An Oral History of the X-15 and Langley 11" Hypersonic Wind Tunnel: Recorded Interviews with Mr. Jim A. Penland

Laura T. Freeze
NASA Langley LARSS Program
Summer 2007

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NASA Langley Research Center Oral History Release

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7-17-07
Date

Jim A Penland
Signature of Interviewee

Jim A. Penland
Typed or Printed Name of Interviewee

Interview with Jim Penland
8 June 2007
Laura Freeze

Notes from 8 June:
-Mr. Jim A. Penland started working at NACA July 1948.
-He had an interest in hypersonics and how to control vehicles and break barriers.
-Today Mr. Penland and I looked through the early 11" Wind Tunnel logbooks that later included the X-15.
-The first book began 26 November 1947, with testing of other aircraft models often around Mach 6.2-6.5 to calibrate the tunnel.
-Mr. Penland began observing the tunnel 16 July 1948: "Much of the testing was from scratch, except for those that tested cones, wedges, and boundary layer theories."
-Mr. Penland tested temperature flows in August 1948 and began operating the tunnel.
-3 August 1954—they began to go to wedge tails for the Mach 7 airplane (noted in an original 1954 blue research memo by Charles McLellan).
-Borrowed a lot of technology in X-15 from German and Japanese captured technology or aircraft—especially in a certain wing shape Mr. Penland and the engineers tested in the 11" tunnel, because they knew they already worked.
-Eventually 766 documents as a result or related to the X-15 by the end of the program in 1968.
-In his office:
  -Mr. Penland has a black briefcase with all his reports in two maroon colored volumes.
  -In left desk drawer: original blue research memos in regards to the X-15.
  -Top two shelves in his office are all related to the X-15.
  -Second to the top shelf—brown file box of conference reports and dimensional data in which Mr. Penland was interested.
  -Slides of a talk in Bonn, Germany by John Becker in 1968 examining the X-15 program.

Recorded Interview with Jim Penland:

START

Laura Freeze: When did you get wind of the X-15 and the design of a research aircraft?

Jim Penland: Because we had a Mach 7 tunnel, and we had shown that there did not seem to be any problems at running at Mach 7, other than the fact of the heat problem, no pressure problem, that they should shoot for Mach 7. That was when the beginning of the thoughts for the X-15 occurred. And I think that I have got the paper here that will give us some dates—"April the twenty-second of '54—study group consisting of [John] Becker, N.F. Dow," I don't remember him, "Max Faget, Tommy Toll, and the pilot, that was J.B Whitten, he was the pilot…compiles its notes and finding its recommendations, two copies, are attached… Objective: The next research airplane should be capable of providing advanced research information primarily in the field of aerodynamics, heating, heated structures, stability, control, piloting problems, over a broad range of supersonic Mach numbers and altitudes, including conditions of both very high and vanishingly small dynamic pressures… Speed range of up to about Mach 3 and altitude of 75,000 feet…"

LF: So this is what they were aiming for?

JP: Yes.

LF: [Looking at the NACA proposal from 22 April, 1954] This is what they wanted it to look like at first?

JP: This was the first cut. This was a research airplane that had a wing shape that would give the lift they thought was necessary; the body would have enough room for the fuel. This was sent out to all of the prospective contractors. In fact this whole volume here was sent out to them. And then each one of them came back with their idea of what the X-15 should look like. At the time this was written—we'll have to look in the logbook—I don't think we had made any tests. That's what those folks' names we were looking at came up with. Becker and Stack, on the supersonic and hypersonic side, and the pilot and Tommy Toll on the transonic and subsonic side. Their idea was that this configuration would give us no problems landing; give us no problems subsonic; probably no problems transonic or supersonic. And when we get up to hypersonic, which is above Mach 5, they don't know. This was their best estimate, and as you can see on the final version, the wing is still the same. That is because of the knowledge that it is not going to give us any problems until we get to hypersonic, which is what we're looking for.

LF: The knowledge that it [the wing shape] wasn't going to give you any problems, is that because you had already tested it in the wind tunnel?

JP: That had been tested before, and also there were some missile design and also some airplanes designed with similar wings, as far as the shape was concerned.

LF: Were those U.S. missiles and planes?

JP: Oh, yes, oh, yes. Plus the fact those folks—this had been right after the war [Second World War]—they had access to everything we had captured from the Germans and the Japanese. The Japanese had some pretty good aircraft during the war. And the Germans had more experimental type vehicles than did Japan. There were some rocket-powered airplanes, too, like the 163. It was a flying wing, but it was rocket-powered and it could shoot up real quick, and then it had to glide back down and land in the pasture. You realize during World War Two, Britain and Germany had very few paved runways—most everything was taken off of the fields. So all the Spitfires and the Hurricanes, in regard to Britain, and the Messerschmitt's and everything in Germany—they were using grass fields. The quicker they could get off the ground, the better…many accidents on landing with mud, etc.

LF: So perhaps some of these [American] technologies were building on those [German, Japanese, British]?

JP: Absolutely, absolutely. All of the knowledge for all of the research airplane program came about from what we knew, what we learned from wind tunnels, and what we learned from the Germans and the Japanese. Some of the research airplanes that we had, like the X-4 and the X-5, were copies, more or less, of what Germany had done, but we didn't have any data on them, and so we needed to know. So one of them, I guess it was the X-4, the wings swept back and forth during flight. And then the X-5 was an all wing vehicle. In that case, the only all wing vehicles that we knew anything about were what Northrop had made for transports, and so our X airplane program was a pretty direct result of copying and then trying to make a step further. This was the big step, and the big step was to go to Mach 7, for the simple reason that we knew from tests that there wasn't any real problem.

Here's Republic's and Bell's configuration. If you look, they are all very similar to our original test one here [memo configuration]. Then there was North American. That was the early one, and that's the pretty one. The North American; see the tail looks more normal; still has the same wing; and it still has the same strakes on either side. The diameter of this central core is greater on the final than on the initial one. That was to house more fuel. Here are tests that we started making in the 7 by 10 tunnel [to test launching the X-15 from the B-52]. It was a subsonic tunnel. The B-52, of course, was a subsonic airplane. It could go high subsonic speeds, but not past Mach .8, probably something like that.

The original tails that North American came up with were going to be a diamond cross-section, but they found out that the actuators to change the aft portion of the tail were going to weigh so much that it was going to throw the balance of the airplane off, because all airplanes have a center of gravity that is determined by the shape and the location of the wing. That wedge tail not only was required in order to make the airplane stable going in the same direction, not deviating, but we were able to measure pressures on that. And it was the first time that had pressures that were measured over a wide Mach number range.

LF: Does this relate at all to the wedges you were testing here [in the logbook in the late 1940s]?

JP: Yes, it was as a result of those tests that Charles McLellan, who was our immediate boss, came up with this particular tail [wedge tail used on X-15]. Notice that the tail on it [the memo configuration]; the tail was not a wedge. It started out a wedge, and then it actually at the center part of it could open up. Then we couldn't do that because of the weight of the actuators. And they ended up making it solid. It's hollow really, but it's solid as far as the air is concerned. The vertical tails were going to be unsatisfactory at Mach 7. It was the result of that McLellan said, "Okay, we've got to go to a wedge tail." And then he wrote a report—I found it. These are all 11" tunnel papers in here [left desk drawer]; other papers are located other places. This was done August the third of '54. It was at that time that it was decided that we have to go to wedge tails. He wrote this paper over the weekend. If you notice, all of the figures are still hand-done. But he got a 1000-dollar reward for this idea. This information was given to North American and they made a tail while the model was in the tunnel. They had technicians with them, and they actually made a tail and put it on the configuration. We tested it and it showed an improvement. So then they went back and designed one that was an increased area, oddly enough, with the value of angle that would give sufficient directional stability.

LF: Who decided to give you the contract?

JP: It was a combination of the NACA officials and the Air Force. It was their decision to go to North American from all of these configurations that each company had given. And of course there is a pack of data that comes along with each of those configurations. I didn't have anything to do with that. The final decision came that they would get it. It's my opinion that part of it was the fact that they realized that the volume of the fuselage that we had estimated here was insufficient to house the fuel, plus the hoses and the tubing and the electrical stuff. They were the only ones that really looked into the overall thing in a manner that said, "Gee, we've got to make it bigger in order to do the job."

LF: You had to make alterations later?

JP: Really the only alteration was the slight increase in the fuselage side diameter, in order to house more fuel, and those tails. Those were the two major changes after the configuration we looked at in here [the memo]. The major things were these tails, because they're bigger and they're heavier, and they're at the rear of the airplane. That is something that is difficult to manage on the design of any airplane, is to keep it from being tail-heavy. Because if it's tail heavy, you have no choice but to move the wing back in order to get the center of gravity where it should be. They did make some small changes in the location of the wing, but it turned out that that was to change the CG [center of gravity], it wasn't for any other reason. And with their coming up with configuration two—we called it—I think configuration two was the same as configuration one, which was the same except for the tail. And then configuration three was the increased diameter, the shifting of the wing ever so slightly, and these new tails. And then that was the final one we tested off and on through the flying of the airplane, on up to the last X-15 test in the '60s, even though the first flight was in '58, I think.

LF: Are a lot of these tests recorded in the logbooks?

JP: All of them, all of them… The main breakthrough after North American got the contract was the vertical tails. And the next thing was to increase the size of the fuselage ever so slightly for volume purposes.

LF: Do you know who would have [the original RFP]?

JP: Central Files, I guess, it's probably been photographed and it's available on film someplace…

END

Interview with Jim Penland
12 June 2007
Laura Freeze

__Notes from 12 June:__
-Received more material from the X-15 program to look through.
-Looked at two large display diagrams of the X-15 configuration on the top of Mr. Penland's filing cabinets.

__Recorded Interview with Jim Penland:__

START

Laura Freeze: Why was the B-52 chosen to launch the X-15?

Jim Penland: They needed something to carry a load. They had used the B-29, I guess, on the X-1, and I think on the X-2, but then they needed something a little faster, and the B-52 was the going machine at that time. And they used it on up until this X-43 that shot off a couple years ago. That was still the same airplane, and it's been retired since that time, I think. But we don't have any further test vehicles to be carried aloft, unfortunately.

LF: In the study group memo [NACA X-15 proposal from 1954] there were a variety of scenarios to launch the aircraft, and one of them was the B-52. Which other ones did they use to launch the vehicle later on?

JP: After they started to use the B-52, they had no other vehicle. They stuck with it on up through this X-43, which was a couple years ago. And that was due, of course, to the fact that it was high subsonic, because it was up .8, something like that, Mach number, so that saved a lot of fuel that would have been required to boost the vehicle up to fit speed.

LF: Why was the Reaction Motor's XLR99 engine chosen?

JP: Well, it was the one that was essentially designed for the X-15, and it worked horizontal. Most rocket engines are pointing down, but the XLR99 was designed and built to operate horizontal. And it was also throttleable. You could change its thrust, and that was one of the first rocket engines that we had anywhere that you could change the power. But it came about for the X-15. Now the first two flights were not with the XLR99; they were with engines left over from the X-1, X-2 days. I think they had three or four of them on there. So that was good for low Mach numbers, but then when the XLR99 came about, that was the main engine.

LF: So that was used until the end [of the program] also?

JP: All through the program, that is correct. And what they learned on that design was used for the Shuttle engines and really, I guess, all other rocket engines since that time, because of its ability to operate horizontally as well as vertical. That was something different. What they learned they could use in building others, let's put it that way. That was a big step in rocket design at that time. That was a new innovation.

There were a lot of new innovations on the X-15. For instance, the pressure suit that the pilots used; the design of that was used extensively in the pressure suits that were made for Mercury, Gemini, and Apollo. And that saved the space side of the house a tremendous effort to have that space suit. And it was designed as not only a NASA input, but the Air Force input. A college friend of mine was a medical doctor at Randolph Field in Texas, and he worked on that suit at that time period. I didn't know about it, I didn't know he worked on it until afterwards. I would say that was the first pressure suit that any of our astronauts used. And I guess some of the pilots of the X-15 got astronaut status because they went above 50,000 feet. So that was of historic interest.

LF: I was reading about the Inconel X…

JP: The skin, the sheet metal skin was made out of Inconel, which is a stainless steel.

LF: Weren't there problems with that?

JP: The problems were in fabrication, because they had made a lot of aluminum airplanes, but they had never made a steel one. And the stuff doesn't bend as easy, and it was difficult to conform to the shape. But it was necessary to not only to carry the load, but to carry the heat, because the X-15 was a heat-absorption vehicle. The heat that it gained in flight due to the friction… the only heat that it lost was that due to radiation. And the rest of it was absorbed.

It had a limited ability to stay aloft at those high speeds, and I was told by Joe Watts, who worked at Edwards, that everybody out there felt like the X-15 had done about all it could do, as far as speed was concerned, because the heat load would have been too great if somehow they could have sustained the flight for a longer period. He felt like they had gained about all the knowledge that they could from that particular vehicle, particularly in terms of the heat load and everything. Now on the X-12 and the Blackbird [SR-71], that came later, they were able to with heat lamps… they were actually able to put a heat load on it to see how much it would bend. That airplane bend, it got hot on the bottom and the nose would come up, the tail would do the expansion as much as 18 inches, and so the question under those circumstances was the stability and control. Would the pilot still have the ability to control the airplane if it bent that much? So there were tests run to verify that we had sufficient control.

LF: Just to clarify, what was your official title during this [X-15 program]?

JP: I guess I was just one of the aeronautical engineers. We really didn't have any clear title. That was essentially it.

LF: Were there any other things you wanted to add about the early stages of the program?

JP: I guess at the time we tested at Mach 6.8, 6.9, the same airplane was tested at Mach 4 here on the [Langley] Field, and also was tested subsonicly. They actually made some drop models and dropped them from a helicopter for spin tests. And they also ran spin tests on the X-15. They made two; one of them I feel heir to for display purposes, and it's in storage.

END

Interview with Jim Penland
15 June 2007
Laura Freeze

Notes from 15 June:
-Discussed his role in the early work with the X-15 program and in the 1958 X-15 conference.
-Discussed the 11-inch hypersonic wind tunnel.
-Saw an original model on his desk of an X-15 drag brake model, which was tested in the 11-inch wind tunnel from 1955 to 1956.

Recorded Interview with Jim Penland:

START

Laura Freeze: [In regard to Jim Penland's copy of the "Conception and Research Background of the X-15 Project"] Do other copies exist?

Jim Penland: I'm sure there must be other copies, but I have no idea where they might be. I don't think I have anything other than copies. I don't have any originals. I had some that were more original than what you have.

LF: Who wrote it? It said the staff members of the Langley Research Center.

JP: That would probably have been the, I would think, the division-level folks, similar people that were the conceivers of the X-15 idea. It was Becker, Soule, and Tommy Toll and that group, because they were all division-level folks, which is next to headquarters. One person at headquarters was in charge of all the division. At that period of time, Dr. Reid was the administrator of the whole lab. From him, it would have gone to John Stack, and then John Stack would have been over all the division chiefs.

LF: In one of these sections, it says, "Langley Investigations of Aerodynamic and Structural Problems of the Proposed Airplane in the Period June-December 1954." It said principal credit for the Langley work is due to...it lists a few names and your name is one of them.

JP: I'm lucky.

LF: What do you think, off the top of your head, were some of your major contributions in this early period?

JP: Everything I did in those days was connected with the [11-inch hypersonic] wind tunnel or testing in the wind tunnel or variations in the mechanisms that we used or the instruments that we used. That's really all I had any input on at that particular time. We tested some wings, which gave proof that the wedge was better than a flat plate or a contoured airfoil. I very well may have had some input in that, particularly designing the models and its selection in the tunnel and then running them.

Now, the way we operated. We had recording devices that had paper that ran through them and had pens that would draw a line depending on what the magnitude of the parameter you were measuring was. And then we had a group of girls that we called "computers" at that time, and most of them were math majors, either in school or they had been very astute in high school. Then they reduced that data. They read it off the chart and gave a magnitude and then computed the coefficients, whether it was lift or drag or pitching moment or yawing moment or whatever. Then after seeing their results we would then, in turn, possibly go back into the tunnel to make additional tests or make alterations to the model for additional tests.

In some cases we ran different pressures in the tunnel, in order to measure the various instances of Reynolds number. We had a constant Mach number tunnel, so the Mach number was constant, but the Reynolds number varied depending on the pressure level. In retrospect, I don't think we did that enough, but at the time period I guess it wasn't really necessary. Most everything we ran at a relatively high Reynolds number, because we could run the facility about a minute. One minute would give you a chance to move the model from zero angle of attack up to maybe ten degrees, twelve degrees maybe, and back down to zero. You would get maybe ten points in there, maybe zero, two, four, six, eight, ten, twelve, something like that. That would be what, six or seven points? Then that would give sufficient variation in the data to plot it up with angle of attack or angle of sideslip or whatever. That was about my total input at that point. I was in the process of asking questions; nobody asked me any.

LF: What was it like to work with the other engineers at that time in the tunnel?

JP: Well, all of us were new, so it was a learning process for all of us, really. The only person in the group was the boss, that was Charles McLellan at the time, and he was the only one who had had several years of wind tunnel experience. He had a big input into the design of the tunnel and the facility and the pumps and the dryer and the temperature measurement equipment and everything. And then we all worked very closely with the IRD, which was the Instrument Research Division group. Then they had a special group that had to do with pressure, and then another group that had to do with force measurements, and another group that had to do with balances. These balances that we had were rods that were milled out such that they put string gauges on them, and as you would go to various angles of attack, you could measure what the forces and moments were with the balance. The balance generally had six components. We'd have normal force, axle force, and side force. And then we'd have pitching moment, and we'd have yawing moment, and we'd have rolling moment. So there were six parameters that we measured on the model.

Then the wind tunnel would be set to run at a certain pressure and a certain temperature. That could be varied also. If you varied that, the pressure or temperature, well then you would get a difference in Reynolds number, but you still had a constant Mach number. Now that's not true of all wind tunnels, but it is true of all hypersonic wind tunnels. The only variation in the Mach number in the tunnel is due to this boundary layer buildup on the sidewalls, which would tend to make the test section a little bit smaller at low pressures. Now if we ran at the very highest pressure that was possible, then our run was shortened. And so we couldn't get a complete angle of attack test range. But generally speaking, we ran less than maximum. Roughly 30 atmospheres, if I remember, was our normal high pressure, even though we could run at 33, maybe 36 atmospheres of pressure. Most of the runs were a routine thing, really, once you get set up. Then the mechanics, our wind tunnel technicians, would vary the controls between runs, while we were pumping up the pressure tank, and evacuating the vacuum tank. We would open up the window and actually check the model to be sure it was at the correct angle and nothing had slipped or anything.

We actually set the angle of the model during the run by having a mirror embedded in the side of the model. And then we had a point source of light that shined into the window, hit that mirror, and came back out and showed itself on a chart. We would actually calibrate—we would move the angle one or two degrees—and then mark it on this chart. So you would have a line for zero, a line for two degrees, four, and so on. And then during the run somebody would actually change the angle of attack of the model, during the run. With this little beam of light he could set it on the angle that was necessary, going up to the maximum, and then come back to zero for stopping the facility.

They wouldn't let you do that today. They wouldn't let you sit down with your head up against the window and do that for safety reasons, but we got by with it then and nobody got hurt, even though we did break some glass one time. It didn't fly out of the frame; it just cracked.

LF: Why did it break?

JP: A model broke and went over and hit it. The glass in the hypersonic wind tunnel is something special. It's not just plain old window glass. To start with, it's about an inch thick, and it was made out of quartz. The best glass we got, I think, was made in Germany. And they somehow deposited the glass on a plate somehow, maybe it went in there in an evaporated state, I don't know, but their glass ended up, once it was polished smooth, gave the best image of any glass that we had. At one time those windows cost more than gold. Now gold was only 35 dollars an ounce at one time; it's 600 dollars an ounce now. But when it was 35 dollars an ounce, those windows were worth their weight in gold. We were extremely careful with them, and you didn't dare touch it with your finger, because you would put oil on it and it would collect dirt. When we would clean the windows, all you would do would be to spray them with a cleaner and then take fresh clean, absorbent cotton and swab it off. And don't rub too hard.

I can remember one time Dr. Reid came over and he was up looking into the wind tunnel and he started to put his hand on the window. And one of the mechanics grabbed it! He jerked it and said, "Don't touch that!" Of course Dr. Reid was real shocked at what had occurred, but when he understood the reason, it was no problem. He was the first engineer NACA had, soon after World War One. I don't remember the exact date, but then he was the head of the whole Laboratory here on until his retirement in 1965, or something like that. Everything ran according to what he wanted. He didn't mind calling you up. I remember he called me up one time—I had only been to work [at NACA] for a few weeks. I had been asked to write a memorandum in regard to the purchase of some of these windows for the tunnel. This outfit had done a poor job, and so I let them know it in this letter I wrote. And it got up to Dr. Reid, and so I answered the phone—it rang—and it said, "This is Reid speaking! Jim, we cannot send a letter out like that, we've got to make some alterations." That was my first time I ever talked to Dr. Reid. He was talking to me; I was listening. But he was a very nice guy. We turned out to be very good friends, because he was a competitive rifle shooter. I took up the hobby, in fact I was captain of the rifle team here for 20 years, and when we would have a match somewhere, Dr. Reid would be there, well then I would compete with everyone else. We generally had five to ten people that would fire. And we had five or six different teams on the Peninsula. We fired once a week. We had a range here on the [Langley] Field. They did away [with the range] after Dr. Reid retired.

LF: You had presented a paper at the [1958 conference on the progress of the X-15]. What went in to the research and what was the experience like?

JP: Well, I guess that was the first conference I ever presented a paper in. So that was a new experience. Plus I got to travel to the West Coast, go through the [North American Aviation] factory and that's when I sat in the [X-15] airplane—it was under construction. It was secret, too. That conference was secret. I guess the big thing was that here were all these notable folks down here from industry, and from Air Force and the Navy and from everywhere. So it kept you on your toes, to be sure you didn't foul up. Fortunately they didn't ask me questions I couldn't answer. I was lucky, because there was a guy from our Washington office who, during the rehearsal, asked some very pointed questions that we didn't have answers for. I know Dave Fetterman and I and McLellan, worked from dinnertime 'till bedtime trying to come up with answers. But fortunately this guy got called back to Washington, and he wasn't there for the conference.

It had to do with the theory. We had theories for simple shapes like cones and wings, but not combinations with the wings and bodies. We had no theories to make estimates. Some of the graphs that I presented in that paper, I think there was theory on some of them.

LF: When did your involvement with the X-15 program conclude?

JP: I was involved with initial tests on the three configurations. Once that was done, then I had very little to do with the X-15, from roughly the last ten years, through the '60s. Now I attended the conferences and kept up with all the plights and trials and tribulations. I didn't do any more wind tunnel testing. Now a guy by the name of David Fetterman, you may remember his name, I went on to another facility, and he stayed on at the 11 inch tunnel. He made some additional tests, I think, during the '60s, but I don't know if they were ever reported. A lot of times on projects like the X-15 project, you may do wind tunnel tests, but they may not get reported in a report. A lot of the instruments that we used in the 11-inch tunnel, we borrowed from Edwards [Air Force Base], because they had the ability to measure pressures and record them on film, that we had no other instrumentation to make those particular measurements. We borrowed the instruments from Flight and would run them in the tunnel. And every now and then we'd have to pull one out of the tunnel and ship it back to Edwards, because they had installed it in an airplane or something.

END

Interview with Jim Penland
19 June 2007
Laura Freeze

Notes from 19 June:

-Took a picture of the North American X-15 model and drag brake test model from 1956.
-Looked through the first section of the 1956 conference and the relations to Jim Penland's work and the work in the 11-inch hypersonic wind tunnel at NACA Langley.
-Looked through his two bound volumes of his research memorandums from 1946 to 1975.
-Looked through several research memos in his left desk drawer.
-Was given the "X-15 Research Results" booklet from 1965 to look through.
-Noted that he worked primarily on research airplanes throughout his career.
-Two papers from his bookshelf entitled, "Langley Working Paper-343: Stability and Control Data at Mach Number of 6 of a Preliminary Configuration of a Delta Wing X-15 Research Airplane," by Theodore J. Goldberg; and "Hypersonic Aerodynamic Characteristics of Two Delta-Wing X-15 Airplane Configurations," by Theodore J. Goldberg, Jerry N. Hefner, and David R. Stone from October 1968—NASA Technical Note D-5498.

Recorded Interview with Jim Penland:

START

Laura Freeze: I'm just going to ask a couple questions from the 1956 conference, the "NACA Research-Airplane-Committee Report on Conference on the Progress of the X-15 Project." The first one [section] is called "Review of Technology Relating to the X-15 Project," by John Becker. A few key points: NACA initiating a study in February 1954 to study hypersonic speeds on aircraft. That was the study group, right?

Jim Penland: Yes.

LF: He identified heating as the primary problem of the structural design.

JP: That was major, because it was unknown. We knew theoretically what the heating was supposed to be, but we didn't really know exactly what it was, how much it was, and how to design for it. Fortunately, the folks at North American [Aviation] did a pretty good job, really. The use of the stainless steel was the major innovation, because they knew it would take the heat without failure. There was no other material; no aluminum-type material would have withstood the heat.

LF: That was the Inconel X?

JP: That ended up being the Inconel X. Yes.

LF: Can you describe how the 11-inch [hypersonic] tunnel was involved in any of this, or were they?

JP: We didn't do any heating measurements on the X-15, per se. Heating measurements were made on cylinders and wings separately. Then that knowledge was applied to the design of the X-15. We never tested the X-15 itself for heating capabilities. The X-15 was a heat-absorption vehicle. In other words, it had no means of cooling, other than if it got real hot, then it would radiate heat. It would also lose heat due to conduction, if the outside temperature was cooler. When it was up flying at Mach six, six and a half or so, it was absorbing heat all that time. So that really limited the time that it could fly at high speeds. The rocket engine and the propellant load was such that it never exceeded its heating absorption capabilities.

LF: So from the beginning you knew that you wanted it to be a heat-sink structure?

JP: I really don't know who decided that, whether NACA decided that, or whether that came in the study by North American. I honestly don't know who decided that.
LF: But that was decided early on?

JP: Yes, yes that is correct. There was no additional material added to the airplane to absorb the heat. That was required structurally to contain the aerodynamic loads... They certainly didn't add any weight to the airplane for heat absorption capabilities.

LF: The NACA gave the requirement that there would only be three years allowed for design and construction. This seems, that compared to other developments for aircraft, this seems like a short amount of time for something pretty new and different.

JP: I guess at that time period that was not unreasonable. Today I think if we started from scratch, it might take longer than that. There's more involved than there was in that airplane, too. I have no idea what set that limit on time period. Most contracts are limited; they're not unlimited. The Air Force may have very well set that, because Air Force money was one of the big contributors to the overall project. I don't think I've ever seen a breakdown of how much the NACA paid, and how much Air Force paid, and how much Navy paid, etc, etc. There's probably a listing of that somewhere, but I have no idea where.

LF: John Becker's paper also examined the general main features. Can you think of some of the main features you and the 11-inch tunnel were involved with at that point?

JP: Really only the stability and control and the general lifting capabilities of the airplane. We were almost 100 percent aerodynamics at that point.

LF: If they tested, say, the stability and handling and something didn't perform quite like they wanted to, did that ever happen where they sent it back to you and you had to reevaluate the structure or design?

JP: Well, the vertical tail changed, between configurations one, two, and three was as a result of the wind tunnel tests. That was the major change to the overall airplane design as far as we were concerned. The minor changes due to the variation and the location of the wing, and the diameter of the fuselage and everything was more something that the company [North American] came up with, because they didn't have enough volume in one case. The wing, they moved it, in order to change the center of gravity. When they first moved it, I was under the impression that it was done for aerodynamic purposes, but I later learned that it was center of gravity purposes.

LF: In the 1954 study by the NACA, one of the scenarios was to launch the X-15 from the B-52. I know that is what they eventually did, but here in 1956, North American was saying it is to be air-launched from a B-36. Why did they think of that for just a little bit, and abandoned the idea again?

JP: I don't know, to tell you the truth, why. I don't know. They definitely made the right choice. For one thing, the B-52 was faster, and it would be launching the test airplane at a higher speed. That certainly would be desirable, that would save rocket fuel in order to accomplish that. The B-36 was strictly a subsonic airplane, and the B-52 was almost transonic, because it was, I imagine, pushing .9 Mach number.

LF: Here's another paper from a man at Langley. It was entitled, "Aerodynamic Characteristics from Wind-Tunnel Studies of the X-15 Configuration." And it was Herbert Ridyard. He was talking about the exploratory wind-tunnel test program that was initiated in January 1956 to study the aerodynamic characteristics of the X-15, and how the models were also tested in eight different facilities, with a broad range of Mach numbers. Where did Ridyard work?

JP: The 11-inch. The papers that we looked at, they're in that suitcase down there, of the initial airplane—the one that Tom's got on display—Dave Fetterman, Herbert Ridyard, and myself were the three engineers in the 11-inch tunnel that did all the testing on that configuration, and all the various tails. Then the same model was tested at Mach 4 in the East Area. Gee, I can't remember who all tested that, but there was I think a guy by the name of Alman was one of them, I don't remember who else, but there were others that tested the same airplane at Mach 4 that we tested at Mach 6.8. All those data went to the contractors. Then when we got the X-15 model, it was also tested at Mach 4. Bigger models were tested at the 8 Foot Tunnel under transonic conditions and at Unitary Plan Tunnel at Mach numbers of one and a half, two, three and a half, four, something like that—a series of Mach numbers. That was a big model; heavy as all get-out.

LF: Another one is, "Some Calculations of the Lateral Dynamic Stability Characteristics," by Martin T. Moul.

JP: Marty Moul.

LF: In his paper, it showed the calculated lateral response characteristics of a configuration without dampers for two speed-brake conditions. What do you think he was trying to accomplish here?

JP: The airplane ended up using a stability augmentation system, which I really don't remember the extent of, but it would have had the dampers in it. Marty's calculation would have tied the aerodynamic forces down to the geometric characteristics of the airplane, the weight distribution and everything. He worked in the, I guess it was the Stability and Control group that we had here on the Field. He was more of a theoretician, mathematician sort that tested for static stability—that's what you do in the wind tunnel. Dynamic testing for stability can be done in the air. There are some experiments that can be done in the wind tunnel.

The X-15 was not completely unstable without the dampers and other systems, but one of the very first flights that [pilot] Scott Crossfield made, he came in to land, and he was pitching it up and down. I guess that's called pilot-induced oscillations, where the airplane wants to do one thing, and you can't follow it quick enough with your hand to move the controls to keep it straight and level. Due to his abilities, they didn't tear it up, but as he slowed down, well then this oscillation quit, and he was able to go on and make the landing.

LF: This one is by John Paulson. And it's "Flight Characteristics of an X-15 Model at Low Speeds." It said a low speed stability and control investigation has been made with the 1/7 scale free flying model representing configuration number one of the X-15 airplane. The purpose is to aid in the evaluation of the use of the horizontal tail for roll control, which is unique to the X-15.

JP: That's true. They actually use it for roll control. Ordinarily they use elevons on the wing, but in this case they used the horizontal tail. It went, like so, for up and down, and then it rotated like this to give you roll control. I don't know if it had been done before the X-15 or not. It's been done a lot since. But that was an innovation that North American designed into the airplane. It probably saved a lot of weight. That's one thing in the design of airplanes—weight is always a major problem. The weight is extremely important. I suspect that that's the reason that they did that, to keep from having elevons on the wing, which would require additional servos and cables or whatever. But you know, I don't know that North American told us why they did things. We didn't ask. At least I didn't ask, or didn't think to ask.

LF: Were there many questions being asked between North American or the NACA or the Air Force, or did everyone do what they were told to do, without much question?

JP: If someone asked a question, they certainly would have got an answer. I don't know that either we, or the Air Force, went to North American to ask questions. I don't know that, whether we did or not. It could have been on an individual basis, like Marty Moul and maybe John [Becker] may have asked them something. I don't know. It would have been nice if they had written it down, had they asked and gotten an answer to a question.

LF: I know the third configuration [of the X-15] had the delta wing, and they tested it, but they never actually built it and flew it.

JP: That is correct. I can show you some pictures of that. It had an entirely different wing, and I think the fuselage was a little bit longer, too. There was quite a few tests made here.

LF: Were you involved in any of these tests?

JP: No.

LF: Do you know why it never flew?

JP: They never funded it. It didn't have any money.

LF: They ended the program. Were they just concentrating on other things they wanted to?

JP: Well, the space business took over. And that's where all the money went. In retrospect, there have been some major redesigns of the structure for this Delta wing X-15, because it was supposed to fly up to Mach 8, if I recall.

LF: Who came up with this configuration?

JP: I don't know. North American, I assume. If I recall, the 11-inch tunnel had already been given to VPI [Virginia Polytechnic Institute]. What was the date on that paper?

LF: '69. I think the tunnel was given… in 1972.

JP: This was done right before. I don't think any tests were made in 11-inch, but I don't know that. I was not in the 11-inch tunnel at that time. I was over in Continuous Flow Facility. It's a shame that it didn't materialize, because we would have learned a heck of a lot that we could have applied to the [Space] Shuttle. For instance, that delta wing X-15 had tip fins. And we learned later on that at hypersonic speeds, tip fins are not the best. They're good for stability, but they also cause control problems. We would have got some flight numbers, if it could have run that… I'm not sure whether the delta project was initiated by the Air Force or NASA, or whether it came from North American. I don't know. I wasn't on that part of the project.

LF: You said the drag brake model [on his desk] was tested from '55 to '56 in the 11-inch tunnel?

JP: Yes. This is the only one. I guess that's the smallest drag brake we had. We had two bigger ones. Then there was one that was just a cylinder. I don't know what happened to the other parts of the model; they all disappeared.

LF: Did you find they worked pretty well?

JP: Yeah, they did satisfactory. They were never used. What they did was, once they put these details on here, then this part right in here opened up. So that was used for the drag brakes. The idea may have been used on some other missile or something. I have no idea. At this time period, Dave [Fetterman] and I found out that the more reports that you wrote, the better chance you had to get a promotion. So that was one reason that when we got an idea for a test, we would go and see if they'd let us do it. And in this case, they thought it was a good idea, so we went ahead and ran the tests and published a report.

LF: [Looking at the report] March 19th, 1956.

JP: Yep, and that was right after we were testing the airplane prototype… [Flipping through pages] This was on the beginning of the space program… Mercury. Here are the configurations that we tested.

LF: So you were a part of that?

JP: Yeah, in the very beginning.

LF: Did they start that in the 11-inch tunnel? Where did they test that?

JP: Well, it was tested everywhere. They even made some models and threw them off the roof of the building to see how they would act subsonicly. At the time the space program came about, the Russians, you see, had already, they hadn't put a man into orbit yet, but pretty close, so everybody was real ginned up, "Man, we've got to catch 'em," you know. So everybody was working on it. Each tunnel, if they could cook up some project that was applicable, they would test it or publish the data.

LF: So this was the late '50s?

JP: ...'58, yeah.

LF: Did you all drop what you were doing with the [X-15] aircraft, and then focus on this?

JP: You see the X-15 was the only aircraft that we had, that we worked on. If something came up, then we would work on it. And then here were some airplane-type configurations that were candidates for space jobs. Now for the configuration two [of the X-15], they actually opened up the back of the rudder for the speed brakes. Now this is the method used on the Shuttle even today. The tail of the Shuttle actually opens up in the back for speed brakes. That also makes it more stable, like having a parachute back there.

END

Interview with Jim Penland
21 June 2007
Laura Freeze

Notes from 21 June:

-On his top shelf, we looked at a wooden box, with the original X-15 test model from 1954 to 1956. The model fits through slots on the top shelf of the box, with an interchangeable aft portion. In several partitions at the bottom of the box, there are a couple dozen fins that were placed on the model.

-We again looked through some of the original green 11-inch hypersonic wind tunnel logbooks from 1947 until 1971.

-Next to that box of logbooks there is a box of L-301 aircraft data from the 1970s.

-We looked at a big cardboard folder from his top shelf that had copies of diagrams measured around 1993 on the X-15 in the Smithsonian. He and another man went to the Smithsonian and measured the landing skids of the X-15, because no one had any record of them.

-We compared pictures of other aircraft with the X-15.

Recorded Interview with Jim Penland:

START

Laura Freeze: These [pictures] are airplanes, or like the Space Shuttle, most of them came after or during the X-15 program. See if anything was used on these that came about from the X-15.

Jim Penland: Okay. Let's see, I would say on the B-70, no, because the B-70, the construction of it was at the same time that the X-15 was being built. When I visited the [North American Aviation] factory in 1958, the B-70 was behind a screen and I couldn't even look at it. When I went through the factory, the guy that guided me said, "That's the B-70 project over there. We can't go over there." And I had no idea what the hell he was talking about. I didn't know what the B-70 was at that time... Now the HL-10 came after the X-15 and I guess I would say that the technology we learned on the X-15 was utilized in that design, but it was a lifting body, whereas the X-15 was a winged airplane.

LF: Can you think of any specific things with the technology or the structure that was similar?

JP: I guess the drag brakes. It [the HL-10] had drag brakes on the tail. That was really one of the few similarities, because this lifting body concept was a whole new concept, compared to the wing business. It came about from the fact of the Mercury capsule. During the period of time that the Mercury capsule was being designed, there were folks that thought we ought to have a winged vehicle to go into orbit, and to come back very slowly and alleviate the heat by not getting so hot in the first place and coming back very slowly, whereas the Mercury came back like gangbusters and got extremely hot and had ablation to absorb the heat. This was after that. The idea was to have a research airplane that would possibly even go into orbit and then come back. Now they made some low speed versions of this airplane and dropped it, but it never exceeded a few hundred miles an hour.

Well, that winged reentry business, that's where this hypersonic glide model came from. And that Blackbird [SR-71], I had nothing to do with the Blackbird, and I had nothing to do with the [Space] Shuttle. Again, the vertical tail opened up in the back like the X-15 originally was designed. That was used not only to slow the Shuttle down, but to improve the directional stability. Now, this X-24C was an outgrowth of this HL-10, and there was one or two other lifting bodies at that period of time. And then the Air Force came up with the X-24C concept. The X-24A and X-24B looked more like this [the HL-10] than they did this one [the X-24C]. Then I worked on this extensively. Most of the data is packed up over there [in the filing cabinets]. The detailed data was never published. I guess we modified the design to improve the stability and L over D and everything, but it was primarily an Air Force concept. We got right up to the deadline, and then they didn't fund it.

Now I had nothing to do with the Blackbird. It went up to Mach 3 or so. It was tested here on the [Langley] Field, but it was all secret. The thing was flying before anybody knew that it had ever been tested. And the Space Shuttle, I guess it utilized the heating studies that were made on the X-15, and of course the stability and control. Incidentally, the Shuttle at one time at tip fins, like the HL-10 here, but we found out that they weren't satisfactory. Control-wise they fouled up the stability.

LF: Didn't you say they were calibrating the [11-inch] tunnel for about the first year?

JP: Hardly anything but calibration, that's correct.

LF: So '47 to 48?

JP: Yes. I would have to look through the logbooks, because there were three different nozzles that were tested. The first one was real thin and expanded out to about 11 inches. Then it expanded vertically and that was completely unsatisfactory flow because the boundary layer built up. It built up all on this little small tunnel, and then after the final flow, built up even additional. Then the second tunnel was built, which went from a little slit all on to 11 inch by 11 inch. It was made out of ordinary, I think, 301 stainless steel. That stainless steel works rapidly with change in temperature. So that meant that this little slit that the air went through, it actually expanded. It ended up that the Mach number changed so rapidly in the test section that it was unsatisfactory for test purposes. Although I did run a whole bunch of cones and cone cylinders, if I recall, in that tunnel. We could only use the data, say at the 60-second point. We would go 60 seconds and then read the data. The tunnel seemed to repeat itself at that time period—to do the same thing every time.

And so then the third tunnel was built. The walls were made out of a metal called Invar. The Invar does not expand or contract with changes in temperature. It's the thing that's used to go through the glass and the filaments for light bulbs, because if it expanded when the bulb gets hot, it would break the glass. But then they use Invar for that, and it doesn't. So there was a source of Invar, but to get plates big enough to make the tunnel was very expensive. I think they had some trouble welding it also, but they learned how to do that. Then that nozzle is the nozzle that is on display at VPI right now. That one was never superceded. The only other nozzle we had was for Mach 10, and it started as a square hole, rather than a slit. It expanded sideways and vertically. It again was roughly 11 inches square at the test section. That was decided by the contour, or shape, of the nozzle to give you parallel flow at the test section at the Mach number of interest. In the case of the one with the slit, that was Mach 7, and in the case of the one with the little square, was Mach number 10.

We had another couple of nozzles that were built strictly for helium. We used helium in lieu of air for the gas. That was so we could get a higher Mach number. I think we were shooting for 18, Mach number 18, with that. But those were strictly experimental tunnels, and they're in the logbook, too, of course. Nothing was ever tested in them; they were just more or less proof of concept

LF: What is this box?

JP: [Looking inside the original X-15 test model box from his top shelf]. This body here slips into this port. The one that Tom's got [on exhibit] went in this one right here. This had the wings. We only had one set of wings. I had that in the showcase. It was the prototype X-15, Mach number of 4.06 and 6.86. The full contract. Frequently, particularly models this elaborate, the shop would make up the boxes. They did that, in order to protect their efforts.

[Looking at cardboard folder from top shelf]. Let's see what we got here. They sent us to Washington to make measurements on the X-15 that's hanging in the Smithsonian for these skids, because the X-15 landed on skids. It had a nose wheel and then it had skids on the back. Well, we had no drawings of these skids, so they gave us a car and we went to Washington just to measure. This was part of that. Then this is a copy of some of the stuff that I got at the Smithsonian of the X-15 and how the internal plumbing was put together and all that kind of business. But at one time that was the best drawing we had of the X-15. There's data on the X-15 at Mach numbers of three, five-five, five-seven, and Mach six. This is data now from the real machine measured in flight. These are copied out of a report. Here's lift versus drag for the various Mach numbers. You see, what we were going to do was to put the geometry for the X-15 in the big computer and see with what accuracy it could be calculated. And this was the data and we were going to compare them.

LF: But it never worked out?

JP: Well, the guy who was an expert on running the computer, he was the one who he and his girlfriend fell out, and he moved to Florida!

END

Interview with Jim Penland
28 June 2007
Laura Freeze

Notes from 28 June:
-We discussed the origins of the 11-inch tunnel.
-We looked through more in depth the first 11-inch hypersonic wind-tunnel logbook from 1947 to 1951.
-He gave me original Schlieren photos from the tunnel and of the X-15 to look through. The numbers of the runs are listed on the bottom of each photograph.
-We looked at one of his papers that was never published, entitled "Flow Visualization," written June 5, 1952.
-I was given the landing skid diagrams of the X-15 to copy.

Recorded Interview with Jim Penland:

START

Laura Freeze: Can you talk a little bit about how the 11-inch tunnel came about; how it originated?

Jim Penland: I think John Becker is the one who, to my knowledge, he was the one who was responsible for the original concept. And he was also the division chief of the organization that designed it and followed through on the contract and calibrating it. Some of the new guys came in just in time to do some of the testing, because the tunnel first ran in '47, late '47. Sometime in 1948, we changed the nozzles to the second nozzle, which was a single expansion nozzle. Once we put the Invar nozzle in, the only changes that were made to the tunnel were we put in a new heater to heat the air. And we put in the second high pressure tank, so there would be a larger volume of air. And we put in a larger vacuum tank to accommodate this additional air. That was the way it was run from roughly 1951, on until 1972 in that condition. The only changes we made were was there was an improved Schlieren system in order to see the shockwaves and everything. But those were optical mirrors. We got improved windows, too. Windows were always a problem, because when you shine the light through to see the shockwaves with the Schlieren system, they show up any deviations in the glass. And so it was very difficult to get windows for the tunnel. These windows not only had to be clear, optically clear, they had to withstand heat. So even Pyrex glass would be insufficient. It ended up that we got glass that was called Homocel that came out of Germany. I don't know precisely how it was made, but I understood it was a process where they built the glass up from a hot gas. As a result, it was striation-free. You could hold it up to the Schlieren, move it around, and you couldn't see anything. It was beautiful, and it cost a beautiful price.

LF: Where did they get the technology for the tunnel?

JP: Well, you see, supersonic tunnels had built themselves, had grown, on up. There were other tunnels around in order of Mach number 2 and 3, some pushing 4. The idea of building one for Mach 7 was generally based on what the estimates were that you could get the right pressure and the right temperature. That was essentially the reason for Mach 7. Now we did make a Mach 10 nozzle and run it successfully. It took a higher pressure ratio. Our heater was good enough for Mach 10, but we could not go any higher. It was not a hot enough temperature for anything else.

LF: On the first page of the logbook, it says "Project 506." What was that?

JP: In those days, everything that was done was secret. So they gave it a project number, rather than saying this is an 11-inch hypersonic tunnel, it was Project 506, which happened to be a Mach 7 tunnel. The secretive nature of the project was the reason for having that name. Now why it was '06, rather than 504 or 507, I don't have any idea.

LF: Did they keep calling it "Project 506?" Or just for the start of it?

JP: Once we moved from the East Area to the West Area here [in June 1950], then they started calling it the 11-inch hypersonic tunnel. By that time, it had already been running for a couple of years. I think some reports had come out on the development of the tunnel, and as a result, the cat was out of the bag; everybody knew we were working at Mach 7. The "11-inch" came from the size, of course, and we stuck with that.

LF: So you all had to be pretty secretive at the beginning?

JP: At the beginning, I couldn't even tell my wife what I was doing at work. It didn't bother her, but it bothered my dad. I couldn't tell him anything. Unfortunately he died before I could tell him anything. He was only 60 years old. Even the first paper that I put out was classified "confidential." And it came out in '54, early '54.

Now many of the tests we made were basic, in other words, basic cones, basic wings, which included pressure distribution, temperature measurements, and also force measurements. Most of that was chasing theory around, in order to determine if those theories that the college crowd had published and all the mathematicians had come up with, if they were correct. I guess that's another thing that perpetuated the X-15, was that we found that the theories, particularly on cones and wedges and wings, were quite accurate. You could do a very good job of predicting the aerodynamics with mathematics, rather than having to test. What was really needed then was the combination of the wings and cones, and one thing or another, into a configuration. Then what can you do with that? The initial calculations on the original X-15 models, and everything, were done by calculating the wing by itself and the body by itself, and then adding the two together, rather than as a combination. Let's see, I guess, by the mid-60s, we were able to calculate a full configuration at one time.

LF: [Looking at 11" Hypersonic Wind Tunnel Logbook Volume 1] The "2 Speedomax, 18 Pressures." [Run #1] What were they doing there?

JP: Probably with a rake, and to measure the Mach number at various points in the flow, because the desire is to have a region where you have a constant Mach number. So when you put your model in there, it will see the same flow all over, as it would in flight. So all this initial business in here is calibration. It took a long time, because to measure the pressures, the pressure actually went from a steel tube inside the flow, generally to a rubber hose. And the hose went to an instrument. And then this instrument actually had a diaphragm in there. And this diaphragm moved a mirror. The mirror reflected a beam of light onto a piece of film. And so you actually had a graph drawn on this piece of film that moved behind the light source when you were measuring. Individually, these graphs on film had to be measured all at the same number of seconds from the start on through to the end of the run. Each instrument, you had to read it at the same time. And then that pressure that was measured, ratioed to the pressure upstream of the nozzle, that ratio looked up in the handbook, would tell you what the Mach number was. So it was a laborious effort to make these measurements.

LF: It says the "22LK leaks badly" [on run #19]. What is that?

JP: That was a leak into a particular instrument. That 22LK indicated which instrument was giving the problem. I see Mitchel Bertram ran those. "M.B." That was Mitchel Betram.

LF: I just have picked out a few diagrams, or pictures, that were drawn. This is "Stagnation Pressure Survey Rake" [on run #116]. Is there anything you can tell about that?

JP: Well, the stagnation pressure is that pressure that you measure when you have an open-ended tube pointing into the flow. And that differs from the static pressure, which means, if you had a closed end tube and holes in the sides of it, that would be a static pressure, whereas the dynamic pressure, or total head pressure, strikes the front end of the tube. Those two measurements are used on real airplanes today at all speeds, as well as in wind tunnels. It's a standard method of determining speed.

LF: What is "spark test" [on run #225]?

JP: A spark test is a method of getting a Schlieren picture or a shadowgraph picture.

LF: Also on the same page, [page] 36 [run #225], it says "flat wedges." So you're taking pictures of those?

JP: Yes, and they may have had pressures also. I honestly don't...when was this... eight, '48. I had been to work a month. I don't remember anything. Here I am [initials], "J.A.P.," that's me. In this case we were putting, I think it was tape... here it says three layers of tape. This was to cause a shockwave to occur at each one of these [on run 243], and then in the Schlieren photograph we could see where they occurred. [Reading] "Schlieren pictures were taken, but were no good, because wire had gone downstream during run 243." Wire, wire, wire. I don't know what wire we're talking about.

LF: It says, "Cemented to the bottom of the nozzle."

JP: Okay, so these were wires cemented to the bottom of the block. Some of them came off.

LF: What was the purpose of this?

JP: That would be to get a disturbance from each one of these, and then follow the disturbance through the test section. Again, a calibration procedure. What we were looking for was to have parallel flow through the test section at a constant Mach number. In the 11-inch tunnel, we ended up having about a four to five inch square of good flow. And then the other two and a half inches on either side, that's boundary layer.

John A. Moore, that's "J.A.M."

LF: Here you all are doing a static pressure survey on different numbered cones [on run #636]. Like one, five, and I think it continues on the next page... Do you remember what that was for?

JP: Again, calibration procedure, and this would be to compare with the Taylor and McCall cone theories. Taylor and McCall were British mathematicians that did a super job.

LF: Here are second minimum tests [on run #751], these pictures.

JP: The second minimum is... We have a big pressure tank, okay? It goes through the heater. Then we go through this slit that we talked about. The little slit. And it opens up in to the test section. From the test section, it goes on downstream in toward the vacuum tank. The second minimum, they bring the walls in a little bit after this run. What happens is, the shock sparks at the first minimum, that's the slit, and goes on through the test section, goes on downstream, and then they shut the walls down. That keeps the shock from backing up for a while. It lengthens the run. It lengthens the time you have good flow in the test section. This is one thing I think that was developed here at Langley, was a second minimum that really did the job. We could almost double the run. When we first started out, we could only get about 30 seconds. With the second minimum, it was up to a minute. Later when we put in the second high pressure tank and the second vacuum tank with the second minimum, we could get two minutes. Two minutes was about all we ever got.

LF: Here you start investigating a diamond airfoil [on run #845]. This went on for a long time, I noticed.

JP: Well, a diamond was considered the optimum, theoretically, for supersonic or hypersonic flight. It's rarely used in real life, because it's not as strong as curved surface. The curved surface airfoils are superior. Now on the X-15, they were essentially curved surfaces. Pretty rough, but curved. But that, again, is chasing theory. Well, they worked good, except that strength-wise, it wasn't as strong for use in flight. It's still good for comparing theory, and all that.

LF: And then here, on page 113, I think that is...

JP: Pressure distribution.

LF: On a diamond airfoil?

JP: Okay, yeah. Well, it may have been a curved airfoil, because we tested some circular arc airfoils.

LF: I think that happened starting there [at run #879]. So that would make sense.

JP: "I.E. Beckwith." That's Ivan Beckwith.

LF: "Force tests on cylindrical body with 20° nose cone" [at run #1001]. So you guys were testing cones a lot.

JP: Well, that's correct. Number one, checking with the theory, and number two, that's the ideal nose for a missile or a bullet, whatever.

LF: Here it's a wedge airfoil [at run #1140].

JP: Yes.

LF: Who came up with this idea?

JP: Again, it's really a wing. It's a wedged airfoil wing. Again, it's to determine actually how much lift and drag you have, and how close you can calculate it. The other thing that came about in all of these tests is this pressure on the base. What is that pressure? That is a number that is extremely important. On the X-15 with those wedge tails, they had a tremendous pressure. Now at hypersonic speeds, it's a near vacuum. It's not terribly harmful to the flight, because you've got so much momentum going ahead, but at low speeds, airliners, private airplanes and everything, the base pressure is extremely important. It is a much higher number compared to the rest of the drag of the airplane. So it's very important that you streamline everything. It does not want to have a flat bottom like the X-15 did. The base of the X-15 and the base of those two vertical tails probably added, I would say, at least 50 miles an hour to the landing speed. Had they been sharp, it would have been slower by at least 50 miles an hour. It crossed over the edge of the runway at about 200 miles an hour, which is going like crazy.

LF: Here you tested "Bodies of Revolution" [on page 214]. And they're all different.

JP: All various nose cone angles and the common body. Here we've got probably the same nose and various length bodies. So you've got two combinations. Again, there was no consideration of a winged airplane at this time. In fact, if you talked to most everybody, I never talked to John Becker, but to most of the other engineers, we were in for the study of missiles. There won't be any airplane at this speed. So it was a big change when it was decided to make a Mach 7 airplane. That was something big. And so I think Becker, he gets the credit.

LF: Sometime, in March 1950, it says it [the tunnel] moved to the Physical Research Building [on page 251].

JP: Yeah, that was our move. It stayed there until it was given to VPI. The big part of that move was the huge vacuum tank. The vacuum tank was, I think, it was 35 foot high. It must have been at least 15, 20 foot in diameter. It was huge. And it was three quarter inch steel! It was heavy. They put it on a special truck, and it came across from the East Area of Langley to the West Area. It came across and they brought it across on runways. They didn't come around the road. They'd tear up the roads, but the runways were strong enough to take the load. I remember coming out on a Saturday morning, and watching them tow that tank over and set it on its concrete blocks.

LF: Why did they move it? It seems like you just got settled, and then they said, "Let's move."

JP: Well, the new Eight Foot Tunnel was built in the place that took our place in the East Area. They had an Eight Foot Tunnel that was running at the time, but they made a new Eight Foot Tunnel. They tore down the old building we were in, because we were in what was left of the Propeller Research Tunnel that ran all during the '20s and '30s. They had closed it and were using the floor space for our tunnel and then storage. And then there was another tunnel on the other side of the building. The two tunnels moved out and they tore the building down and built the new Eight Foot Tunnel.

LF: Here's my last question for this logbook. This is, "Forces on Triangular-Planform Wing #1" [at run #1860].

JP: That's a triangular with a diamond cross-section, airfoil section. That would have been for considering tail surfaces on a missile. Again, there was consideration, thinking in terms of missiles that actually flew, that weren't just riding on the impetus that they got from takeoff, but where it was actually flying along. Cruise missiles, you would say. They were being given consideration. You think in terms now of, here we are in nineteen and fifty, and everything's atomic bomb. And so how are we going to get 'em from point A to point B? And the Cold War. We did not have missiles that would go around the world at that time. If you put a wing on it, maybe you could go further. So that was one of the considerations. The other was general research, in order to pile up information for the design, or to have a selection.

LF: You were showing me the model in that top box [of his bookshelf]. What was it for? When was it used?

JP: That was the prototype for the X-15.

LF: And then the one that you have in your desk, it's smaller.

JP: It's the same thing, except it was smaller.

LF: That's for concept?

JP: Well, it was while that other one was being made, this one was made and tested. [Looking at the small model] You see, it doesn't have any airfoil sections, and that other one actually had a wedge slab, wedge airfoil. In other words, it started out sharp and then it came and it was parallel for a while and then it tapered off. The tails, you remember, we had various tails. This was the concept that came about as a result of that memo from the [1954] proposal. We got that proposal on Wednesday, we made this model and tested it and gave the data to Becker on Friday.

LF: Wow.

JP: We shocked him, too. He even mentioned it in one of his overseas talks, the rapid turnaround of our ability to turn out data. Now that's something that the 11-inch tunnel was capable of doing that most of the other tunnels would not have been able to do in that speed. Now the balances that we tested this model on, and also that other model, were very simple balances. The model just slipped on a rod, and then the balance was back in the housing back here. Now on the real X-15 model, we had a six component balance.

LF: [Looking at wing cross-section from his desk drawer].

JP: That's the same wing, except smaller. This was made so we could test at high angles of attack. Ordinarily the balance would be at 45° and then you could go to high angles of attack.

END

Interview with Jim Penland
3 July 2007
Laura Freeze

Notes from 3 July:

-We looked through more in depth the first 11" hypersonic wind-tunnel logbook from 1951 to 1955.
-He discussed the first testing of the X-15 in the tunnel in mid-1954.
-I was given the original folder that contains correspondence and logs on the process and reasons for closing the tunnel from 1971 to 1972.
-He is letting me look through a copy of Hartley Soule's June 30, 1950 NACA paper entitled, "Review and Status Report of High-Speed Research Airplane Program."
-Additionally, I will copy a Langley Working Paper #343 by Theodore J. Goldberg entitled, "Stability and Control Data at a Mach Number of 6 of a Preliminary Configuration of a Delta Wing X-15 Research Airplane," undated.
-I am also looking through a NASA technical note D-5498 by Theodore J. Goldberg, Jerry N. Hefner, and David R. Stone from Langley entitled, "Hypersonic Aerodynamic Characteristics of Two Delta-Wing X-15 Airplane Configurations," Oct. 1969.

Recorded Interview with Jim Penland:

START

Laura Freeze: [Logbook #2] On page 37 [run #2484 and 2493], it says "8 D after body," or "6D."

Jim Penland: Generally, that means six or eight diameters long. The only other case of a D body would be a D-shaped cross-sectional body. That seemed like we did make some tests on that shape as a possible re-entry configuration, but I don't think it ever amounted to anything.

LF: It says [run #2512] ,"Tests of total temperature probe for IRD." What is IRD?

JP: IRD is the Instrument Research Division [at Langley]. They developed instruments and we were testing some of their new thermocouple measuring devices.

LF: Did you find they worked pretty well?

JP: Yeah, they kept up pretty good.

LF: I was looking [at run #2792], and it said "NACA RM-10 with fins and removable tail cone."

JP: Yes, that was a missile and that's a good picture of it right there. It had a shape like that and was round and it had fins on it. I don't remember what that missile's intended use was. I don't remember, but we tested it extensively. I think it was an NACA design, and hence warranted extensive tests. I never tested it personally.

LF: This is run #3395 and I was going to ask about this, the dummy sting.

JP: That sting was actually around that, and then you would run this sting that held the wing. And then you would subtract off the measurements that you had made on the sting and assume now, and assume, that what was left was wing data, which it may or may not have been. There may have been some interference between the sting and the wing, but at that period of time we didn't have any way of differentiating it.

LF: It [run #3767] says the "delta wing tip flap model" for a delta wing missile flap.

JP: I think the idea was to determine the feasibility of having aileron-type controls, or flap-type controls, on a delta wing. This is something we didn't know. Even the X-15, the wing didn't have any ailerons on it. The horizontal tail rotated back and forth to take the place of ailerons on the wing.

LF: And this was going to be for a delta wing missile, right?

JP: Yes.

LF: Here [on run #3963] it says, "Surfboard, Surfboard model." What is that?

JP: That model was long. The cross-section from one side to the other was diamond shaped. It was pretty thin. It was held with a sting. And I don't know what it was for. I remember we got two models; they were shipped to us for test. One of them was a cylinder with a conical nose, and then a probe that stuck out in front. Then the surfboard, so to speak… I've got a picture of it around here, but I don't have any idea where it's located. We tested it and gave them the data. It turned out that the cone-cylinder with the probe in the front was the same length-diameter ratio of the Atlas missile. Although we didn't know it at the time, that was what the test was for, to get some idea of what the lift and drag of the Atlas missile was. So Convair was the one that sent that missile here. I remember that it had the best lift-to-drag ratio of any model that we had tested at Mach 7 up to that time.

LF: This is [11-inch Hypersonic Wind-tunnel] Logbook number three. This is an interesting one [run #4612], the boundary layer program on a certain wing. They put oil streaks…

JP: About that period of time [late 1953], we were just beginning to put a mixture of oil and chalk, or oil and lampblack on models to see what the flow was doing. That was used throughout the life of the 11-inch tunnel, and then was used at the Mach 6 tunnel down here.

LF: This [run #4827] was January '54, when the new Invar Mach 7 nozzle was installed.

JP: Oh, yeah, shakedown run. How about that? That came just in time for the X-15 tests, because they would have been compromised if we had had the test in the old nozzle. But this one had almost four by four inches of really good flow right in the center. That was enough to put the model in and run through some angle of attack range.

LF: So this was a good development?

JP: Yes. Now, that Invar nozzle was the one that was made of Invar, because it didn't change with temperature. It didn't either expand or contract with temperature.

I recognize that model. [Looking at run #5343 from June 24, 1954 of the 1/100 scale M-7 model, which later became the X-15].

LF: Yeah, so here appears the Mach 7 airplane.

JP: That's this little one in my desk.

LF: You initialed it [the run].

JP: And Herbert Ridyard and Dave Fetterman; three of us here.

LF: Do you remember what you did that day?

JP: We had "wall pressures, three temperature Browns, Schlieren photographs, and two pressure instruments." An Si-1. I thought that was a string gauge balance. Well, the Browns would have measured for the Si-1. That was the first force tests that we made, and that's probably... Let's see, this was on... June 24, 1954. It would be interesting to know if they were on Wednesday.

LF: What happened on Wednesday?

JP: Well, that was when we got the [study group] memo. We built the model all day Wednesday and Wednesday night, and then tested it on Thursday and then worked up the data for Friday. [Looking at an old calendar]. June 24th was on a Thursday! And we learned about it on Wednesday. That's when we tested. The mechanics built the model within so many hours. This one was something that was done in a hurry to show our enthusiasm. And [John] Becker even mentioned it in one of his talk of less than three days turnaround from suggestion to data. That's unheard of these days.

LF: [Run #5487] A Mach 10 nozzle was installed. But it sounded like it had problems.

JP: We did have some problems. I think the problem was that the so-called second minimum wasn't the proper shape or dimensions for Mach 10. Also, we had a problem with pressure ratio. In other words, for Mach 10 it takes a higher pressure to do Mach 10 than it does for Mach 7. It's possible we had trouble getting enough heat also with the heater, because we used the same heater for Mach 7 as Mach 10.

LF: [Run #6121] It says it's a hypersonic research vehicle, but it's a PARD Wedge Nose?

JP: PARD was an organization here on the [Langley] Field. And they had come up with this configuration.

[Looking at run #6519] "Mach 7 airplane test." Once those were completed here, we never went back to testing that again, because soon after that, the honest X-15, the number one model, came for us to start testing. So we never backtracked. I think it's four papers that [Herbert] Ridyard, [David] Fetterman, and I wrote. And those were the only four on the Mach 7 airplane. Plus that little [1954 study group] memo for the hundredth scale; that was never published. That was strictly a memo to Becker's office, on this little airplane in here [in his desk].

END

Interview with Jim Penland
5 July 2007
Laura Freeze

Notes from 5 July:
-We looked through the 11-inch hypersonic wind-tunnel logbooks from 1955 until 1958, from runs 6708 to 10351.
-He talked about the Langley hypersonic research group and the X-15 tests in the 11-inch tunnel.

Recorded Interview with Jim Penland:

START

Laura Freeze: When did you move from the 11-inch tunnel?

Jim Penland: About '64, I think. And then I didn't come back until… I was only gone about a year and then I came back.

LF: Okay, so you really weren't gone long.

JP: No, I was still testing over there once and a while, but my office was moved over in the Unitary building. The reason was, that it was the beginning of the 36-inch Mach 10 Hypersonic Continuous Flow Tunnel. My immediate supervisor in 11-inch [tunnel] and I had attended all the contractual meetings and everything. Although they didn't send him over there, they sent me over there. So I stayed over there for a couple years and didn't really do much, other than write reports. I wrote a couple reports over there. Then we started working on the hypersonic transport, the models that we looked at that I'm working on now [for a current paper], and I came back to 11-inch. That was in '65, I think. So I wasn't gone much more than approximately a year, I guess. Then I stayed at 11-inch until it closed in '72. Then they moved us over to this other building over here. After that we were 20-inch Tunnel, the Mach 8 Tunnel, the Low Turbulence Tunnel in the East Area, which is subsonic, the 8 Foot Transonic Pressure Tunnel, tested there, and also in the 12 Foot Subsonic Tunnel in the East Area. That kept up really until I retired. It was all during the '70s. And then in 1980 I retired.

LF: [Looking at 11-inch Hypersonic Wind-Tunnel Logbook #4 from 10/18/55- 5/1/57, run #6708]. It says, "Hypersonic Research Vehicle." And when they mention that is that a specific one?

JP: No, because there were six or eight different configurations that were considered for replacement of the X-15. So it could have been any one of several different…

LF: Here's another one that shows up for a little while, the Lockheed Missile Program [run #6731]. You initialed, so I was wondering if you remembered anything about that.

JP: That was, I think, probably the X-17. You remember looking at it. It had different noses and different conical tails. There were three or four of them. Some of them had a triangular cross-section body, rounded corners, of course. When was this? This was '55, so that was… Oh, Si-10, okay. That was the X-15 balance, so that was definitely the X-17. Yeah. Although we tested several versions of it, I don't know how many they tested. It was a research vehicle. That was at the time that heat transfer was being studied extensively on the ground. With that vehicle they were able to study it in the air under true conditions, really, true speed and true temperature and everything.

LF: Here [on run #7036], this is early 1956, the X-15 starts being mentioned as the "North American Model Program." Before that, they had just been saying the "Mach 7 Airplane."

JP: Well, we're really talking about the X-15, but to my knowledge, it hadn't been named yet; didn't call it X-15 at that time.

LF: So this is after North American [Aviation] got the contract.

JP: Yes, yeah. That's correct.

LF: And then they sent it back to you for testing?

JP: Well, they brought it. They had an engineer. His name was E.W. Johnson. Two, three mechanics came to work on the model. We were running two shifts, you see. So they needed one mechanic in the daytime and one at night, and then there was a spare. But we ran the tunnel and got the data, and then he [Johnson] looked over the data and had suggestions on what we should test next, and so forth. It was a very cooperative program. Everybody got along real good. The guys that they sent were extremely good machinists. One of them, actually on just a plain old drill press, put different milling cutters in the drill press and had a vice to hold the item, and he actually made us some different tails to put on the airplane. He made them right there in the shop. While we were testing, he was out there playing. That group was super. One of them was named Adams, I remember him quite well.

LF: [Run #7372] "North American model X-15, second model."

JP: The first model had the small tail, and the second model had the big tail. It also, the second model, the body diameter was slightly bigger. Sometime during that period of time, the strakes that were on the side that originally went to the nose, we pulled them back. That was due to stability, the pitching moment. That took some load off the nose and allowed the nose to give more reaction to the tail and less to the nose.

[Looking at run #7940]. "Dynamic stability and reentry noses." Oh, yeah.

LF: What can you say about that?

JP: These were possible ICBM bombs. This is the reentry part. This was the part that, once the rocket had shot up, the afterbody would fall away, and then the missile would go ahead. These shapes were made such that they would reenter the atmosphere and not burn up. Then they were possible explosive heads. So we were working on munitions then.

LF: So this is the fifth logbook we're going through [11-inchh Hypersonic Wind-Tunnel Logbook #5 from 5/1/57-1/14/58]. We're looking at Mach 7 elliptical leading edge delta wings [on run #8764].

JP: Ellipsis… There was some idea of a method to alleviate the heating. They wanted to know what the lift and drag, particularly the drag, penalty would be with such a device.

LF: This is a North American X-15 flutter model with a horizontal tail [run #8895].

JP: I know that we tested over in the 9 foot by 6 foot tunnel, which is a lower Mach number; they tested it full scale. I think we had some pictures of it there at the 9 foot by 6 foot with the wing or the tail sitting up in the floor. And they tested it for flutter at that Mach number, but at Mach 7… Ordinarily a flutter model is made to a scale strength. I'm amazed that they would test this in the 11-inch tunnel, because we had water cooling tubes downstream. If something flew off the model, it could cut those tubes. In fact we had that happen one time. We had the tail of a model fall overboard, and it was just a thin piece of sheet metal, but it hit those tubes back there and actually cut a hole in them. And so we had to take the whole tunnel apart, and had to weld up that tube and put it back together.

LF: Here's a jet interference investigation on an X-15 jet model [run #8927].

JP: What the worry was, was the flow coming along this body would see this big balloon out here [from the X-15], and act as if it were a solid object. Then it would separate. But fortunately, it may have, but it didn't hurt anything. But in the tests, with the tails up here like so, this separation here did not materially affect the stability of the airplane. But that's what these tests were. Dave Fetterman did this, didn't he? Dave Fetterman, Davis Crawford, and Peter T. Bernot and Tom Blackstock. But that's what that was. And somewhere around here I've got a paper that Fetterman presented. In fact, that might have been in one of those volumes, because I think he presented that at the one conference.

And I remember when we had a meeting where Dave told all the assembly here at Langley about these tests. And I remember Hartley Soule, who was in charge of all of the X-15 project, he says, "Fetterman, boy! You're scaring me to death!" Because he was worried that if we had separation with the rocket on, that then the airplane probably would go catawhompus. You never know what's going to happen if it goes unstable. And so he was seriously worried.

LF: How did this concern come about?

JP: Probably when they started testing the jet. They saw what a big balloon it was under the vacuum conditions of high altitude, because the higher the altitude, the bigger that balloon is going to get. Fetterman was given that task of a test at Mach 7. And what he did, we had a body that duplicated the X-15. We had... The wings actually came in, on this particular model; the wings extended from the side wall and supported the model. And the air hose came in through the wing into the fuselage and squirted out the base to simulate the rocket. So what Fetterman did was to get the pressure ratio inside the tunnel to this rocket motor, the same as the true rocket was, and then we assumed that the balloon that we got in the tunnel was the same as we'd get in flight at the same Mach number. And then we measured the stability and control with it. So it was a very important experiment, and they got the right man to do it. Fetterman was a good man. But that was the test.

END

Interview with Jim Penland
10 July 2007
Laura Freeze

Notes from 10 July:
-We looked through three additional Langley 11-inch hypersonic wind tunnel logbooks, covering the years 1958 to 1963. The material stretched from the X-15 to various manned space capsules, such as Mercury and Apollo.

Recorded Interview with Jim Penland:

START

Laura Freeze: [Looking at 11-inch hypersonic wind tunnel logbook #6, 2/17/58-3/5/59] I was going to ask about the Mach 18 helium nozzle that was installed [run # 1-48, date 7/28/58].

Jim Penland: I don't know we ever tested anything in that. We calibrated; I remember that. But I don't ever remember testing anything in it.

LF: Why did you decide to install that?

JP: To get a higher Mach number. In fact, our helium nozzle here was sort of a prototype. We felt that we had the pressure tank and the vacuum tank. The combination was such that we could do Mach 18 helium. And that's the reason that number was selected. But later, what little bit we did with it showed that it was promising, and so they made a 22-inch helium tunnel in this building. It's now disassembled; we can't go look at it or anything. That ran for twenty years.

It [the Mach 18 helium nozzle] was strictly experimental. In fact, we tried other gases also. I think we tried argon, and I know we tried nitrogen, which gives the same result as air does, expect nitrogen tunnels you've got to be very careful.

LF: I have a question. This first shows up, "Project Mercury, Little Joe" [run #18-1, date 1/22/59].

JP: Yes.

LF: It says, "short model." And I was wondering if you could say anything about that.

JP: Let's see. Max Faget here and H. Julian Allen at Ames... Allen came up with the concept that a blunt object could leave orbit and it could absorb the heat and reenter and then parachute down. This was his idea. Before that we had considered going up in an airplane-like configuration. Most of them were delta wing-type things, and coming in very slowly, and let the heat... Don't come in so fast as to overheat. Hence, you could come in that way. It might take a couple orbits of the earth to do that, but it was decided that that was entirely too complicated, and they should come in with the blunt business. Time we started competing with the Russians... What period? That was...

LF: Early 1959.

JP: '59. This was the first tests of these blunt objects. Now we had some wings in here, too. Again, here's some of the lifting vehicles. That's sort of the background as to why we got into the model testing. And 11-inch was quick, because we could build a model, as we had shown, overnight. At the same time we were making Mach 7 tests, they actually had some very lightweight models that they had honestly tossed off the top of the building over here to see whether they were stable or not. It turned out that the final Mercury capsule actually had an extension that came back sort of like that on the back end of this. But the curvature, and this angle and all that, was more or less dictated with these tests, really. These configurations were... I don't know how much input the 11-inch crowd had in that. Bill Armstrong and I, we were strictly the operators, run the test and then we presented this paper in the conference.

[On the new test run numbering system for the logbook]. Rather than have consecutive numbers run, that you would have a project number, runs one through whatever for that particular project. This is the way some of the other facilities did, and I don't know... It wasn't my idea, but that's what they did.

LF: [Looking at logbook #8, 7/18/60-5/3/63] I was wondering what the "Trailblazer" was [run #68-3, date 5/26/61].

JP: It was a reentry project, as I recall. This was to help to decide what the shape's going to be. What the purpose of it was, I do not know. I don't remember.

LF: And then here comes Apollo [on run #75-1, date 6/22/62].

JP: Yep, this was the beginning of... Well, it was after the Mercury and after the Gemini. This was in preparation for the moon shots. I was over in the other facility at that time, and I had a project that we spent an immense amount of money on building the models. And then they decided they wouldn't run them.

LF: [Looking at logbook #7, 3/6/59-7/14/60] I was wondering what the "M.A.C. Capsule Mercury Canister Tests" were [run 18-22, date 3/37/59].

JP: I don't really know. If it had something to do with Mercury, it was probably that afterbody that I penciled in when we were talking. It had the cone, and then there was a cylinder on the back end of it. I think that that housed the parachute. The front was the heat alleviation material that they put on there to ablate when it reentered. So it actually either melted or sublimated on reentry. And that got rid of the heat.

LF: Here it says, "Thermocolor on X-15, the 1/50th scale." [run #26-1, date 5/6/59]. What were you trying to accomplish there?

JP: We had paint, which is readily available today that would melt or would change color at a certain temperature. The model was molded from the one we had pictures of, test pictures. It was a little smaller than that plastic model that I had sitting here on the desk the other day, a little smaller than that. We sent that to the shop and they made a mold and then made a plastic model of it out of white plastic. Then that was put in the tunnel with the paint, different paints on it.

LF: This came up a few times, testing the Space Ferry [run #45-1, date 2/2/60].

JP: The Space Ferry was an airplane-like configuration. It was delta wing and had quite a big body on it. It would reenter by actually flying at a high lift-drag ratio and coming in very slowly so it wouldn't overheat. It never... It was a dream, so to speak. There were many, many tests at different speeds, but it was never seriously tested, because about this time the Apollo was going hot and heavy. The space crowd didn't go for this airplane business much. They liked the capsule better. And it is probably better, I don't know.

LF: It says, "Flow field investigation, Lunatic program" [run #51-1, date 6/15/60]. That came up many times. So I assume that was a program having to do with the moon?

JP: It must have, because it's talking about flow field investigation. But there's so many ideas. Everybody that had an idea would run off to test. What you would do with oil flow is to... Generally, the best thing was to either have a white model and use blackened oil, or use a red model with white oil. They would take a toothbrush and pour it in a pan, and take a toothbrush and dip it into it, and then take something with a toothbrush and rub across the bristles. When you do, it would throw off little droplets. You could cover a model very nicely with that once you learned how to do it. Then while the oil was still wet, you would put it in the tunnel and run the tunnel only for a few seconds. Then that would smear the oil and you could see which way the air was going on the surface of the model. That gave you some idea if you had any separation on the model, meaning the flow actually stalled on the model. You might have some backflow flow upstream, really, if you had a vortex and stuff.

END

Interview with Jim Penland
12 July 2007
Laura Freeze

Notes from 12 July:
-We looked through another Langley 11-inch hypersonic wind tunnel logbook, covering up to the year 1971. The material contained a couple more, late X-15 tests and extensive testing of a hypersonic cruise transport.
-Jim Penland discussed the end of the 11-inch tunnel in the early 1970s.

Recorded Interview with Jim Penland:

START

Laura Freeze: [Looking at 11-inch hypersonic wind tunnel logbook #9, 5/3/63 to 2/25/71]. It says "Martian Reentry Vehicle" [run #92-1, date 1/15/64]. What were you testing on that? And do they ever actually use that?

Jim Penland: There were several Martian reentry vehicles, which this one I don't recall specifically what it was. In all cases, when they send a probe to Mars, it has to reenter. High heating and high drag is the same as reentering Earth orbit, but it's probably less severe on Mars than it is on Earth. Then the final letdown is either by parachute, and I think one of the probes actually had balloons that expanded all the way around it, and when it hit it bounced like a rubber ball. That protected the instrumentation and everything. Then once it stopped, I think it collapsed.

LF: "X-15 Ramjet Plume Investigation" [run #112-1, date 9/30/65].

JP: So this was prior to the ramjet tests. They were planning on lighting that ramjet off, but because they had so much trouble on the dummy tests, where it almost burned the tail off, it was never tested.

LF: So this is what the investigation was?

JP: I think that's what that investigation was.

[Looking at the HL-10]. That was tested and they actually made a subsonic manned vehicle of that. They dropped it from the B-52 out at Edwards [Air Force Base].

LF: The HL-10?

JP: Yes. They obtained subsonic stability and control data from it at subsonic speeds, but there was never a high-speed version built. There were two lifting body configurations. The HL-10 was designed and pushed by Langley. And then Ames Lab had one that they called the M-2 that they pushed. It was actually a blunt cone, and I think the top side of it was straightened out. It was, for all practical purposes, a blunt cone that was used for a vehicle. And I think it had some horizontal, vertical tails for guidance.

[Looking at run #116-1, date 2/17/66]. These were cruise configurations that were tested prior to going on contract, and coming up with the ones that I had the drawings of in this paper I'm working on. They only came up with two, and then we made variations for the rest of them.

LF: This appeared dozens of times [in the logbook].

JP: Yes.

LF: Did anything ever come of the study? Did they ever build a hypersonic cruise transport?

JP: No, never did. Money, I think was the... Nobody was willing to spend the money. Now the military, see, their money bought the X-15, even though we were more in charge of the design and the testing and even the flight testing. But the Air Force was not happy about that. They wanted to be the big cheese and everything. Some general said that the Air Force would never get in a predicament like this again.

LF: But it turned out so successfully.

JP: It did. And that's probably the reason, is they got out of it or didn't get in it in the first place, because it would not have been so successful if they alone had done the testing.

LF: Here's the last time that the X-15 is mentioned [in the logbook]. "The X-15A-2" [run #124-40, date 11/15/67]. So this is late 1967.

JP: I honestly don't know. I could speculate, but I don't know.

LF: What do you speculate?

JP: I speculate it was additional tests for the vertical tails for stability and control, because here we are in '67, so we're two years out of completing the mission, really. If it had been the delta wing X-15, I wouldn't have been surprised, but they would have mentioned delta wing, I'm sure, if they had tested that. And I honestly don't know if that was ever tested in the 11-inch tunnel or not. I didn't have anything...

LF: Yeah, I didn't see it at all.

JP: I know it was tested in this building [hypersonic facilities] at Mach 6, and I know the guys who tested it. [Looking at X-15A-2 run]. Body, wing, X, I don't know what that is, horizontal tail, vertical tail.

LF: I know you can't read it very well, but tests came up on a Martin Orbiter Delta Wing Shuttle [run #140-1, date 1/4/71]. I was wondering if that was one of the proposals for the Space Shuttle.

JP: Probably, yeah. There was a whole bunch. The main difference in the initial studies for the Shuttle and the final Shuttle, was that the initial studies were, more or less, a delta-wing airplane. It had tails on the tips of the wings. We learned during the tests that we were better off with one big vertical tail in the middle, than we were with two on the tips. So that's the way it ended up. That was a result of tests here on the [Langley] Field, both at Mach 7, and then in the Mach 20 Helium Tunnel.

LF: I was hoping we could talk about the end years of the [11-inch hypersonic] tunnel. I have been reading through the folder that has... As they were thinking about shutting down the tunnel. It said that there was a two month off-on cycle usage for the tunnel. When did that start? They would use it for two months and then not use it?

JP: I don't remember why there was that delay, but it probably had to do with the equipment that was something that we couldn't borrow, or somebody else had used at the same time. Another thing, I guess I haven't mentioned... The 11-inch tunnel had very low pressures in the test section, and those pressures that were measured on the wings and bodies were very low pressure, compared with atmospheric pressure. And the instruments that we used to measure those were instruments that had been designed and built here on the Field by the Instrument Research group. They were made for Edwards. They were made for flight testing. Very frequently we would have to shut down the tunnel and take our instruments and ship them to Edwards for flight tests. And then they would ship them back and we would use them in the tunnel. Nowadays there are instruments available on the market to do the job, but it wasn't at that time.

LF: I was reading [the records folder], and there were different memos back and forth in '71 and '72 about the tunnel and safety issues. One of them was how only 4 out of 48 bolts were being used in the strut actuating section. Do you remember anything about that?

JP: I don't remember specifically... There was probably some concern about the strut and its... We had a couple different struts that we used, which attached to the mechanism that gave the angle of attack. Some of those struts were made to hold balances, and others for other purposes. That may be what they were addressing, was the possible safety concern of that strut attachment. That's the best that I can remember on that. The loads on the models and on the strut were relatively low at 11-inch tunnel, because the pressure in the test section was quite low. It was down to a one-hundredth of an atmosphere. It was going like crazy at 3,000 feet a second, but it was a very low pressure. The loads on the model and on the strut were small. One bolt would have done the job, really.

LF: Why did they close it in the end?

JP: I don't know. I wish I did. Nobody told me why. I don't really know. I think I could give good justification to keep it nowadays, but at that time, I was not as familiar with the abilities that we had in that tunnel that you don't have in other hypersonic tunnels, namely that it's all laminar flow. We had very great difficulty getting any turbulent flow on the model. The models generally had to be quite long in order to get any turbulent flow at all. The hypersonic tunnels we have that are in use today, have limited laminar flow, and lots of turbulent flow. That facility was unique in that sense. I personally think that laminar flow is mandatory for hypersonic tests and for sure, a large extent of laminar flow that will be necessary on any kind of a hypersonic cruise vehicle, because the skin friction is so high, that I don't know that a hypersonic cruise vehicle could operate very efficiently.

LF: So when they said they were closing it, did they just assign you to different places?

JP: Yes. I was the one that was responsible for trying to give it [the tunnel] away. My boss said, "If you'll write the letters, I'll sign them." So I wrote a form letter, sort of. We sent it to all the universities in the East that we knew about that had any kind of aeronautical research... Also to the aero lab down at Tullahoma, and maybe, I can't remember if we sent one to Wright Field or not; we probably did. It turned out that VPI [Virginia Polytechnic Institute] was the only group that showed any interest initially. Later on, after we had already given it to VPI, and started moving it, then the people down at Tullahoma thought about it, but it was too late. Not only did the specific tunnel itself go to VPI, but some of the vacuum pumps, and some of the compressors also went. And then the rest of them were put on surplus. Lord knows what happened to them.

We had a couple of dozen vacuum pumps that came from the Oak Ridge atomic uranium manufactured during the atomic bomb years. It had two kinds of pumps on it. It had a pump that actually was a rotating pump that rotated in an elliptical chamber. Out of the thing that rotated, there was a vane that came out that rubbed the side. Attached to that was a series of diffusion pumps that took it on down to even lower and lower vacuums. They had a boiler that boiled this oil. It came out and sprayed all the way around. Then the vacuum would come down here and this spraying would pull the vacuum. There were three or four of those along with the mechanical pump. When you get to very, very low pressures, the diffusion pump is the only way to go. It was then that we attached to the 3,000 pound air line that goes all over the West Area over here, and used the high pressure from the central pumping station, rather than our local compressor. I think the local compressor that we used, I think it went to VPI, and also some of those vacuum pumps. All of them didn't go I know. We had trouble getting rid of them.

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