NASA GLENN HISTORY OFFICE
ORAL HISTORY TRANSCRIPT

NED HANNUM
Interview by Ginnie Dawson
January 22, 2003

My name is Ned Hannum. I retired in the summer of 2000. My last job here was as the Chief of the Turbo Machinery and Propulsion Systems Division. I actually started here in 1962, after graduating from Rose Polytechnic Institute in Terre Haute, Indiana. Rose Polytechnic Institute is now called Rose Holman and it admits women, which it didn't do when I was there, and that's a good thing. I started here in 1962 in the rocket business.

I got started in RETF as my really second job here fresh out of school. My first job was with the RL-10 over in PSL, and that lasted from '62 into mid-'63 or something like that, and then I immediately went to RETF, and spent a lot of time there, on a series of tests.

My first impression was it looked like a factory or something. It didn't look like…I expected a rocket test stand to look like rockets. And I expected white coats and things like that. And this place just doesn't look like that at all.

It's very large, lots of machinery. Everything is exposed. There are no panels that hide things.

The first time I saw the RETF facility, I was just shocked. It didn't look like what I thought I'd seen on television, where there were these launch gantries and things like that. And I thought maybe that that was what I was going for. This is a research facility. The RETF facility is made to be very versatile, to be able to do lots of different things and fairly quick turnovers. So it looked more like a factory than what I expected.

A research engineer is, and what is research in the rocket business, is someone who is trying to figure out how to do something that someone else has pretty well determined we ought to do. So it's kind of like the nuts and bolts of how to do it. Now, that's not a complete definition, because there's lots of spin-offs and things get reused for different ways. But a research test facility is one that is very versatile, and so instead of having whole engines, we were able to test partial engines, or we were able to test things that looked like, that were easy to be changed to do research.

The way decisions are made about what to test are often, start from the researchers themselves, listening to what's going on in the country, attending meetings, hearing what mission designers are planning on doing. Then we advocate for money to try to get money to do that. In the early days, this center when it was the Lewis Research Center was able to make a lot of independent decisions about what research to do. As the Shuttle Program grew, and as it became very necessary for the agency to centralize a lot of decision making in order to make something as grand as the shuttle program to happen, then we had to have the concurrence of headquarters folks more in more detail than we had in the early years. And in the later years then, as the Marshall Space Flight Center was delegated to headquarters authority, then it became the decision making when research became something that happened between researchers here, program managers and researchers at the Marshall Space Flight Center with the oversight of headquarters.

The work on the shuttle main engine happened pretty much at the contractor and with the supervision and guidance of the Marshall Space Flight Center. This [Lewis] center made one contribution, or we tried to make a contribution in the earlier years. For instance, Sam Stein, who retired from here holds the patent on the concentric tube injector, which is the baseline kind of injector for hydrogen and oxygen engines, and of course, that's what kind of injectors are used on the main engine. Ignition understanding came out of research here that was applied to the shuttle. Lots of turbo machinery things and lots of technologies that were spread over the engine. But this center didn't really have a contribution much to the design of the space shuttle main engine.

I think my favorite research program at the RETF was the screech program, and that was the first one I was on. Let me say what "screech" is. Screech is an instability in the combustion and it occurs at frequencies of several hertz. Maybe 2 or 3, or 5, or even 10,000 hertz, and so therefore it makes an audible sound, and it can be heard with the human ear. It's a very destructive thing because it enhances the heat transfer in the engine many times over what it was designed for. So, when a designer designs for one level of heat transfer, and this instability occurs and the heat transfer is 10 or 20 times greater, then you have real serious problems and the engine burns out in a matter of fractions of a second. So it's a serious problem. We worked on the screech program back in the mid-60s for quite a few years. And it was strictly an experimental program.

The screech program was in response to two things. It was kind of caught between the need to build future hydrogen and oxygen engines, and at the same time to try to solve problems with the F-1 engine which was being planned for the moon shot, and the F-1 engine was not hydrogen and oxygen, but it had a lot of instability problems, and we were trying to plan for the future, and hopefully, at the same time come up with something that would help the F-1.

The instability problem with hydrogen and oxygen was solved at an experimental level. It became pretty clear what some design rules were, some good practices that would mean that there was not instability with high pressure, higher temperature, hydrogen/oxygen engines. With the other kerosene engines, or carbon-based propellants, I think it probably wasn't solved, although there was a significant increase in understanding and we never were able to get it right, but not with the same level of detailed physics.

I was involved at RETF with Test Stand A primarily because it was a vertical rocket stand. Then later on, when the B Stand was built, which was the altitude facility, I wasn't personally an experimenter or researcher at that time, but that work was happening in my organization, and that was just some work that will be appreciated in baseline for many years to come. In the higher ratio nozzle business.

Folks who were involved in the B stand higher ratio testing were Al Pavli, Dick Quentmeyer as kind of the senior folks. There were a lot of other researchers that were very important in that process as well.

The B Test Stand is able to test higher ratio nozzles in an earth-based test facility. Otherwise, the only other way to do that is just to launch engines and then track their trajectories and from the way they perform in space, infer what the nozzle performance is. So this is a much cheaper way of doing it. The other thing is that there was no data then on nozzle area ratios greater than maybe 150 area ratio. That's the ratio of the area, to the minimum area of what's called the throat. So having this data allows designers when you're trying to get the very absolute best performance like for a deep space mission or something like that. And it's baseline data. It's the kind of thing that ends up in reference books that people designing nozzles for any size and any propellant combination can use this data to anticipate what the performance will be.

There was a wonderful relationship between the researchers and the operations people. Let me just say that absolutely nothing of value could have ever happened without the operations people being able to perform their tricks of making, to simulate environments and make things happen in a cost-effective, quick way. Let me just digress for a moment. For instance, in the screech program, they were able to design hardware that when we'd failed an engine early in the evening, they could rip it off the stand, put another one on, and we could test a second engine the same test day. In fact, a few times we even tested three different complete build-ups in the same test day. So the operations people were absolutely crucial to any success. As far as the research objective was concerned, that usually came from the researchers at the stand, plus collateral organizations that built instrumentation and computers and other organizations like that who contributed in a tremendous way to the research.

As I think back over the career, I think that the changes in the way we gathered data had just unimaginable impacts on not only the quality of the research, but the actual organization of the people. In the early days, it was so difficult to gather the data. It had to be done by hand. You had to read strip charts or take pictures of monometer boards and look at them through magnifying glasses and make hand calculations that the data crews had to be large, and that meant the whole research crew was large. The good news was that we all had mentors. When I first came, I was a part of a five person research group, and therefore I had mentors, folks who had been here five years and ten years and fifteen years, all in the group. It was a wonderful learning experience. The bad news is that it was a real slow process, and we missed a lot by taking data by hand. As we went from jet engine testing which were 20-second tests and you could kind of take your time measuring things to rocket testing where tests were maybe 1.8 seconds or a long test was 3 seconds, and where most of the information of value was in the transient, the start, from the start of transient, we had to learn to take data much faster. That meant the evolution of instrumentation and it meant the evolution of ways of recording the data and reducing the data from electrical impulses to meaningful pressures and temperatures and things like that. So there was a concomitant evolution of a lot of things that happened in order to support that. One of the bad things that happened was that now the research crews didn't have to be as large, because we didn't need as many people to make all this happen. And so we began to sacrifice some in this mentoring structure that had been so important in my career and in the careers of others who came in about that time.

The effect of computer modeling was just amazing. Let me just use screech as an example although I'm going to say would apply to several different areas. In those early days, this combustion phenomenon that happened at several thousand hertz was something for which there were no models available. There were some theories of understanding that said when this parameter goes up, this other parameter probably goes down, but we don't really know what the magnitudes of the ups and the downs really are, so that's a theory as opposed to a model. So the way we worked on Screech was just build experimental configurations and more and more and more and test and test and test until we had enough data that, I always said any fool could have begun to see a pattern here from what was happening. But at the same time we were doing that, there were evolving faster computers and better mathematics, essentially, better ways of dealing with large amounts of information. And then models were developed and in the very end of that, I need to skip ahead several years, and not so much with Screech as with other phenomena, we were able to use models to predict what the outcome would be if you made an engineering change. If you changed a diameter or you changed a material or changed the velocity, what would the outcome be. We built an experiment to determine if we were right with the model. And often times we weren't or often times we were close, or often times we weren't exactly what we wanted, so you build a second or third one. But instead of building hundreds of configurations to get to the final answer, with the aid of modeling we were able to get to a good answer much, much quicker and much, much cheaper.

The relationship between the researchers and industry evolved also over time. In the early days of the space program, the nation wanted to build something, and build it quickly, and so industry geared up to build things. In the meantime, the researchers were thinking about the future problems and we were trying to anticipate what the questions were going to be for the next configuration or the next configuration. So there wasn't a great deal of communication between us and industry because what their objectives were not the same as ours. As the space program evolved, to second and third generations, and we started advertising the results of our technology, sometimes what we had done was of interest to them, and sometimes we'd worked on the wrong things, and it wasn't what they needed to know at all. And so dialogues began to be developed using forms like the AIAA (American Institute of Aeronautics and Astronautics) professional forums and other forums between industry and the researchers to try to narrow the gap between what researchers thought the industry needed to know and what they really did need to know. And I would say by this time in history that there's a very close relationship, and industry does often come to the research centers and say, we're going to need this or sometimes we go to them and say we think you're going to need this, and they concur, and then we become partners in the research. And one of the early problems was trying to transfer the technology. We thought we had an answer to their question and they'd thought the question was a different question or that our answer was wrong, and there was some reluctance to pass the information or we called it technology transfer. I would say in present times it's not nearly as much of a problem because we engage those future users of the technology early on, and make them partners in the development of the technology so they know it's there, it's coming, they've helped develop it and technology transfer is not really an issue. Now, technology transfer is still a big issue for the collateral applications of the technology, when you're trying to move the technology to some other user that you didn't anticipate in the first place or to some other new use. Technology transfer is still an issue.

If I look back at the RETF and say what really did it value the nation, I guess my kind of arrogant answer is that it was a great learning place for a lot of terrific researchers who made contributions, both on the research they did at RETF and later the research that they did or directed, or led in other ways. So it was a great place to learn. I don't know that taxpayers would like that answer particularly, so let me try another answer as well. I think the contributions to the center were that it was a unique facility in a lot of different ways, and I could elaborate on that, but it really was a jewel in the crown of this center for a lot of time as the nation tried to break into the space business and as this center tried to find a role in the space business. Let me just say very specifically ways to use hydrogen. This center has always been the center that was best prepared to handle hydrogen, to burn hydrogen, to ignite hydrogen, to deal with instability to design for hydrogen, to cool with hydrogen, and that facility was real important to the center. As far as what RETF did for the nation, it's difficult. I suspect that, and I'm only guessing here, I suspect there were certainly more than 100 approaching 200 technical reports that came out of there. I'm sure that a lot of those have not found their day in the sun yet, but they will. And a lot of them have been used by a lot of people to make contributions.