occupancy of the Orbital Workshop, providing flow, ventilation, cooling, those types of things for the workshop. Other people around us were working the other portions of Skylab. You knew what everybody else was doing and you knew what was going together. I think we had a feeling of satisfaction and did not have any real concerns when we launched it. That does not mean everything we did was successful. Here again, you have to understand my philosophy. My philosophy of life a little bit relative to the job is you have to do everything you can do before you launch because after you launch, it is too late to do anything. You have to do it before you launch. I think I was pretty well able to work in situations that allowed me to do that. They allowed us to go off and do what we needed to do. If we said it needed to be changed, fixed, or do something, they let us do it. Or, in some cases, if you explained to them and they said they were willing to take the risk, in that case, I have done it the best I can do. We will go with it.

Johnson: Let us talk about the shuttle. What was your involvement with the shuttle while you were working? Was it still working on heat transfer, heat flow problems?

Worlund: I started out initially, yes. I started out with the External Tank working heat transfer, fluid flow, pressurization systems, feed systems for engines, and interfaces with insulation systems for both fluids and gasses. It was similar to the same disciplines that I had worked with on the Saturn.

Johnson: I bet there was a difference in money, am I correct?

Worlund: Yes, there was a difference in money, but I think we had a much bigger database of information and experience to work from. When we were in the Saturn Program, we did not have experience. In lots of cases, we did not know what was going to work, so we carried a couple of solutions along. When we got through that program and we were on the Shuttle Program, now I think we know what works.

Johnson: The Saturn V work actually set you up for the shuttle?

Worlund: Yes, it set you up. They were essentially the same types of systems. Your analytical models are now pretty good. You have this Saturn data to correlate the analytical models with, so now you are confident you know what heat transfer coefficients are. Any heat transfer coefficient, you do not know it but within about twenty percent, but you have validated those analyses and those approaches. I think we did not need as much money for lack of knowledge.

Johnson: Over the course of your career, especially moving into the shuttle, the use of computers would become more and more prevalent. Did that help you in your work to be able to do simulations without actually having to do hands-on testing?

Worlund: Yes. We have done a lot of that. That goes back, I think, to the comment I was making about our analytical models. Back when we were going along in the first parts of the Saturn Program, we did not really have an analytical model. You did it with a slide rule and a Friden calculator. You wrote the equations by hand and you solved them by hand. You had to make simple things and assumptions. During the course of the Saturn Program, computers began to come in and we began to develop analytical models. We led studies for people to do analytical models. We would then take those analytical models and do parametric studies with them. By the time we got to shuttle, we had a lot of model programs you could use on computers. You may have to change the computer they are on, but we had the basic program. We could take it and reprogram it to fit another computer system. We had a much better technical database as to what needed to be done and we had confidence in the analyses.

Johnson: Any of the data you came up with over the course of your career, the Saturn Program and then with the shuttle, is any of it still being used or are developments you were a part of still being used in the space program today?

Worlund: Yes, a lot of those models and that data is still available. I think you learn some things that do not go so much in the technology range. Technology moves along and allows you to make computations quicker, but the basic engineering data that goes into it is the same type of data. If you change material, you know what type of material data to get. You go in the lab and get that data for the new material and you know you are in good shape for going forward with it without as much large-scale testing. Though, we still did a lot of large-scale testing with shuttle.

Johnson: Did it stay as much fun as it was during the Saturn V days? Over the course of your career working first on the Saturn and then on the shuttle, did it stay fun?

Worlund: Not quite, for me personally. Part of it, though, is where you sit in the decision tree. I will try to explain that. Early on in the Saturn days, the Marshall Space Flight Center and the Germans were the people with the most experience with launchers. Therefore, we had more experience, we had more, in lots of cases, data, and we did a lot of the analyses and testing ourselves. We were kind of the judge and jury, in some cases, as to what we did and how we did it. When we went into the Shuttle Program, we went more away from the NASA thing as a hands-on doing things here, and we gave the design to the contractors, we put people in place to be the integration contractors over the two contractors, and we moved more into an oversight role. We still ran analyses and the type, but we were not responsible. We had to kind of force our way in. In lots of cases, it is very difficult to change. You could not say their concept would not work, necessarily. In fact, once you had their concept, you were in the business of making it work. We have some things that came up. We made poor choices in some cases that ended up requiring us to do a lot of testing and analyses.

Johnson: Which program are we talking about here?

Worlund: The Shuttle Program. Part of that, on the Shuttle Program, we made some ridiculous assumptions or wrote in ridiculous requirements. We wrote the requirements like the shuttle was going to fly fifty missions a year. If you are going to build a tank and you are going to build fifty of them a year.

Johnson: The External Tank?

Worlund: The External Tank, you are going to build fifty of them a year. Now you have to figure out what you are paying and how to keep the cost down. What we did was make some choices based upon the fact that you have a flight rate. It turns out our flight rate, I do not know that we ever got more than eight a year, and in most years it was less than that. We struggled all the way through the Shuttle Program with some things that if we had been told early on that we were going to fly eight or ten times a year, we would have made different choices than we did when you asked us to fly fifty times a year.

Johnson: Would the vehicle have been less robust? What do you mean when you say different choices?

Worlund: It would have been better in lots of cases. I have one that I think created all sorts of problems. The Inner Tank, when you get ready to have a tank, one of the things you have to know is how much propellant is in that tank before you lift off. It is absolutely crucial that you know how much propellant you have because you think you are going to make a certain journey. If you do not have enough propellant, getting part of the way there is not good enough. You have to get all the way there, so you have to know how much propellant you have. Because we were going to fly fifty times a year, we went with some sensors that were cheap. They were kind of hot wire sensors and when the liquid got to them, they would go from black to white, on to off. The problem being that that is one instantaneous location. What happens when you are loading the tank and it is coming up, when you hit that sensor and it says you are where you want to be, before I can send that signal off and stop the valves, the propellant is still flowing. Now they are above my sensor and I do not know where they are and I do not know how much they have. Now we have to go through all sorts of engineering and tests to try to figure out how we evaluate that sensor. That sensor is kind of cheap. If we had put a cap probe in there where it can measure continuously in that top section around where you want it to be, then you would know where that thing is and you would not have to do a bunch of testing and analysis. You could go out there and read it and know exactly where you are at.

Johnson: You wish you had known to do that.

Worlund: I wish we would have done it differently.

Johnson: You were there during and after the *Challenger* disaster. What did you do after *Challenger*?

Worlund: *Challenger* was about the time that my role was changing. I had progressed, been around long, other people had retired and gone on. Before *Challenger* had happened, I had actually gotten off of the Shuttle Program. I had gotten an assignment from [James E.] Jim Kingsbury to go over and try to develop a technology testbed, which at that time was really a name we gave for doing testing in the West Test Area. He wanted to put a shuttle-type engine over there and we wanted to use it for technology. He wanted me to get that facility up and running and define the technology that would go in there. I was off doing that when *Challenger* actually happened. I was sitting in the conference room and we were having a meeting about what to do. I think we were working instrumentation and how we were going to instrument it when somebody stuck his head in the door and said the shuttle blew up.

Johnson: In the Saturn V days, there were problems, but no problems that had anything to do with propulsion systems. You get to *Challenger* and something happened. As a guy who had worked making these incredibly complex systems work, did it take you aback? Was it shocking that something like this had happened?

Worlund: No, I did not think of it in those terms. Early on, I was working stage problems. The stage, we really did not have any problems. In my personal opinion, we had some glitches with hardware, but we knew how to design and make that system. The big issue on shuttle was the engine. The problem with the engine was we had changed because we needed higher performance. We had changed from a gas generator cycle to a topping cycle, which is a closed loop cycle. Instead of having two pumps, now we have one pump on fuel, one on LOX. Now we have two pumps on fuel and we have two pumps on LOX with a boost pump, so you have about three pumps that are all in a series. They have the pump and you are operating with chamber pressures. Instead of operating them down less than 1,000 psi, you are operating them at 3,000 psi chamber pressure. That means that upstream of the main injector, you are at about 7,000 psi.

What we really found out is that in addition to that, you want it to be reusable. At the time we started, we said we wanted to operate it fifty-five times. Now you start making decisions on the criteria you are giving yourself to go off and do. Reusability brought in a bunch of problems that we never thought about. It brought in a new set of problems with those things we reuse. In the early days, you did not even think because you flew it one time and it was gone. We did not think anything much about high cycle fatigue because you did not run it long enough to put in a whole lot of cycles. Now you are running it, starting it multiple times, getting a whole lot of low cycle fatigue with each start you go through the cycle. With every rev, you get a whole lot of high cycle fatigue. We would run into lots of problems we did not anticipate. I was not disappointed. It was like you were in the swamp killing alligators. You do not worry too much about the size of the swamp, you just try to kill that alligator. I would not say I was disappointed, but we kind of knew what the problems were.

The other problem we had with the engine is that we did not have any way to test it at less than full-scale. We did try to test the engine, the pumps, the preburners, some of that type thing here on a test stand, but we were told that we had to go off and modify the existing facilities. Budget was a problem, you did not have the money or the time. We tried to use valves, but, in order to get the pressures and flow rates through them, those valves would not move fast enough to simulate it on the ground. Our simulations were inadequate. Because of schedule, we ended up basically taking the hardware, putting it on the engine, and developing the hardware and the same time we were developing the rest of the engine. We burned up a lot of hardware.

Johnson: What I think I am gathering from the last things you have said is that you were not surprised there was an accident or that something went wrong.

Worlund: On *Challenger*?

Johnson: Yes, on *Challenger*.

Worlund: I was surprised at that. Everybody expected the problem to be on SSME [Space Shuttle Main Engine], but it was not. It was on the solid.

Johnson: On the much less complex system.

Worlund: Right. I was not surprised because, to be perfectly honest, I had very little to do with the Solid Rocket Motor before *Challenger*. In fact, I was in the meeting on Testbed when *Challenger* happened. Within a day, I had a guy come to see me and say they wanted me to come down and help with the *Challenger* investigation. They ended up asking me and I went down and assumed the role of the chief of the Propulsion Laboratory because the guy that had just been made chief of propulsion had gotten married and had prepaid a honeymoon to Africa. He was leaving and did not want to cancel his honeymoon, so they asked me to go down there and help with the *Challenger* investigation. I walked down there and a guy grabbed me in the hallway and asked if I wanted to see something. I said, "Yes." He said, "Come here, I will show you where the problem is." He took me into the photo room, pulled up the film, and showed me the puff of smoke that came out of the joint right at liftoff. We knew where it was at. We did not know why it was there, but we knew where it was at.

Johnson: You eventually figured out exactly what the problem was.

Worlund: Eventually we figured out what the problem was. I was the interface going back and forth between some of our analysts and the team who was putting the data together for the Solid Rocket Motor as well as the total stacking. I got to see a whole lot of that. It was a great learning experience for me because until that time, I really had not done much Solid Rocket Motor work.

Johnson: Looking back on the things you got to do, ranging from working on the Saturn V to the development of the shuttle, to working on satellites, to investigating the terrible accident with *Challenger*, is there a way to look back on your career and put it into perspective, how you feel about all the different things you were able to work on?

Worlund: My perspective of it is that through no effort of my own, I have worked in the program probably at the very best of times. I worked through it when we started from a very low technology base, develop all the hardware to go to the Moon, and then do the Shuttle Program. Every day was interesting. I lived at a great time in the program. It is that I happened to live at the right time, I did not do anything to necessarily put myself in that position. The good Lord just put me in that position and I lived in this point in time. Maybe lots of people do that. I remember sitting on the porch talking to my dad, and he said he looked back through his life and he thought he had lived at the most interesting time in history. Maybe I could say that too, except my time in history was probably more interesting than my dad's.