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THEODOR KOSTIUK
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is August 16, 2017. This interview with Dr. Theodor Kostiuk is being conducted for the NASA Headquarters Science Mission Directorate Oral History Project. Dr. Kostiuk is speaking with us today by telephone from the Goddard Space Flight Center in Greenbelt, Maryland. The interviewer is Sandra Johnson. I want to thank you again for agreeing to talk to us. We really appreciate it.

KOSTIUK: You are welcome.

JOHNSON: I'd like to start today by talking about your educational background, and how and when you first became interested in working for NASA.

KOSTIUK: Well, I went through the New York City [New York] public school system. In my junior high school, it was a time when we had the International Geophysical Year. I think it was 195[7 to 1958]. I was really involved in that whole project in the school. In fact, I guess that was my first exposure to space and technologies, and things that relate to this. This was also done in connection with the [American] Museum of Natural History [New York City, New York]. They had a program and I was involved in that.

That provided some initial interest. But then once I got to high school, I went to Brooklyn Technical High School [Brooklyn, New York], which was a technical school. We learned a lot about not only science, but also engineering. Then in college I went to City College of New York, where I majored in physics. From there I went to graduate school at Syracuse University [Syracuse, New York], and I majored in what was then called solid-state physics, which is now called condensed matter physics.

I dealt with microwave spectroscopy of solids. Things that were relevant, I think, to understanding of time constants within these solids, changes of spins of atoms in molecules within materials, and a kind of science that eventually evolved into the MRI [Magnetic Resonance Imaging] technologies that we use today for medical applications. In fact, I even helped teach an outside course by a fellow, his name was Hugh Hair. He was a private industry guy who was a pioneer in the MRI effort.

That was the extent of my experience in graduate school. I received my degree, but before I finished my degree, I had an opportunity to apply for a National Research Council Resident Research Associateship at Goddard Space Flight Center. That came through my professor, Gunther Wessel, who was called by someone—I guess a former colleague of his—about the fact that someone was looking for a microwave-type person to work at Goddard.

That was my first dream, of coming to a place like NASA, so I applied. I wrote a proposal. I spent some time writing a proposal on a topic that I knew very little about, and that was to generate microwave frequency standards, how to make a frequency standard.

The group that I came to visit was a physics group within the Engineering Directorate at Goddard, in Code 500. That group (led by Fouad Major) was working on an electromagnetic trap of microwave radiation at 40 gigahertz. This frequency was a transition in mercury atoms, and the technique provided a spectrally pure and stable output frequency. So a very narrow frequency emission, and that would provide a kind of secondary standard for measuring frequencies. I guess they argued one application provided a technique that could be used for collision avoidance in aircraft, for example, and things like that.

In fact, this group, as I said, was an engineering group, and it dealt with frequency standards. That division built the frequency standards for the satellite tracking stations around the world for NASA. They actually built atomic standards; it may have been argon standard or whatever was used at the time to basically get accurate time information during the tracking. So each station was basically referencing their time of tracking to an atomic clock. They built atomic clocks.

That, of course, was superseded later by the TDRS [Tracking and Data Relay Satellite] satellites. The ground stations, if there are any today, certainly are there for possibly other reasons. But at that time, we had stations all around the world tracking our Earth-orbiting satellites, so that was the group that worked on that.

The other thing that that group, and that branch specifically did was laser ranging. In fact, that's still done at some level here at Goddard. Basically, they were tracking satellites with lasers, and looking at their positions and comparing them. From the science standpoint, it was also a way of looking at changes in distances between let's say, California and Goddard. In fact, I remember even in those early days they measured that California was drifting out into the Pacific [Ocean] at 5 centimeters a year or something like that.

It was a way of triangulation. They were able to track a satellite with two stations: one in California, one here. These were maybe even mobile stations in the case of California, where they were able to measure the distance between the points by measuring the distance between the satellite and the two stations.

In any case, that's where I came in. I came as a post-doc [doctoral researcher] and it was very quiet. There was very little action in my group. That group had just managed to achieve its first resonance within this electromagnetic cavity at 40 gigahertz, and they published a paper in *Physical Review Letters*, which was a very prestigious physics publication.

Then, as history would have it, they RIFed [Reduction in Force] the whole group. So that group disappeared, including Fouad [G.] Major, who was the head of that group. He was a very, very, very brilliant guy, I thought at the time. He eventually left Goddard, as did some of the other colleagues that were there. In any case, it turned out that I never got to work on this project. But I had to find a place to go, because even if they RIFed that group, I still had my NRC [National Research Council] post-doc position.

They wanted me to stay there, by the way. It was kind of interesting, because at that time, especially in Engineering, the hierarchy was almost military-like for me, coming from a university. I don't get to see the Branch Head unless I have an appointment and I get called. I certainly never got to see the Division Director or the Deputy Division Director without some special occasion or invitation.

Well, I started to get these invitations, because they realized that I had options to perhaps go elsewhere. Because, technically I was self-employed. So I got to see the Deputy Director of the division, Henry [H.] Plotkin, who was also a physicist, I think, by degree. In fact, Henry and I did work later on—10 years later—that was very important.

And Walt [Walter J.] Carrion, who eventually, I think, became the Director and Head of Engineering. He was the Branch Head at the time. And then the third person was [Robert J.]

Coates, who was the Division Director there. So these were people that I got to talk to, and it was a little intimidating. I'm just a student coming out of school, and here I am—these are very important people who you have to go through two secretaries to even get into the office. So it was kind of, I guess, impressive.

There is a funny story about this, which I can tell you. In any case, Fouad Major in order to help me, recommended, "Let's go and make contacts with people in Sciences, where you are better prepared for." Again, I was very impressed by the fact that there was this lowly post-doc, and I'm coming in and having these meetings with division heads and major scientists in several divisions. There was Astrophysics—I forget what it was called at the time. There were the Earth Science-type people. Then there was this Laboratory for Extraterrestrial Physics.

Now, going back a bit, when I came to this Engineering branch originally, they somehow discovered that I liked to play soccer. Goddard formed what's called the Goddard Soccer League about that time. One of the colleagues there came to me and sat on my desk and said, "Ted, do you play? You want to come? We are going to have practice next Thursday," or whatever it was. So I went out to practice.

Of course, at that time in the early '70s soccer wasn't a big sport in this country. Most of the people there were relatively inexperienced, and parents whose kids played and they wanted to participate and so on. Anyway, we were kicking the ball around, and then we are in a little scrimmage. I kicked the ball away, and then some guy with glasses came in and just rammed into me for no reason. He and I had a conversation, and that was it.

Anyway, now I'm back, and I'm being coaxed into coming to the Laboratory of Extraterrestrial Physics. I am in the Lab Chief's office—they were called lab chiefs at the time instead of Division Directors. Here are people from every branch, and each one is telling me what they are doing, and I feel so important. Because they see free labor, you see? I am free labor to them. They don't have to pay, it's already taken care of.

Then there were two fellows after this meeting who were sort of continuing to lobby me. One of them was Bill Jackson. He eventually left Goddard. Then there was Mike [Michael J.] Mumma, who was a relatively young guy. He has been there two years. We walked down the hall, and there is an empty office. We get into the office, and I am sitting on the table, and both of them are jabbering away, so to speak. Then this guy walks by this office, the same guy that kicked me on the soccer field. He sees me and he recognizes me. He comes in and he is really friendly. We are saying hello and I'm not impolite, but I'm a little reserved. And so we have a chat and then he leaves.

Then I notice something that I didn't realize—the two colleagues who were incessantly trying to sell me their project were so quiet when this man came in you could hear a pin drop.

I said, "What?" I turned to them, "Who is this guy?"

"That's Norman [F.] Ness." He was the Laboratory Chief (Division Director). Also, he was a feared Laboratory Chief. His nickname was "Stormin' Norman." If you look in your history, I am sure there is a history about Norm, because he was the PI [Principal Investigator] on missions that detected magnetic fields on all planets that have magnetic fields.

In any case, I told this story at his retirement. I said I was young, I was impressionable, but I didn't want to be on a team opposing him, so I joined his team. And I went to the Laboratory of Extraterrestrial Physics.

JOHNSON: That's funny. And what an introduction.

KOSTIUK: Right. We got introduced on a soccer field. There were other stories, because later on I was the league referee. I would give him warnings and red cards for his antics on the field. But we were good friends, very good friends. After he left to head the Bartol Research Foundation, he used to drop by even when we moved to this new Building 34 here. A couple of years ago, he still would drop by. I haven't seen him for a while, but it was interesting.

Anyway, that's how I got into the field. The field I got into was infrared astronomy. That's what was being sold by Mike Mumma. Basically, he was anxious to initiate an infrared program that focused on comets. That same year was the year of Comet Kohoutek. Now, at that time, that was supposed to be the brightest comet ever to reach the Earth. It was supposed to be as bright as the Moon.

One of the fellows in Astrophysics, Steve [Stephen P.] Maran, he was the main PR person for this comet, and of course it was published everywhere that this is going to be the brightest comet. The reason it was supposed to be bright is because it was detected very early on, before reaching close by to the Earth and to the Sun. At that time finding comets was a fairly rare event.

To make a long story short, Mike Mumma wanted to observe this comet and build instruments for that, so I started doing that. We got some money from the Directorate, and we built two instruments to observe the comet. One was actually one that could be used. It was mainly built on a contract with a company up in Boston, Arthur D. Little, and it was using solid state diode lasers as local oscillators.

There were two techniques. One was to do spectroscopy on the comet—or on anything for that matter—by using a technique that's analogous to a radio receiver. What you do is you collect light with an antenna or a telescope, and you combine it with a known frequency, generated by a local oscillator. In the case of the infrared, it would be a laser of some sort, a CO_2 [carbon dioxide] laser in particular, or as we tried, with a diode laser.

Now, this was a technique that was worked on and developed by students under the direction of Charles [H.] Townes at [University of California] Berkeley, and we wanted to use that technique for looking at the comet. They were using primarily CO_2 lasers as local oscillators, and we wanted to use diode lasers, which were tunable so you could get to different regions of the spectrum that were not possible with a CO_2 laser.

There was another connection here, because that same Branch that I came to work for originally did studies on laser communication. They had a project or program to do laser communication. This laser communication involved CO_2 lasers being the transmitters, and radiation from them being received and detected by the heterodyne technique. Again, a known frequency (CO_2 local oscillator frequency) and a frequency that's been modified (with transmitted information) are combined on a detector/mixer. Then, by taking the difference of the two, you can measure the modified portion of the frequency, and that's the information.

That's how your radio works, for example. The radio stations send out a signal at a particular radio frequency—each station has its own frequency—and they modulate it so it has information or the sound they are transmitting. And in the case of TV, video. Then the receiver, your radio or your TV set, has a local oscillator in it. That's the thing you change to find the station. So when that local oscillator frequency that you change matches that particular station, or nearly matches that particular station, the difference between the two is what's analyzed and amplified into sound or video. That's how the detection is made, and we wanted to use the same technique in the infrared to look at molecular constituents of comets, and ultimately planets and even stars. It's called infrared heterodyne spectroscopy.

So that's how it started. There are stories associated with that, and we did our first measurements at the Goddard optical site on a 30-inch telescope. We tried to set things up so we could look at the comet. We had rain in the dome. It was a very Spartan environment, if you will. But we never really saw the comet. It was not nearly as bright as the Moon. Certainly in this area it was maybe not even easily visible by eye.

But what it did is got me a job. Because after that we made measurements, we had papers that were published, mainly on the technique. And that, I guess, was enough to try to hire me after about a year and a half as a post-doctoral fellow. I then continued the work on the technique. We did quite a bit to improve it. We built an instrument and used it at Goddard, not at the 30-inch telescope but a new 48-inch facility. We were able to look at the Sun, Jupiter, and look at ozone absorption and CO_2 absorption in the Earth's atmosphere.

Then ultimately, we decided that we need to go to a much better optical observatory, astronomical observatory. It was an opportunity to go and set this instrument up at Kitt Peak National Observatory in Arizona on the McMath [-Pierce] Telescope, which was a solar telescope, 1.5-meter-diameter telescope. That was a whole adventure in itself. I don't know if one would do it this way today, but we got a trailer, had it refurbished so we could take all our equipment and truck it all the way there to Arizona. So I think 1976 or so we flew into Tucson Arizona and started setting up at the McMath-Pierce telescope. It took several weeks to get everything working. I had a new design for the instrument, which matched the facility and the telescope optics at the McMath.

We observed there for a number of years, until about 1984, something like that, at which time we went to an even better facility, which was the NASA Infrared Telescope Facility, IRTF, at the Mauna Kea Observatory in Hawaii. This was much higher. It was approximately twice the altitude of Kitt Peak. It was at 14,000 feet, and that was like going to Mars, really. It was a really different environment. Certainly in those days where it wasn't as populated as it is today.

But in any case, during this time at Kitt Peak I think there was some significant work that was done. The Townes group students had measured CO_2 emission lines from Mars. It may not have been so unusual, because Mars's atmosphere is primarily CO_2. But what were measured were very narrow, non-thermodynamic equilibrium lines. These are lines that were emissions that corresponded to brightness temperatures much higher than the local temperature of the Mars atmospheric environment. They occurred at low pressures from higher altitudes (mesosphere) and were very narrow in frequency, so that they could be used as a measure of temperature in that region by just the width of the lines. The position of the lines could measure velocities.

If you could measure the absolute frequency of these lines in one location versus another location, you could experience a Doppler shift. This is similar to the state troopers, for example, who catch you speeding. They send out a signal, and they know the frequency of the signal they send. And then the return signal is modified by the fact that your vehicle is moving and the light is reflected, or that radiation is reflected, off your vehicle. So they measure the difference frequency between the two using the heterodyne technique. That difference frequency is related directly to the velocity of the vehicle.

Using the similar Doppler shift information, we were able to measure winds on Venus, for example, and later on, on Mars, and then even later, on Titan. Venus, Mars, and Titan. During this time, we analyzed and measured these emission lines even more accurately, and we determined that these lines were actually not only non-thermal, but they exhibited lasing phenomenon. These were actually the first naturally occurring lasers, CO_2 lasers.

Now astrophysicists have measured masers, which are microwave non-thermal emissions, but these are lasers in the infrared, and generated by CO_2 molecules. In fact, the local oscillator, the frequency standard that we used in our heterodyne technique, was a CO_2 laser. And so we were able to use this to measure the CO_2 lines that were in the atmosphere of Mars and Venus.

From both the emission lines and absorption lines in the Mars atmosphere, we were able to obtain a lot of information. The major discovery or revelation was the fact that they had a significant lasing component, both on Venus and on Mars, because both Mars and Venus have predominantly CO_2 atmospheres. The papers were published— ["Discovery of Natural Gain Amplification in the 10-Micrometer Carbon Dioxide Laser Bands on Mars: A Natural Laser", Mumma et. al.] was the original paper in *Science* [academic journal] in 1981, I believe.

And then the other aspect of it. We went to longer wavelengths using isotopic lasers of CO_2, which enabled us to go to different frequency ranges. We were able to measure the polar regions of Jupiter, which exhibited a very strong infrared auroral effect. This auroral effect we measured was emission spectra in ethane gas in Jupiter's stratosphere. That was the other, I think, significant study, and it's a unique phenomenon on Jupiter in the north. It's unlike the Earth where the aurora in the polar region is dictated by the geometry of the magnetic field. On Jupiter it is, sort of, but at these wavelengths the middle infrared has a fixed location. So it's not like you see the Northern Lights here that change shape over time and change location, and even come down to lower latitudes. At some point, you could even see them at latitudes that are as low as Washington [DC]. On Jupiter this is a very fixed hotspot, if you will, and so we have been studying this hotspot for over 35 years. That was the first sort of discovery and targeting of this spot.

Then we moved to the IRTF in 1984, which was twice as large a telescope as the McMath. It was a 3-meter diameter telescope. It was also brand new and very untested. Actually, before that, Dave [David] Buhl and colleagues went there to try to do a submillimeter test and observations. The frequency regions between infrared and microwave are millimeter-wave and submillimeter wave regions. They had a submillimeter laser. This is a laser of much longer wavelengths than the mid-infrared CO_2 lines. They used a heterodyne system at those frequencies to make some studies. They may have been astrophysical, probably stellar sources. Dave, who was also a member of our group, did the scouting of that facility and Hawaii during their run. I guess the most that came out of that first submillimeter effort was some proof of principle, and also a tourist guide from Dave about what to see in Hawaii. That was 1980. It was the first year of this facility. The telescope and their instrument didn't work perfectly then. There were a lot of glitches.

Then we went out there to look at Mars, and to measure hydrogen peroxide on Mars, which has an important role in the chemistry of Mars's atmosphere. We had a graduate student, David [A.] Glenar, and his goal was to do that. There were a lot of funny stories. Like he packed liquid hydrogen peroxide much stronger than what you buy in the store for cleansing, and it came apart in this suitcase, and of course it destroyed his clothes. We had all these stories.

JOHNSON: Well, I was going to ask you, because you are talking about being out at Kitt Peak, and then again in Hawaii—how long of a period of time did you spend out there at those locations? Where did you stay? Maybe share the details of being there.

KOSTIUK: Most of these observatories have dormitories attached to them. At Kitt Peak we each had a room in the dorm. At that time it was a thriving enterprise. Today it's completely different, but they had a volleyball court, and they had pool tables and ping-pong tables so you could enjoy yourself while you are not working.

The weather is relatively nice, even though it's in the middle of the Sonoran Desert. It's about 60 miles from Tucson, Arizona, and it's on an Indian reservation [Tohono O'odham Nation]. It surrounds this particular mountain, this Kitt Peak. It was actually fun. We spent a lot of time trying to enjoy it, and we had great food. You could order a meal. They had a kitchen, and you could order breakfast any way you wanted. If you observe all night, you'd look forward to having breakfast before you go back to sleep. Actually, looking back, although we worked very hard—and we worked out there 14 hours a day at minimum, if not 16, maybe more—it was fairly benign, because it was only at about 7,000 feet, which you get acclimated to relatively quickly. It wasn't as dramatic. But it was quite nice.

Now, the first time we went out to Mauna Kea in 1984, they had what they called dorms. What they were really were army barracks, but very primitive. In other words, there was communal bathrooms and washing sinks, and you slept in these little rooms. The rooms were just big enough for a bed and they had a heat pump, I think, or some kind of a unit in the wall or in the window for heating or air conditioning.

You needed heat, actually. It was pretty cold because the dorms are at 9,500 feet. And as I say, it was very primitive by the standards of today. Later on they built very nice dormitory buildings, and a big cafeteria. At its height, it was very, very dynamic, if you will. But at that time it was relatively Spartan. So we stayed there.

I experienced my first earthquake. I thought it was that a student of mine next door who was making noise in the middle of the night, but it turns out it was a real tremor that went through, which I had never experienced before.

We had to kind of walk up the hill to the kitchen, which was maybe, I don't know, 30 yards or so up the hill. And believe me, you feel it at those altitudes. Now, where you really felt it is when we went to the summit. At the summit, there were only I think four telescopes at the time. Canada-France-Hawai'i [Telescope]; UKIRT [United Kingdom Infrared Telescope]; there was an old University of Hawaii telescope; and the IRTF. Today, that mountain is filled with, I don't know, 20 telescopes or more. It's like a city up there now, but at that time, it was very, very sparse and natural — and there was no paving.

You would go up this mountain road with a four-wheel drive, with a drop-off to the left and right and switchbacks. It was actually quite scary, quite scary. Of course, the people who worked up there, they weren't scared. The drivers would zoom up and down, and make those sharp turns, and of course that would put the fear of God into us. But we got used to it, and of course later we were equally bad, or good. But it took a while.

I have never been so sick or so cold, or saw so much snow anywhere in my life as I did in Hawaii. In 1986 we were scheduled to observe Comet Halley, and it snowed up there for six days. We were at the dorms. In fact, it started snowing when we first got there. We had a post-doc, a German post-doc (Ulli Kaeufl) who went up first to start setting up, and then the day crew arrived. These are people that work every day at the summit, on weekdays, for the IRTF. They said they were told not to go up, so we were waiting for our post-doc, to come down. We were about ready to go try to fetch him—because it was snowing already. Usually it doesn't snow at 9,000 feet, but as you go higher the chances of snow are greater. He came down and he had two passengers with him. It turns out that these two passengers went up there in a little Toyota to observe the sunrise or something, and they wanted to stay up there. He, in his German authoritarian manner, forced them to leave the car and get into his vehicle.

After it snowed for six days, there was 12 feet of snow at the summit. But the facilities folks were prepared. The facilities crew had these snowplows, and they would go out there every single day and plow. On the sixth day when we actually went up, you could see these walls of snow on each side of the road. It was probably the most gorgeous snow I have ever seen in my life.

I had spent seven years in Syracuse, New York where it snowed every day, but it was nothing compared to this—the snow was so clear. What would happen was, in the daytime, it would melt and then refreeze, and it would give you this aqua color to the tops of the snow peaks. It was just beautiful. It's just hard to imagine.

So we were there—you asked how long—Kitt Peak, we spent two weeks, three weeks at a time a couple of times a year. In Hawaii we spent two weeks at a time usually, and it was as much as three times a year. Sometimes maybe more over the years, since 1984.

As to experiences during observing runs—in Hawaii before the snowfall, we also experienced the eruption of Mauna Loa. We actually had trouble getting to Mauna Kea because they had blocked off the road because there was lava flow, and they were afraid of tourists going up there. So we had to convince them that we weren't real tourists.

The Big Island of Hawaii is really two big mountains, Mauna Kea and Mauna Loa. They are of comparable height. Mauna Kea may be a little higher, but it's also a dormant volcano, whereas Mauna Loa still erupts. And the Kilauea Crater is one that erupts, which is part of

Mauna Loa, and it is actually the volcanic eruptions that built the mountain and the Hawaiian Islands.

We used to take a southerly route to go from one end of the island to the other. This is after the observing run. We would take a few days to just decompress, and we would go down the southerly route around the Big Island of Hawaii. All of that southern part, which was a state park and black sand beaches, is all under lava now. [Until very recently] the most recent eruptions started in the mid-'80s and it was actually surprising to us, because we didn't appreciate that that was the case. We landed in Hawaii and we went to a restaurant. We had dinner and it was very late for us, and we were going to go to sleep. We walked out of the restaurant, and we saw this plume on the horizon, across Hilo. And behind Hilo, we see this plume of red molten stuff coming out high against the sky. It was just amazing.

It was like a plume of—well, it was lava of course, these eruptions. So we got into the truck and tried to head for it. We got pretty close, and we had some pictures of it. It wasn't that far from Hilo, which is where we were. That's when we first saw these eruptions. That was one of the unique experiences, if you will. We came to do astronomy, and we got consumed with geology.

JOHNSON: Yes, and quite an experience, I would imagine, compared to what maybe you thought you would have been doing five or six years earlier when you were still in school.

KOSTIUK: Absolutely, absolutely. I never imagined anything like that. Once we got to the summit you could see the lava eruptions from the dorm station. From the cafeteria you could see these eruptions along the limb of Mauna Loa. Of course one took pictures of it. We didn't have digital cameras in those days; otherwise we would have had a million pictures. But we had a few, and so that was the first experience with volcanoes. We'd get very sick at the summit, too.

JOHNSON: Is that from the altitude?

KOSTIUK: From the altitude mainly. And we would overdo. One of our students at the time, Jeff [Jeffrey J.] Goldstein, the winds on Venus young man at the time—he was in good physical shape. "Oh, man, it's great up here!" And they had a weight room, an exercise room, at the level of the cafeteria. He went in it and exercised in spite of our warnings that you shouldn't overexert yourself right away. He really felt bad the next few days, because see, you don't realize it, but when it hits you, it really hits you. Anyway, so we had that experience.

Jeff went to work for the Challenger Center [for Space Science Education], after his Ph.D.—actually he went first to the Smithsonian [National Air and Space Museum, Washington, DC]. Maybe he was still at the Smithsonian at the time. But nevertheless, he was very interested in science education, so he would bring in some students with him. Invariably, the young sort of college athlete couldn't take the altitude at all. They would get sick like anything, whereas the heavy smoker had no problems.

JOHNSON: They are used to low oxygen, I guess.

KOSTIUK: I guess. That's my theory, too, that they are used to not breathing. These are the little nitbits of experience.

Well, we did some good stuff there. We helped them fix a lot of the telescope, because we would be like guinea pigs. As I said, we were one of the first users, so by us bringing our instrument there we would discover all kinds of things that needed to be improved, and I think the IRTF benefited from that. It is a NASA-funded facility that was built to support NASA missions, and ground-based support of NASA missions, and we have been doing a great deal of that.

JOHNSON: That relationship that NASA has with different institutes and different entities to do research is interesting, because nobody can do it by themselves.

KOSTIUK: Yes. And the facilities are extremely valuable educational and professional facilities. For example, the first operator that we had at the IRTF—he eventually was the director of managing all of Mauna Kea facilities. One of our telescope operators wound up working at the South Pole, and loving it.

So it was like a stepping-stone for a lot of professional individuals, but it was also a great learning experience for students and graduate students, because even today it's one of the only telescopes of reasonable size that permits for you to bring your own instruments. Where you can actually work and learn how something works, and build and test new ideas. Most of the big, expensive observatories don't allow you to do that. Even at the IRTF, most of the work is done on so-called facility instruments. Such instruments are built under contract or by somebody within the institution, and it becomes a facility instrument. Somebody at the facility helps operate it, and you don't even have to be at the telescope.

This remote observing is now very popular, and in fact has contributed to the decrease in the population at these dormitories—that were very nice—that were built a couple of years after we came the first time. By '84 I think they were built, very nice dorms. Now there is hardly anybody there. Last time we were there, last year, very few people were there compared to before when the dorms and the cafeteria would be packed.

Something you might want to do—this is another story that I can tell you. Google [internet search], on YouTube [social media video site], "Hotel Mauna Kea." It's a parody written by one of my former students and now colleague, and now our funding person—or non-funding person—at [NASA] Headquarters [Washington, DC], Kelly [E.] Fast. She was a student at [University of] Maryland [College Park]. She worked with us for many, many years. She, with one of my other students, Juan [D.] Delgado, who was a guitarist, put together this video that basically describes life on the mountain. It's "Hotel Mauna Kea.[1]" There are other videos. There's "Born to Heterodyne.[2]" That's all Kelly, and that was another video. There is a whole series there on the web. I think they called us Photomixers. But you might want to skim through that and see, because that'll give you pictures of some of our activities and individuals, both at the IRTF and elsewhere.

Actually, this "Hotel Mauna Kea" has a story. I'll tell you another story. This was about—oh, I don't know—about 2003, 2004? Kelly drummed this up, she put this on YouTube, and it became viral. Many people started watching this. I guess the person that they recognized on that was me, because I was, I guess, better known to the astronomers. *Science* magazine decided to do a story on us, apparently. So I get this call from *Science* magazine asking me

1 https://www.youtube.com/watch?v=XPdTlHK1h_0
2 https://www.youtube.com/watch?v=mtQpRkA8yQA

about this, and I tell them I didn't do much. I mean, sure, I am the only other voice on that video, but it was all done by my colleagues. So he says, "We want to do a story on you."

What they arranged is for a photographer to come, and they came and took pictures of us at Goddard with guitars, and then they published this in *Science* magazine. I told the reporter, I said, "What do you want from me? You just rejected one of our papers, and now this little song you want to do an article about." That tells you what's important. The song is okay, but science, "Oh, that's not worth publishing."

JOHNSON: No, that's right. Entertaining.

KOSTIUK: I used to play that video at the end of talks, especially in Europe. When I would give a talk, I would show them how it is to do the work I reported.

JOHNSON: I'll definitely go out there and look at that. That should be entertaining.

KOSTIUK: It will be, it will be. It's quite good. Kelly has a very, very nice voice, and Juan is a real good guitarist. Juan, by the way, we discouraged him from astronomy. He is just about to get his Ph.D. in Holland on how plants generate electricity. But in any case—this is another side story.

We had other events at Mauna Kea. A notable one was the impact of Shoemaker-Levy 9 comet into Jupiter. That was a major, major event, a major program. We were part of that program, the organized program. We had [observing] time days after the impact, and there are publications on what we looked at. We managed to detect ammonia in the atmosphere of Jupiter at the location of the impact, and determined its distribution and the fact that so much of it was there that it's probably not from the comet. One of the interesting aspects would be to see, whatever is detected at the impact point, which of it came from the comet, how much of it came from the comet, and what came from the comet versus what came from Jupiter.

I think we found so much ammonia that certainly a great deal of it had to have come from the interior of the planet, telling you that it had to penetrate down deep enough to eject a lot of the ammonia into the stratosphere, which is where we made our measurements, the upper atmosphere of Jupiter. The reason that's unique is because ammonia is very easily photodissociated. Its lifetime is very short, so that's why there isn't any in the stratosphere of Jupiter, even though there is quite a bit of ammonia deep down in the clouds of Jupiter.

So we were able to measure that, and along with another group that worked out of [University of California] Berkeley, Al [Albert L.] Betz and [Rita T.] Boreiko, they had also made heterodyne measurements. Al Betz was one of Charlie Townes's students. They made heterodyne measurements days after, so together with them we published—Kelly Fast was the PI on the second paper, where we looked at the temporal changes of ammonia after the impacts on the various impact sites. It's sort of a revisit of Comet Kohoutek with much more visibility, if you will, or impact than Kohoutek had—other than getting me a job.

JOHNSON: Which is important. I noticed on your resume, you we a part of the IRTF Telescope Allocation Committee.

KOSTIUK: Not anymore. I was through 2015. You have to write a proposal to get time on the telescope. This TAC [Telescope Allocation Committee] makes decisions and selects who should be given time, because there is a limited amount of time available during any semester. That was my role, to contribute to that.

JOHNSON: About how many people can be up there at one time?

KOSTIUK: Well, it depends.

JOHNSON: Well, as far as the facilities and what they would hold, each experiment would take up different amounts of time so that would have to be scheduled out.

KOSTIUK: Exactly, and that's exactly right. Some people just need a couple of hours. In fact, if you are on a facility instrument, you can just ask the telescope operator to take the data for you. Or you could do it remotely.

In other words, now what you could do is just almost sit in your office, or at your institution. Like in Goddard there is a facility, which is connected to the IRTF. It's the same as being at the summit and looking at a screen, just like you would be there, but you don't have to set anything up. You don't have to do anything with the instrument.

When we go out there, we have our own instrument. We have to set it up, so we have to be physically there. The downside, in our case, is that when we ask for time we need the full night, every night, almost exclusively. Sometimes we can share it. We, in fact, did share the night during the SL [Shoemaker-Levy]-9 impact, but usually that's because our instrument has to be mounted on the telescope, and then aligned, and we have to actually physically work it.

We can't do it through the facilities of the observatory. But even if you use facility instruments, you need to have at least two people at the summit at the same time. There are occasions where only one telescope operator and a helper go up. The helper is some student they hired to be up there in case of an emergency, because at these altitudes you can't be alone. Now, in our case, we have had as many as six people up there as part of the group, like that snowed-in Comet Halley event.

I didn't finish that story, though. I've got to finish that story. Well, we had 12 feet of snow. We made it to the top at the end. It was a problem because there was a traffic jam. When it snows on Mauna Kea—do you know "Mauna Kea" in Hawaiian means "white mountain"? You can be lying on the beach, and you see this white mountain in the background. In fact, I have pictures like this.

When it snows, the locals go up there and they load the truck full of snow and bring it down. So there was a huge traffic jam at the time because they go up there, and then people ski, snowboard now in recent times, come down on cafeteria trays or anything else. It's a little suicidal, of course, because the rocks are very jagged and if you hit one of them it's not very pleasant.

Mauna Kea, by the way, is ski-able. There used to be a rope tow out there at the summit when we first went up there. Near the cinder cone where the IRTF is, there is a little valley or a depression, and there was a rope tow from there to the top. I was told there was skiing, and in fact you can go on tours to ski Mauna Kea. I will tell you another story about that, but let's finish this one.

We went up there and we finally got up there. Of course everything was snowed in, so we had to dig ourselves into the dome, and the day crew went up and they shoveled the snow off the roof of the observatory. We opened the telescope, and we got our instrument up in two hours, which is incredible. Usually it takes us days to get everything working together. It was our last day. We didn't have another day or time on the telescope.

Then it turns out that we hear some grumbling over the mic [microphone], and the telescope operator says that the telescope tracking system is down and he couldn't get it up. So we went up there for nothing. Even though it was cloudy, it looked like it opened up where the comet was supposed to be, but we never got to point it or use it. So it was a total failure. But I remember 12 feet of snow.

I have tons of Mauna Kea stories. Remember, we have been going out there since 1984, and the last time was last year so you can count up how many years that's been. It's been at least once a year, sometimes as much as three. We stay there each time about two weeks total trip, so it's over a total of 2 years on Mauna Kea, during which many things can happen.

Now, just to finish up with the IRTF TAC. So that's what the TAC does, it selects the proposals that would fit into the schedule. They also look at the value of the science proposed. There is also some committed science, or director's science. Like there is a long-term program to monitor near-Earth objects. You have to have time for that.

And sometimes there is a caveat that if you are observing and one of these objects all of a sudden appears, then the priority is that you can move your instrument aside—and that's possible now—and try to use the facility instrument to do the required measurements on something like an asteroid. So the IRTF is a valuable tool. It's always under threat because of budgeting and all, but it's really a unique tool for astronomy, and for technology, and learning for the younger generation. It's quite important, I think.

JOHNSON: Yes, I agree. It's a unique part of NASA that people don't always think about.

KOSTIUK: Exactly, exactly. You think of spaceflight, but you have to do the groundwork to generate a good spaceflight proposal, or a good proposed mission.

Also, you can do unique things from the ground. We have supported a lot of missions. For example, in the case of Voyager we made measurements of Neptune's thermal profile and ethane abundance prior to the Voyager flyby. Then we had a comparison between ground-based and spacecraft results. Our measurements are at a much higher spectral resolution. For example, to build an instrument that would see these Mars non-thermal emission lines or lasers, or measure winds on Mars from a spacecraft—that's yet to be done. Yet from the ground, we have maps of wind patterns on Venus, on Mars, at altitudes that the spacecraft would not be able to do. So there is a lot of complementary effort.

In the case of the Juno mission recently, Juno does not have a capability to measure Jupiter's thermal infrared aurora. Although auroral studies are one of its goals, Juno doesn't have the capability to measure aurora in the thermal infrared. This is in a 10-micron region of the spectrum, like in the hydrocarbon emissions.

As I indicated, this particular aurora is quite unique because it has this hotspot in the north, and we have yet to understand how that's possible. From the ground this can be done with instruments, and our instrument in particular. Unfortunately, you are limited on a spacecraft what you can fly.

JOHNSON: Right, weight and everything else.

KOSTIUK: Exactly, exactly. So that's another very valuable aspect of ground-based facilities, especially such as the IRTF, which allow you to do a great variety of things.

JOHNSON: Has the majority of your work been with ground-based observatories? I noticed on your resume there are things like the Infrared Astronomical Satellite [IRAS]. That was in 1974 and launched in '83, where you were working on that as a co-investigator.

KOSTIUK: It might have been a proposal. I also worked on the preliminary planning for the Earth Science mission [Upper Atmosphere Research Satellite, UARS]. It was the major mission that did Earth Science. We also did Earth Science work during the '80s where we looked at chlorine monoxide from the ground. Chlorine atoms, are major culprits in destruction of ozone. Chlorine monoxide was the radical that was a catalyst in this chemical process. Resulting in destruction of ozone, but not chlorine.

That's the danger of having chlorine atoms in the atmosphere, because they don't get destroyed. The ClO [chlorine monoxide] is one of the major constituents that needed to be addressed, and there were measurements made depicting abundance values that we just didn't see.

That was a controversy, by the way. We really didn't see as much ClO. We could measure very, very fine spectral features from molecules, and we just didn't see chlorine monoxide from the ground in the Earth's atmosphere with a very, very bright source—the Sun shining through the atmosphere. There was a Mumma *et. al.* publication in *Science (1983)* on that. "Is There Any Chlorine Monoxide in the Stratosphere," That was very interesting because balloon measurements, which were grab samples and relied on chemical reactions of the sample—they would put the sample through a chemical reaction that they believed they understood—would get values that were much, much higher. So that created some controversy.

Then we did a diurnal measurement of chlorine monoxide. This was done from Kitt Peak. We had real difficulty publishing the results. We eventually gave up. We tried to publish the results, and we had, I think, very inappropriate reviews such as, "The instrument isn't sensitive enough." We were looking at 6,000-degree source, whereas our instrument is sensitive enough to measure 150-degree source like Jupiter, so it didn't make any sense.

Again, it was a controversy. It later sorted out to some degree, but many years later. There was so much investment in one approach, one theory, that it's very hard to change that. Years later I think I even got some interesting papers addressing this that were sent to me. This is when I long forgot about this.

But we did do some laboratory work to measure the parameters of chlorine monoxide, which helped alleviate some of the differences in the discrepancy. Nevertheless, that may still exist today, although we no longer have—quote, "have"—the ozone problem like we did in the mid-'80s.

We submitted two proposals to the UARS to look at chlorine monoxide and ozone. It was kind of interesting because we wrote two proposals. There were actually two different proposals. A JPL group was the PIs on one, and we were the investigators of the other. Just to give you an idea of how times have changed, in 1978, we had no email, we had no [Microsoft] PowerPoint [presentation software], we had no texting, nothing. We wrote these proposals in about a week. This is a mission instrument proposal. It was just me and another colleague, because it was left to me to do. And another colleague here, and the colleagues at JPL. We exchanged by FedEx [Corporation, shipping service] or overnight mail, and faxes. And telephone, of course. Those were the major communications.

We were able to put these together, and they were deemed to be decent proposals. The technology was not maybe quite there yet, so that was kind of interesting. Whereas today an equivalent mission proposal costs, I don't know, $40,000 and I don't know how many personnel at Goddard would help write it. It's become much more competitive, and the quality of the proposals is much greater, and cost estimates are much more involved and complex. It's not "faster, better, cheaper," that's for sure.

JOHNSON: There are a lot of things about spaceflight that have definitely changed over the years as far as the cost and the time involved to get things done.

KOSTIUK: Absolutely, absolutely. And some of it is positive, but a lot of it is just—it just doesn't make any sense.

This kind of reminds me of the opening of the West. I was fortunate. I have to say I was fortunate that I had the opportunity to work at NASA during the end of its pioneering age, where you had an idea like the Comet Kohoutek instrument, and then boom, we could do it. Within months we had something. It would be virtually impossible to do something like that today.

It's like in the Old West. The early pioneers had open plains. They had cattle, they had open range, and then over time the fences went up. You had regulations, you had water rights, you had barbed wire, you had this and that, and eventually no more pioneering. Unfortunately, I think that's really an analogy as to what happened here. We are a mature agency, which is really more bureaucratic, certainly than it has been, and more bureaucratic than I think it should be for the kind of mission that it has.

Yet, as I say, I am fortunate that I was able to be there. When I needed something simple machined, I would just go to the shop, and they would do it. Now I have to give them a number and a detailed computer-generated engineering drawing. Even then, it's not clear that they will have time to do it.

JOHNSON: And when you first started there, when you did work on a proposal for something to be done, like the Kohoutek—as you said, it usually happened quicker, and it was less formal. Compared to now, when scientists are putting in proposals for work that they want done, what's the percentage of the ones that were accepted back then compared to now? Are there a lot more now and the processes are longer?

KOSTIUK: Yes. Well, first of all, there was less competition in a way, because there were fewer people working on the same projects, the same kind of work. You could look at the statistics of just R&A proposals, for example—Research and Analysis-type proposals. I think in the old days a success rate of 30 percent or more was not unusual.

In fact, even when I was at Headquarters—I managed the Planetary Instrument Definition and Development Program in '93 to '96. It was the most competitive program because it required more money, but it was a fairly substantial percentage that got funded, like 30. Now, very often a typical percentage is 11 percent. It's like a crapshoot. You have lots of very good proposals that are not supported, mainly because there are not enough funds to support all of them. The competition is much stronger, and we also make decisions with panels, which often are limited as to expertise in the broad range of science proposed.

Panels have their strengths and weaknesses. If you get the right panel, you always win. If you get the wrong panel, no matter what you do, you are not going to win. That's the way it is. It's just part of the game. So the percentages are much lower, as I say. On many of the proposals that we submitted recently, and in panels that I was on, a very low percentage is successful. It's very competitive. One of the reasons—less money and more potential participants.

The other aspect of this is also, I think—and I have to say this — when we went to what's called full-cost accounting at NASA, that was probably the least beneficial thing that was ever done. Because it wasn't really full-cost accounting. In other words, right now you have to account for all of your time and all your money, supposedly. But of course, Congress allocates salaries for every single civil servant. So even people that aren't successful, they still are funded. They still get paid. It's not like we fire them.

But what it does is it creates a situation where, well, not only is morale affected, but where you can't really do what you want to do. In fact, just recently—I come in as an advisor here. I spent yesterday, half a day, trying to solve an FTE [full-time equivalent] problem for an engineer that is here. It wasn't much of a problem. It wasn't really a problem, but because of all this institutional bureaucracy, it's a problem. This is not conducive to an agency that's supposed to deal in research. It might be more appropriate for a lot of government agencies, but not ones that should be focused on missions and research.

Over the years there have been a lot of efforts to ameliorate these conditions. In the early days, as you asked about—one of the advantages was that a civil servant cost an additional

$11,000 at most per year at the Center, and the rest you had to get: money to support only your research associates, your university colleagues, your post-docs, needed equipment and so on. It made it possible to do a lot of work. Now you have to not only get support for your outside colleagues and those at universities, but also for the civil servants, and civil servants are even more expensive. So anyway, everybody recognizes the problem, it's just that nobody can do anything about it.

JOHNSON: Right. So the proposals or the funding nowadays have to cover the civil servant salaries also?

KOSTIUK: That's correct. Let me tell you, a typical civil servant's salary coverage is of the order of $200,000 to $250,000, some number like that. But the most you can ask in an R&A proposal is about $150,000. It's different from missions, where you have many millions of dollars. The most you can ask in a normal R&A proposal, such as observing proposals like we have been doing, is not enough to support the people that I was able to support in the '90s, like the students and all, I couldn't do that today. It would not be possible. Yet, in the past all these students came out for the better. As I pointed out, one of them is running one of these programs.

JOHNSON: What is the impact of not being able to involve all those students in these programs now, do you think?

KOSTIUK: Well, there is a dual impact. One impact is, of course, you get less labor, and less very innovative ideas. I have to admit that most of the success that we had, in the '90s and so on, a lot of these SL-9 results and all would have been very, very difficult to achieve without student and post doc help. It might not even have been possible.

It also affects the individuals themselves, the students. They don't have the same opportunity. They would go into doing something else, like do computer programming, or go in some peripheral field. Which for some might be okay, and for others it would be, I think, a loss because of their talent. Some of them may go and manage programs instead of doing the research. That may benefit the program management, but it takes away from the research community. One has to choose what one thinks is important.

JOHNSON: I would think that it would discourage people from wanting to work in research with NASA if the positions or the opportunities aren't there like they were for you.

KOSTIUK: Absolutely. I was extremely lucky I have to say. Remember, I came in doing something completely different. I had no clue that I would be involved in anything like this when I first came.

JOHNSON: That's interesting, and kind of sad at the same time that we're missing those opportunities now. I assume it's because of funding issues that everybody else is going through.

KOSTIUK: Well, and the overall sense of needing control. In the military you want to have control, but when you are doing research you have to relax. People do, to a degree. The management, the Science management, understands all this. The point is it's not a formal acceptance.

It's different, it's different. It drives some people away from NASA, and if they can get an equivalent position at a university, they would go. And they have. As I say, they could go to Headquarters and manage the programs that they don't like. It's a choice one has to make, or just go in a different field. That happens, too.

JOHNSON: You mentioned the military, and it just brought to mind one of the things I read about. This was back in '80 again. It's on your CV [curriculum vitae] that you were an organizer for the NASA–U.S. Army Workshop on Sub/Near Millimeter Wave Technology.

KOSTIUK: It could be. Was that in Maryland?

JOHNSON: At Goddard, it says.

KOSTIUK: At Goddard? Yes, it could be. It could be. Yes, we did. We did have it, because we worked mainly in the infrared, but looked at extending the techniques and investigations into the longer sub-millimeter spectral region. Interfacing with DoD [Department of Defense] research efforts was important. At that time, people working in the infrared had to have secret clearance in order to have access to the latest technologies, many of which are no longer classified at all. I did have a secret clearance—I had to have it—and that enabled me to get information. I remember even in our building, in Building 2, we had a room with a file cabinet that was locked all the time. And the guards had to check every day.

At that time we had the other infrared group here that eventually went into the astrophysics areas. Mike [Michael G.] Hauser was the head of that, and John [C.] Mather was one of the members of that group. John Mather, as you may know, is the Nobel laureate in physics a few years back. He was the PI on the COBE [Cosmic Background Explorer] mission. They were all part of that group that had access to these materials, and I think that particular group was responsible mainly for this filing cabinet. So we did have more, but I think that's pretty much passé now.

In fact, these mixers that we use, these detectors that generate these difference frequencies in the infrared in our heterodyne spectrometers, those mixers were developed for military use. Those mixers were classified mixers at one time, and that effort—I guess it was for forward-looking radars and all that—but they don't do that anymore.

It's hard to even get those devices anymore, so really to continue or enhance our work we would need either a huge infusion of money or just use what devices we have until they die. Because a lot of the technology is no longer supported. It was mainly supported in the past by military needs, by DoD needs.

JOHNSON: Let's talk a little bit about the international component of doing science and research. Looking through your CV, there are a lot of things that you worked on that involved working with the German Planetary Telescope Science Working Group, for example, or the Solar and Heliospheric Observatory, which was in collaboration with ESA [European Space Agency]. Working with these different international groups and other space agencies in other countries—talk about that for a little bit, that relationship and how some of that worked. The [Cassini-] Huygens probe is another example.

KOSTIUK: Huygens, yes. In fact, support of Huygens was a major, major effort.

JOHNSON: Well, talk about that. And some of the international components of doing this kind of research.

KOSTIUK: Yes. Well, we did have quite a few. For one thing, there was a group in Cologne [Germany], which contributed two post-docs to us, both of which made major contributions. They were [National Academies of Sciences] NRC [National Research Council] post-docs that were major contributors to our current instrument, HIPWAC, Heterodyne Instrument for Planetary Wind and Composition. Frank Schmülling in particular. This is the instrument that we currently use and used to make measurements of Titan, Titan's winds, prior to the Cassini mission and during the descent of the Huygens probe.

These measurements we made were unique in the following sense—they started out as a kind of a practical need. You see, Titan is a moon of Saturn. It's got a very dense atmosphere. Well, how dense? One and a half times the density of the Earth's atmosphere. It's very, very similar to the Earth in another way, and the other way is that its atmosphere is predominantly nitrogen, much like the Earth's.

However, it's so far away from the Sun and so cold that oxygen is virtually not observed, or cannot be present in gaseous form. In fact, it has a hydrologic cycle—or kind of a cycle—of liquid, rain, and lakes, but the rain and lakes are predominantly hydrocarbon gas, liquid gas, like methane and ethane. The atmosphere—it's got a stratosphere like the Earth. The stratosphere is formed by minor molecular constituents that absorb solar UV [ultraviolet] and then emit in the infrared, and they warm the atmosphere. On Earth it's ozone. But on Titan these molecules are hydrocarbons, methane and, ethane and perhaps others. But again, most of the atmosphere is nitrogen, so there are also nitrogen hydrocarbon compounds that are present there.

During this Cassini mission and the Huygens probe, which was a European ESA contributor to the mission, there was one issue. They had to release the probe so it goes into the atmosphere of Titan. As it enters the atmosphere, you would like to track the probe from the orbiter as it gets to the surface. However, if there is a strong wind in the atmosphere it's going to drive the probe one way or the other, so you have to have this sort of target ellipse such that you can monitor the probe until it lands. You have to release it in such a way that whatever winds are present, it won't drive the probe into eclipse behind the moon before landing. Then you can't see it, so then the orbiter will lose that information. Communication between orbiter and probe to the surface was the mission goal.

The original timescale for this probe release and entry was such that this became a very critical issue. It turned out that later on because of other changes it was less of an issue, although it was still important. But it was critical at first.

So how do you measure winds? Well, they have data from Voyager IRIS [Infrared Interferometer Spectrometer and Radiometer] instrument where they look at the temperature map, and from the temperature map they could determine how strong the winds are. If you know you have a certain temperature at the east limb, and a certain temperature at the equator and the west limb, you can determine a wind field from models.

The other unique aspect of Titan is it's one of two objects in the solar system with an atmosphere that rotates very slowly. The other one is Venus. Venus rotates on the order of 250 Earth days for one year, so it's very slow. Titan takes about 19 days to rotate. All the other planets and objects with atmospheres we know have much faster rotation periods—like the Earth is 24 hours, Mars is approximately 24 hours. Rotation periods of all the giant planets are of the order of 10 or 20 hours for a single day.

Theories of atmospheric circulation on these objects are reasonably developed. Whereas Titan's period is much longer, so the physics of coupling between the surface and the atmosphere is not well understood. We don't have many examples where theories about the so-called cyclostrophic circulation can be tested. One is Venus. Of course we know a little bit about it, but we didn't have another example.

Titan is another example, so it was very important to measure the winds. In fact, on the Cassini-Huygens probe there was an experiment called the Doppler Wind Experiment, which was a radio experiment to measure the winds as the probe descends. But again, that's only when the probe is descending, and we need information before that.

Well, as I mentioned earlier, if we can measure molecular emission lines and determine their absolute frequencies from Doppler shift, we could determine the wind magnitude and direction, which is something these thermal maps cannot do. The thermal maps can determine the magnitude of the wind, estimate from theory, but they can't determine which direction the wind is blowing. It's a complex theory, a thermal wind equation is used, and I don't fully understand it myself.

In any case, it was important to measure the direction of the wind. And we had that capability, because if we could measure an emission line of ethane on the east limb and an emission line of ethane on the west limb, and then compare their frequencies, the difference in the frequency between east and west would give us information on the zonal wind going around the globe of Titan.

It would tell us not only the magnitude of the wind, but also its direction because the Doppler shift tells you which direction the wind is going. If it's approaching, the frequency of the lines would be shifted to higher frequency. If it's receding, it would be shifted to lower frequency. You could tell that, and so we said, "Hey, we could do this."

We tried it at the IRTF. The problem is that Titan is very small. It's actually even smaller than our field of view on the sky with the IRTF, the three-meter telescope. The three-meter telescope diffraction-limited field of view, that's the smallest field of view you can resolve with a coherent instrument like ours, with a heterodyne instrument like ours, is one arc second on the sky, and Titan is eight-tenths of an arc second in diameter. And so we tried anyway. We looked at the east limb and the west limb—we only nipped a portion of Titan in both cases, but most of our field of view was the sky. So the amount of signal we got was very low, and it took us a week to measure the lines to get reasonable signal-to-noise [ratio] so we could get some information. And we did. We got some information as to the direction, but it wasn't very good.

To do a better job we needed to go to a telescope that was bigger, like a 10-meter or an 8-meter telescope. We were fortunate to get on the Subaru Telescope, the National Astronomical Observatory [of Japan] telescope Subaru, which is also on Mauna Kea, on the next cinder cone next to the IRTF. We could take our instrument and put it on Subaru.

We put it on Subaru. It's an 8.2-meter telescope. That means the diameter of the beam on the sky, the best we can achieve, is now about one-third of what we had, so it's like a third of an arc second. It's a third of an arc second as opposed to one arc second, and Titan is eight-tenths of an arc second. So now even when we go for east limb and west limb, hopefully most of the planet or Titan will be in our field of view, and we would get very little sky.

And so we got time on Subaru. The Subaru Telescope Director at that time (Hiroshi Karoji) was very, very responsive and supportive, and the Director of the IRTF, Alan [T.] Tokunaga, a long-time colleague, was able to encourage him to support us. We got time to do that a year before the actual encounter.

One night, we had data that was better than what we got within the two weeks at the IRTF, so it made a big difference. We got results that were consistent with what we had before, but with a lower uncertainty. This gave us the fact that the wind blows in a prograde direction—that's in the same direction as rotation—something that was possibly anticipated by theory, but needed to be proven, and so we were able to do that.

During the descent of the probe we wanted to make measurements at exactly the same time, but unfortunately we had a snowstorm, and then a blizzard before. Right at the day of the actual descent, we were out there. We had huge winds that prevented us from tracking and opening the telescope. We got some data the day after, and they weren't really great either, because the conditions were still not great. But the snowstorm—again, just like the Comet Halley event, all the momentous measurements we were supposed to make were hindered by snow and weather. That's the price you pay to observe from the ground.

Anyway, we did manage to get some limited data. At that time, we were in constant communication with the ESA folks—Jean-Pierre Lebreton, who was the Project Scientist for Huygens. He was one of our major supporters, and we had communication with them, and with the mission. Also with the Doppler Wind Experiment folks.

As a side story – the Doppler Wind Experiment, which was on Huygens, Mike [Michael K.] Bird, who is an American but he is part of the Astrophysical Institute in Germany [Argelander-Institut für Astronomie, University of Bonn], he was the PI on this Doppler Wind

Experiment. The idea was to make measurements by radio signal from the lander, and for the signal to be picked up and compared to an ultra-stable oscillator on the orbiter, then relayed back to Earth. Then you could monitor the change in frequency of the signal from the probe, the Doppler shift and wind.

Well, as all the best-laid plans of mice and men, there was an issue. I'm not going to go into that, but there was an issue with the oscillator on the orbiter, so they couldn't do that. But they salvaged the experiment by using the larger Deep Space Network telescopes on the Earth to pick up the signal from the descending probe into Titan. Those measurements from the various Deep Space Network tracking observatories on Earth—they used that data to determine the Doppler shift and get a velocity profile with altitude on Titan.

So the Titan Doppler Wind Experiment on Cassini-Huygens was a great success, and Mike Bird and I actually are good colleagues in this regard. He has been very supportive of our work, particularly since we measure the wind velocities much higher in the atmosphere than the probe does. The probe measures only below 120 kilometers above the surface, and we measure around 200 and more kilometers above the surface. From that, we have actually a wind altitude pattern.

[Some of this work is described in a book that came out on Titan, "Titan from Cassini-Huygens". I think it was a Springer Press book, I am not sure. I think Mike actually put in our chart of velocity in his chapter. And we have a several papers you could see there on this work. So that was, I think, probably, a prime example of how ground-based measurements supported a major mission.

This observing program was one of the most difficult and rewarding in my career. It was made possible by a superb team of close colleagues (Tim Livengood USRA/GSFC now UMD, Tilak Hewagama UMD/GSFC, Kelly Fast and David Buhl GSFC, and Frank Schmülling and Guido Sonnabend U. Cologne, both former NRC/GSFC, and engineering support from John Annen GSFC) and Juan Delgado (UMD). We had to interface our instrument HIPWAC to the Subaru telescope, learn and execute the observing procedures, and ultimately reduce, analyze and interpret the results. Results were published in Geophysical Research Letters and Journal of Geophysical Research in 2001, 2005 and 2006.]

JOHNSON: Again, I think people generally don't think about the ground-based work that's being done to support these missions.

KOSTIUK: Right. Then I had several international conferences that I helped to organize and attended as invited speaker.

JOHNSON: Do you want to talk about those?

KOSTIUK: The most recent meeting was in 2015, organized by a colleague at the European Southern Observatory in Santiago, Chile, specifically working on ALMA. I was on the organizing committee. Its goal was to emphasize the importance and complementarity of ground based studies from world observatories, especially ALMA to space-borne investigations. [Ground and Space observations: a joint venture to Planetary science, March 2-5, 2015, Santiago, Chile]

There was the DPS [Division of Planetary Science] meeting in Baltimore, Maryland, in 1985 and the DPS meeting in Washington, in the Washington area is another example. This was in 1994 maybe, something like that. That was after the Soviet Union broke apart. The community wanted to support scientists from the former Soviet Union to come to this DPS meeting, and there was a budget for that. I was the one to make selections as to who we'd have. We had several Russians, Ukrainians. There was an invite to Kazakhstan. These were a couple of DPS meetings, there were several. So I built up a collegial relationship with scientists from those countries—but I had contacts before, during Soviet times yet. I could tell you about that, too. This was another international role, if you like, that I played.]

Primarily in Ukraine. I had colleagues there. Actually, we had a student [Mykola (Nikolay) Ivchenko] who came here in the mid-'90s, a student who was a sophomore in college at the time. I get a call from our Directorate Chief. He says, "Ted, we got this Ukrainian student. Would you like to have him?"

I said, "Sure." I'm of Ukrainian background, so that's why he called me. I said, "Great, we'll have him come here." So he came and I tell you, this student—oh, and he won some kind of an international science competition— he was one of the winners. This is in the mid-'90s. He was funded by a—this is another interesting story—I think it was a German inventor. Some German fellow who set up a grant for supporting several students from around the world at NASA. The student won this competition or this award.

They directed him to me and I tell you this kid, he was sharper than most of the post-docs that we had here. He really was a sharp young man. But we may have discouraged him from planetary science. He went into space physics. He may be in England now, or in Sweden someplace.

But the best sort of international contact was a Conference on InfraRed Physics (CIRP) that was run by a professor Fritz Kneubühl from ETH [Zurich], the Swiss Federal Institute of

Technology [Zürich, Switzerland]. I think 1984 was my first trip. I went to that conference; I think it was an invitation. I went to that, and this is an interesting story.

We are in Switzerland. Remember, this is dark Soviet times. We are in Switzerland, and they had a Soviet delegation there. I looked at the abstracts, and there were a couple from the [V.E. Lashkaryov] Institute of Semiconductor Physics in Kiev, in Ukraine. "Oh, that would be interesting."

We are at the first poster night. This is the first night we are there, and they are serving wine there. We are at the posters, and among the dignitaries there was Charles Townes, and we were friends. As I mentioned, his students did the heterodyne work, and I had visited him before so we had met. So here I was talking to Townes in front of a poster, drinking my little glass of wine.

I look around, and there is this fellow with bushy eyebrows, and he is staring at me. Then our eyes met, and he walks right over to me. He says, "Hi, I'm such-and-such." He says, "How does an American get a name such as this?" pointing at my nametag.

I said, "What do you mean?"

He says, "Well, how does an American get a Ukrainian name?"

I said, "Well, Americans have all kinds of names. They come from around the world." I said, "But you have a Ukrainian name, Malyutenko." I did that intentionally.

He says, "Well yes, but of course." Then we chatted, and I asked him—or I may have said something in Ukrainian to him, and it shocked him a little bit.

But then he got really friendly, and so we are talking. We had to take a bus to the hotel, so we are in the bus. We get in this bus. He sits behind me, and I sit in the front. He says to me, "Look, I will speak in English, and you speak in Ukrainian, and let's talk." Well, do you understand why? Because all the Soviet scientists that came here, at least one of them was a monitor. When they came to Goddard—we used to have Soviet delegations here—you knew who the monitor was. The most prominent scientists usually kept quiet. They had a person who was more fluent in English, and he was also the one that did most of the talking. But he was probably the least recognized scientifically, through papers or whatever.

So the reason he wanted that—I assume and I gather that so that anybody listening to us, what we're talking about, wouldn't be able to fully understand. It was really funny. We had this conversation, and then after that he came to the next CIRP meeting.

Oh, and then the other story, I don't know if this is—well, it's history, right?

JOHNSON: Yes, it is.

KOSTIUK: This is NASA history. Sometime halfway through the meeting, there is a boat ride on Zürichsee, on the lake, Zürich Lake. He comes to me and he says, "Ted, I want you to be our guest, my guest."

I said, "Where, on the boat? But we are all going on the boat."

"No, no, no. You come with us. You come and be our guest."

I said "Okay." So he finds me on the boat. We are off on this boat, and he takes me to one little corner, and there is a whole delegation from Eastern Europe there. There was the fellow from the Czech Republic, there were several Russians, there was him. He was the only Ukrainian guy there. There was a whole group of them. This turned out to be very typical.

For me this was the first experience, because I have never met any scientists. I have met others, but never a scientist. These guys open up their little attaché case, they pull out smoked fish. This was sturgeon, smoked sturgeon, I think it was. They had baguettes, and of course they had little bottles of various alcoholic drinks, like vodka, and I think they had what they called cognac, too. I forget now.

All of a sudden I got a little nervous, because what if they throw me in the water. I drink all this and they—I have reasons to fear that. So there they are, and we are having a great conversation. It's all in English, of course, so I could understand everything. It was quite an experience. As I said, I was a little nervous. "What do they have in my drink? What do they want from me?"

That was my first experience. I met him, and in fact years later several times. He came to visit Goddard. He won some grants from a DoD-funded institution years later—Kirtland Air Force Base [New Mexico], I think—and he actually gave me some devices to test. We tested his little black body sources for our instruments. I don't know where he is now. I think he emigrated to the U.S. somewhere, this guy.

He was the one that arranged my first visit to Ukraine. This was in 1990. It was still the Soviet Union. I can write a book about that experience. I gave talks at the Institute of Semiconductor Physics, and I gave talks at the Main Astronomical Observatory of the National Academy of Sciences of Ukraine [Kiev, Ukraine]. Then, years later, there was this foundation funded partly by [George] Soros and partly by the USAID [United States Agency for International Development] for supporting scientists from the former Soviet Union for different things, research and for conferences. The Director [Academician Yaroslav Yatskiv] and colleagues from the Main Astronomical Observatory wrote a proposal for a conference in which I was a co-I [co-investigator], and they won. This was, I think, in 2000.

JOHNSON: Yes, I see that here on your list.

KOSTIUK: Yes, that was in 2000. That was Astronomy in Ukraine [2000 and Beyond: Impact of International Cooperation]. I helped with that proposal. I was the American PI, because you had to have an American co-I. I was the American co-I, or PI, on that. It was a very nice, interesting meeting.

Then I was also involved in two others—one I think I was an organizer for, and the other I was just involved. Those are NATO [North Atlantic Treaty Organization] conferences. NATO has a program for funding conferences on various topics at either member states' countries or associate countries.

There was another fellow [Michael Mishchenko] at GISS [Goddard Institute for Space Studies] and a guy [Gordon Videen] at one of the Army [Research Lab, previously Harry] Diamond Labs here [Adelphi, Maryland] who were the principals on that, and they organized these conferences. They invited me to give talks. Of course, while I was there (in Ukraine), I did go to their space center [NIP-16 tracking facility, Yevpatoria], which was in Crimea. They had the large telescopes, of course those are since gone. I visited also, the Institute for Radio Astronomy in [Kharkiv] Ukraine. I visited all these institutions and collaborated with them.

The Institute for Radio Astronomy had telescopes in Crimea. They were part of the VLBI [Very Long Baseline Interferometry] program. The principals are at Goddard. It was started years ago (by Tom Clark) and in fact the receivers on their telescopes were from Goddard. The person that worked on VLBI at Goddard at the time (Choppo Ma) was there with me on that one meeting. If he is not retired, he is still doing VLBI here.

Very Long Baseline Interferometry looks at radio signals from radio stars from different locations on the Earth, and from triangulation you can do several things. You can measure diameter of stars, or you can look at tectonic flow or continental drift, and various other geophysical phenomenon on the Earth. There is a telescope in Hawaii, on Mauna Kea, and there are telescopes around the world and there were telescopes in Crimea, in Ukraine. When I visited, it was kind of eerie, because here were Goddard property numbers on these instruments on the Ukrainian telescopes. That was another connection that we had.

JOHNSON: It's interesting, because you were talking about working, as you said, during that "dark Soviet time." Did NASA prepare you in any way? You said when they came there was a person, or a handler, that was there with them. When you went over there, before it was independent, how did NASA prepare you for that? Or was there any preparation?

KOSTIUK: That's a very good question, very good. It was 1990. I had an invitation to visit institutions in Kiev. I had never been to the Soviet Union. But unlike most Americans, I knew more about it than most because of my heritage. Actually, my dad was in a gulag [Soviet forced labor camp] for five years digging coal. My dad was a university professor, and he was purged in the '30s, as many were.

Obviously, I had met the people I described, and then other visitors to Goddard. Also, in the '60s when there was [Soviet leader Nikita S.] Krushchev, there was a "Krushchev Thaw" it was called, where they allowed intellectuals from the Soviet Union to come to the U.S. There was some measure of exchange. At that time, I had met a couple of poets as a result of my dad's profession. So I did have some contact, and I thought I knew what to expect.

So I'm getting an official visa, and I'm going. They [Travel Office] give me these information lists. It's less than a week from leaving. I get this sheet. It says what I should do, and it says that the Soviet Union regards all people born on territory that is currently part of the Soviet Union and their children as citizens of the Soviet Union. You know who I am? I'm the child of a Soviet. So I look at this and I say, "Well, are they going to—" because my dad was actually a notorious person then, in the literary circles. There were articles against him in the press in the '60s and '70s, and even '80s. But I'm going officially. NASA will protect me, the U.S. government will protect—they gave me the address of the U.S. embassy in Moscow and all that. And so I'm going. It's 1990.

V. Malyutenko (from the Institute of Semiconductor Physics) invited me, this guy that I met in Zurich, and I got an invitation from the Main Astronomical Observatory, from the Director. I'm going for the first time, and so I did. I fly into Paris, and there I take Aeroflot [Russian Airlines] from Paris to Kiev. It's a long wait, and I'm really tired. And I'm not used to flying because I didn't fly that much to Europe up to that point. Time change, I guess, is the thing.

Eventually I get to the gate, and it's not quite organized the way I expected. I showed them my ticket, and they just told me to sit down. I sat down and I saw all these people sitting there. There is this guy all dressed in black, and a big suitcase. Something that normally you'd check. We are sitting there and then eventually it was time to board. We are coming in and I go into the plane, and it really struck me. It was like a hand-painted old Greyhound [Lines, Inc.] bus interior. It looked very, very not like what we are used to. Not like United [Airlines]. I said, "Well, all right, it's a Soviet plane."

I go to my seat, and it's taken. I motioned to the stewardess there. When I show her the ticket she said, "It's okay. Sit over here." She puts me next to this guy that was all in black and his suitcase. The suitcase was in between, in the middle seat, and I'm on the aisle. "All right." So I sat down.

The plane takes off—and it was a very nice ride, by the way. The flight was very smooth, very nice. The food wasn't great. I had something to eat, and then I fell asleep. I fell asleep, and then I woke up and this guy sitting next to me turns to me, and, in Ukrainian, says to me, "How is your father, Mr. Kostiuk?" Can you imagine that? I'm at 30,000 feet in a Soviet plane, and this guy who I never saw turns to me and asks me—how does he know my name? And how does he know about my father? It's just incredible.

This is how my trip started. I have more stories like that, but it turns out that he was in France doing a video about a Ukrainian playwright and author and painter Volodymyr Vynnychenko, whose plays played all over Europe, at the turn of the century. During 1917-1918, during that period after the [World] War [I], Ukraine had declared independence, and he was head of government at that time. And so he was doing a video on this guy, and it turns out that this author—he was a playwright and author—my father was an expert on this author. My father, in fact, transferred his archives from France, where the author lived in exile—because he was under threat of assassination—and my father transferred his archives to the U.S. and stored them in Columbia University [New York City, New York]. But that's a different story.

So that's how he would know about my father, but how did he know who I was? I still don't know. You realize I still don't know for sure? Now, I did have my name on my briefcase, but I don't know if he saw that. He might have seen a picture of me. That's a possibility, because he visited the place where this person lived and died, and I had been there two years earlier, or something like that. Maybe he saw a picture, I don't know. I really don't know.

We kind of chatted, but I was nervous. I didn't know what's going on. It's one thing after another. Then we land, and I did recover my bag. We are standing in line, and this guy is really excited. He says, "Look what I'm carrying," and he has got this suitcase with him. He opens up this suitcase and starts flashing books, showing me books that were forbidden in the Soviet Union. This is 1990, so this is now perestroika [period of Soviet reform]. This is an example of all the changes that are occurring in the Soviet Union.

These books were still forbidden, and my father published some of them—and some were my father's books. He is flashing this, and I see this as a provocation because they could use this as a reason to detain me for some reason, that I'm a propagandist or whatever. I try to avoid this guy, and he is flashing this book about Stalin that this author [Volodymyr K.] Vynnychenko wrote that my father edited and published. He is waving this in front of me, he is so happy to have this stuff.

Well, it turned out okay. We went through security. They bothered me because I didn't give them the exact dollar amount I was carrying and all that stuff, but anyway, I got through and was met. The person that picked me up, the same person I met in Switzerland, had to give this guy a ride because he didn't have a ride to the city. I had multiple experiences like that for the next week or so that I was there in Kiev.

The caveat that's relevant to NASA is that when I got back, I got a call from our international office, "Hey Ted, there is guy here from the CIA [Central Intelligence Agency]. He'd like to talk to you. Well, you don't have to talk to him," she says, "but he would like to."

I said, "Fine, I'll talk to him." So I had a meeting later with this CIA guy who was overt. He said, "I'm an overt [agent]. I'm not a covert, I'm an overt." He asked me about the meeting, and whom I met there, and who the people were and all that stuff. He had some specific questions. Then he offered to pay for my next trip to any international meeting where Soviets were. I, of course, declined.

JOHNSON: Was that with the intention that you would provide information?

KOSTIUK: Oh, of course. Of course. When I went another time a few years later, a couple of years later, I think I had a phone call—they also asked just general questions. They had specific questions—whether specific people were there. I said I didn't meet them, I don't know who they are. I think the third time I was busy, and then they never called back. It sort of decayed with the Cold War.

JOHNSON: That's quite an experience, and I don't blame you. I think I would have been a little nervous on the plane.

KOSTIUK: Oh, yes. There's nowhere to run.

JOHNSON: Yes, exactly. Unless you had a parachute, that was it.

KOSTIUK: Yes.

JOHNSON: You mentioned the work you did at Headquarters, and that was between '93 and '96. What exactly were you doing in that position? It says "Manager, Planetary Instrument Definition and Development Program [PIDDP]." Talk about that for a minute, and what you were doing.

KOSTIUK: Well, the PIDDP was one of the NASA R&A programs that provided funding for new instrument ideas for future missions. Basically, it supported a lot of important things. For example, one of the things that we supported were the sensors that later flew on the Kepler mission for extrasolar planetary detection.

I remember visiting [NASA] Ames [Research Center, Moffett Field, California] where a lot of this work was done. We funded that effort of CCD [charged-couple device] development, and components for the CCD detectors or arrays that were used to look at occultation of stars by planets in the Kepler spacecraft.

A lot of the instruments that came out of this program—supported and were built—were augmented for various missions. Near-Earth Asteroid [Scout], there was a mission like that. Yes, that was another thing that was supported by this program. Some of the Cassini effort—I think that later on even some of the effort that went into the [Mars Science Laboratory] Curiosity spacecraft, that was also supported by the PIDDP program. PIDDP since has been modified and broken down into two different programs, PICASSO [Planetary Instrument Concepts for the Advancement of Solar System Observations] and MatISSE [Maturation of Instruments for Solar System Exploration].

One is more like the old PIDDP, which is not that much money for development, and one that develops the instrument to the next step. But at that time it was only one level of development. That was a program that I basically ran as a manager. I put review panels together and made decisions regarding funding, and also did other things at Headquarters that related to instrument development.

JOHNSON: It looks like you were also the Chief Scientist for Exploration Programs under the Space Sciences Directorate at Goddard?

KOSTIUK: Before that and after that I was Chief Scientist for Exploration. This basically, remember, started with the father [George H.W.] Bush presidency, the Space Exploration Initiative. I was the representative from Goddard to various meetings and planning, activities. One of the activities that came over that period was attending meetings that were on proposed SEI missions. I have some interesting stories about that, too, but I don't know how appropriate they are.

About the same time, we had a conference that I helped organize with Mike Mumma and others on exploration from the Moon, astronomy from the Moon [Workshop on Astrophysics form the Moon, Annapolis, MD, February 5-7, 1990]. The Moon was then deemed one of the platforms we could use for telescopes, For example, for radio telescopes. Its radio quiet environment would make radio astronomy very powerful. It provided no atmosphere, so you could look in wavelength regions that are not accessible from the Earth and so on.

Along with that were also discussions of searches for extrasolar planets, which, I must say, really accelerated after the mid-'90s, 2000s. That was really amazing because I remember I was, during my NASA PIDDP tenure, at a meeting—I remember in Boulder—where [William

J.] Borucki and his proposed [Kepler] mission was really frowned upon, if not ridiculed, at some point. It turns out that he was successful, and it became a very, very productive mission.

That was one of the meetings where the, quote, "experts" at the time were looking at various ways of detecting extrasolar planets. Which was an interesting meeting in itself, scientifically, but it also demonstrated the kind of community that one was working in at the time. And I was there as a Headquarters' representative—I think at that time I might have been there also as the Goddard representative, or just Headquarters. Of course, I couldn't do both at the same time. My roles overlapped over that period.

JOHNSON: Also looking through your CV and your resume, I notice there is a lot of work that you have done—a lot of workshops, a lot of advisory committees—related to education, and educating the public. One of them I am looking at is the member of the Challenger Center for Space Science Education Advisory Committee [or Education and Public Outreach]. Talk about the importance of being associated and working with these different groups that are promoting education and science education, in this country and other countries as well.

KOSTIUK: Yes. I think it's extremely important. It's particularly important in the U.S. because we all—and this is one of the things you learn, especially at Headquarters—that we really are servants of the public. They pay our salary, and so we have to not only think about science in general, but also respond to what the needs are of society.

One of the big influences that I had on me was a former student and a current colleague, both of whom are very deeply involved in science education. The student was the one that did the velocity measurements, the wind measurements, on Venus as his dissertation, Jeff Goldstein.

As I mentioned, he went to the Smithsonian, then from there, to the Challenger Center. Then he left the Challenger Center and he formed, ultimately, his own education organization, National Center for Earth and Space Science Education [NCESSE]. Now, it's not a big organization, but what they do is they go to various communities, disadvantaged communities in particular—one example is Indian reservations—to schools there, and they would provide talks and examples and education about Earth and space science at a K [Kindergarten] through 12 [12th-grade] level. Also, they try to enhance the knowledge by exhibits.

One of the major exhibits that I think my CV addresses is the scale model of the solar system, Voyage: Journey Through Our Solar System on the Mall. I helped to design this exhibit [as a member of the Smithsonian Institution National Air and Space Museum Advisory Committee - 1998-2001].

If you go to the Smithsonian, outside on the Mall sidewalk there is a Sun about the size of a grapefruit at about the glass part of the Air and Space Museum, and I think Pluto is somewhere where the Castle [Smithsonian Institution Building] is. You can walk that distance—which isn't that short—and at the proper relative distances you have a little exhibit which shows the planets, Earth being the third planet. In fact, Earth is so small you can only see it by tactile. You kind of feel it.

The exhibit consists of a solid block, like a Plexiglas-type material with the planetary system—the planet and its moons—embedded in it so you can see the reflections. Then a little tactile display where you can feel how big it is. It's all to scale. I guess the Sun is a grapefruit size when it starts, and you have these little planets at the stations.

Jeff conceived the exhibit, and with Challenger Center support—and the Smithsonian supported this, it certainly gave you the ground it was built on. They NCESSE have actually put up several of these exhibits throughout the country. I think maybe one in Galveston, Texas, and some other locations. I was one of the advisors on this exhibit, and so I feel it's very important. Jeff and NCESSE are now involved in a program/competition that enables high school students to fly designed experiments on the Space Station.

One of my colleagues here [GSFC], Tim [Timothy A.] Livengood, continuously gives talks to the community, particularly schoolchildren, on science and technology and so on. He is very, very active. So through them, I really feel the need that we need to do that. I've done some of that myself personally, but not as intensely. I have been very busy with other things. But I do support all of that, and I think it's very important.

It's kind of a shame. I think a lot of the education budget within NASA has been cut in the last proposed budget, I believe, or at least there's a fear of that.

JOHNSON: Yes. There is definitely a fear.

KOSTIUK: That's a shame, because again, these are the people that pay our salaries, and we have to respond to their needs.

One of the things I learned at Headquarters, which is another thing—we used to get letters from people. Really off the wall, some of them. "Why is NASA hiding aliens?" You know what? I would have trashed them. But that's not what happened. They had a person there who wrote back to every letter, at least in the Planetary Sciences Division, answering their questions and concerns. I thought that was very, very, very good, very appropriate.

JOHNSON: We've talked to Dr. [David] Morrison at Ames, and I know he took that upon himself to do a lot of that, especially around the time of 2012 when people were afraid the world was going to end. But I think it is important, too, that especially the scientists take some responsibility in educating the public, and we do need that next generation also to continue the work.

KOSTIUK: Absolutely. If scientists respond at least at some level, at least it's more consistent with scientific knowledge, as opposed to speculation.

JOHNSON: One of the questions I have been asking some of your fellow scientists this summer, just because of the atmosphere in the U.S. right now—the political atmosphere, but also almost a feeling of people being more skeptical, I guess, about science than it seems like they have been in the past. How do you feel that that's going to affect NASA, or do you think it does? Do you think NASA needs to address that directly more than they do now, or do you feel like it's something that they are dealing with as well as they can? Especially, Earth Science and different types of science that you have been involved in?

KOSTIUK: You are always going to have times when science becomes less important, or less supported, or less respected. When everything is comfortable, you don't look at how you can make things better, or how you can solve a problem if there are no problems. That's part of the problem. Like with the global warming, people don't feel an immediate effect. Although I have felt it for years, because we go skiing every winter to this place in New York State, and we used to freeze. Now I sit in the sun and have a beer.

I don't know if that's just a snapshot of the situation, but as long as people don't feel the effect, they are not going to be as far-reaching into the future, and that's unfortunate sometimes. Our comfort is one of the both good and bad aspects of our lives. As I said, science education is an important part of it, and so our perception of things should be also voiced to the community.

JOHNSON: I think the outreach that you have done or you have taken part in has a good effect on educating people, and hopefully making them more aware of the importance of science. As you mentioned earlier, some of the first things you worked on is one of the reasons we have MRIs, and how the technology transfers.

KOSTIUK: That's right. You have to show that example. At that time, the person that I mentioned, he kept talking about investigating structured tissues with this technique. Even I said, "Wow, that's a good idea." But it didn't even register that it's going to become a real powerful tool in the future.

I always think of the science fiction stories that we had when I was young. If you pick up some old science fiction stories, if you listen to old-time radio where they have science—a lot of that stuff seems like nothing. We have that and more today.

JOHNSON: You mentioned the HIPWAC, the Heterodyne Instrument for Planetary Wind and Composition Project. You said that that's what you are working on currently, or it's what's being used currently.

KOSTIUK: That's our most current instrument for ground-based observing. [We essentially developed and used the technique for spectral investigation of planetary atmospheres (and some stars). HIPWA's orders of magnitude higher spectral resolution mid-infrared spectrometer enabled the many discoveries and measurements possible. Among them—natural lasing on Mars and Venus, direct wind measurements on Titan, Venus, and Mars, ozone and carbon dioxide isotopes on Mars' atmosphere, thermal infrared aurora on Jupiter, composition and thermal structure on giant planets, and effects of cometary, bolide, impacts on Jupiter. We keep upgrading HIPWAC and using it. [We have incorporated widely tunable solid state quantum cascade lasers in addition to CO_2 gas lasers into HIPWAC, which with improved electronics allows us to make measurements over a much wider spectral range and study a greater variety of molecular sources and chemical processes.] We used it last year, for example, to observe Jupiter, prior to the Juno mission orbital insertion, its thermal infrared aurora and atmosphere. It's still usable. Technically, it's been for most of the years a PI, a principal investigator, instrument at the IRTF, so a lot of our equipment is at the observatory.

We only bring the instrument back here to fix it, or to upgrade it, or do whatever. It's here now. We plan to ask for observing time next winter/spring, when Jupiter becomes accessible at night. The key person on that is my colleague Tim Livengood now. I will support it if I can, if I physically can. But I'm trying to retire.

It's still active, but we are doing some other things that might be interesting. In fact, there was a news release or something last year on carbon nanotube composite telescopes that we are developing under internal funding here at Goddard [e.g., http://www.eurekalert.org/pub_releases/2016-07/nsfc-nef071216.php]. That was one of our efforts, and that made a bunch of news. I don't know if I put that in my CV somewhere.

JOHNSON: Yes, I don't remember seeing that one.

KOSTIUK: If you Google "carbon nanotube telescopes for CubeSats"—CubeSats [miniaturized U-class spacecraft] are a new "fad". It may go away, but it's a fad now. My engineer actually walked in a little while ago, probably to talk about that.

Anyway, this work was highlighted in the news, which even had pictures of us. It came out of a Goddard monthly technology report, and then a lot of the news people picked it up. If you want to learn more about that, you could do that.

JOHNSON: Have you officially retired yet, or are you still working?

KOSTIUK: No. I officially retired March 3 [2017]. But I'm an emeritus now. I procrastinated long enough, so now I'm moved into a small desk and office, and I have my 50 boxes of stuff to unpack and filter out. I still date back to a time when paper was important. By the way, in going through my papers, I did find a lot of things that I just totally forgot we did.

JOHNSON: You have had a long career, so I would imagine a lot of that was long enough ago that other things have taken priority in your brain. At least that's the way it works with me.

I just have a couple more questions. I thought we would talk about some of the general questions that we tend to ask of everybody. You have worked for a long time for NASA. Of course, we have touched on the technology and some of the changes, but the technology has changed so much. It's hard to keep up with technology now.

You mentioned that some of the people doing research in Hawaii, they can sit at a desktop just like you would if you were actually there, and follow the information coming down. So there are a lot of things that have changed. Talk for a couple of minutes about some of the technology, and how that has affected your work and what you do.

KOSTIUK: [In the early 1980s I actually did a study predicting or forecasting changes in sensor technology over a decade away. The work was chartered by the NASA HQ Office of Aeronautics and Space Technology (OAST). With input from a large sensor community, the study covered nearly the entire spectral region, gamma ray through radar frequencies and including particle and magnetic field detectors. The work ["Spaceborne Sensors (1983-2000 AD): A Forecast of Technology", T. Kostiuk and B. Clark, NASA Technical Memorandum 86083, NASA/Goddard Space Flight Center, Greenbelt, MD] was published as a GSFC memorandum and as a chapter in the 1984 OAST Space Systems Technology Model [Vol. IIB Space Technology Trends and Forecasts, Payloads Technology]. In many cases the results in the forecast greatly underestimated the actual advances by year 2000 and even more so by today.] Well, clearly today's network capability is important. As an example, we can solve problems. We can be on the Subaru Telescope and our computer fails. We have a backup computer, but not the software, and we can get a software backup from Cologne. I mean, that in fact happened some years ago, but that's new tech. We couldn't have done that in the '80s; we couldn't have done that in the '60s. We'd be dead in the water. Of course, then the complexity of the software wouldn't be as high either, so there is a tradeoff.

Certainly our analysis ability has improved tremendously. We have more sophisticated ways of looking at data and analyzing it. In fact, in ways that I can't anymore. There was a time when I could analyze my data. I can't do that anymore. I need somebody like my colleague Tilak Hewagama, who is much more proficient in that ability to use these sophisticated programs, and to develop these programs to actually do at least the initial analysis.

So this is good and bad. The bad part is that unless you have these people available, you can't work very fast. I think in general, what's happened is that it's hard to be a master of all trades. Much of the effort has become more of a compartmentalized teamwork, and this is the way it's going to be. It's going to be very hard to do everything. Like when I was in graduate school, I did everything. Now I don't think I can. So that's one big change in technology.

The remoteness of communication—in fact, if you want to observe from ALMA [Atacama Large Millimeter/submillimeter Array] in Chile, you go to an ALMA office, I don't know, in Cologne or Paris or whatever. You just sit there, and it's like you are at the telescope. You give the commands, the information that's needed, and the telescopes do the rest. Or the software that drives the telescope does the rest.

The big telescopes—even in Hawaii, like Subaru—they don't like you to be there. Even when you go to Hawaii, you don't go to the summit. You sit in an office with a computer and a monitor in the cafeteria building. And that's true for Keck, Gemini, or Subaru. People don't even go to the summit and experience the joy of not breathing. We miss that.

In fact, we got penalized for being at Subaru too long because the Japanese are very precise. One of the telescope operators told the Director that we have been there over 16 hours setting up our instrument. Of course, for us that's not a big deal, we do this all the time. I had to apologize to the Director as we were leaving. He wanted an official apology. After we had our tea, we are leaving and he says, "Ted, you must do something."

I said, "What?"

He says, "Well, you violated our rules."

I said, "What, what? How did we?"

"You stayed at the telescope longer than 12 hours. So you have to write me an excuse."

And so I did. The remote observing eliminates this, but of course, if you bring your own instrumentation there, it should be a different set of rules. There is a reason for these rules, though. There are health reasons and so on, and you have to not ignore them completely.

Yes, modern technology has changed. Even the fact that we can communicate by cell phone, or reach individuals quickly if we have to.

JOHNSON: Well, looking back over your career with NASA, what would you say has been your biggest challenge?

KOSTIUK: I think the biggest challenge was the transition to provide support for all your efforts, and that occurred mainly after about 2005 or so. I guess the other way to put it, is to deal with the bureaucracy and the sort of formalism of the effort.

You see, when I came out of graduate school and I came here, and when I needed some help in something, whether it's optics or machining, I just would go to the shop and get help. That's why the people were there. They were there, and they wanted to help you. Every person that I spoke to, their job was to help me. It's not that way anymore, for the most part. I mean, they still want to help you, but they want something for it – a charge number. It's not as much of a team as it was in the pioneering days. So the challenge is getting used to the newer system, and funding profile.

Funding, I think, is the biggest problem. If we have to spend from 30 to 40 to sometimes 50 percent of our time getting funding, then we are only doing half the work—even if we get the funding. If you want to encourage research, you have to make it a little smoother. I think that's been the challenge in the last few years. I never had a problem having enough resources to do our work and to support all of my colleagues, but in recent years it's become a lot more difficult.

Well, maybe it's me because I'm older and, as you pointed out, the system has changed and I'm not geared to this. By the way, younger scientists coming in, they know no better. So to them, this is the way things should be, "This is the way things are." Whereas for people like me, it seems we now tend to spend time on things that aren't important.

JOHNSON: Well, the opposite of that—what would you consider to be your most important accomplishment?

KOSTIUK: My most important accomplishment? Well, I think we've had a good scientific return for all the effort we put in. I mentioned some of them. We developed a unique technique that may or may not continue to be used, at least in this form.

Personally, I think the group that I worked with throughout the years—I think we had some great relationships. We really were a team, and you'll see it when you look at those videos on YouTube that I mentioned. That was the greatest, I think, return. It was the people that we worked with and the way that we worked with these people, the way the environment was. I don't think we've had any conflicts at all anyway, and we had good return for our work.

I guess one of our biggest challenges is to finish all the publications that we didn't do while we were actually not retired. Because we have so much data and there just was never any time to finish it. We always had to worry about funding or covering our time, especially colleagues that were dependent on soft money. That's one of the situations that sort of limited what we could do. I would like to catch up on that. I hope that still will happen.

JOHNSON: Hopefully it will. Is there anything that we haven't talked about that you want to mention? Or any project or program, or any other anecdotes that you'd like to add?

KOSTIUK: Anecdotes are many. I don't know.

JOHNSON: Well, we enjoy those, because they are the personal side of NASA history. It personalizes NASA for people when they read it, too. We're all human, and we have these human experiences.

KOSTIUK: There are stories I can keep telling. You already heard some of them. Like my trips to Ukraine, or to Europe, or to Germany. You mentioned the SOHO [Solar and Heliospheric Observatory] mission. Drake Deming and I wrote a proposal for an instrument, Solar Infrared Photometer (SIRP), to measure variability of solar infrared flux on that mission. In connection to that proposal we visited ESA in the Netherlands and made trips to visit colleague and co-investigators in German and the Swiss institutions. For dinner in Zurich, my colleague, he just wanted to go to McDonald's [fast-food restaurant chain]. Just a typical American, he wanted a McDonald's, and I wanted to go to a nice Swiss restaurant. We eventually made a bargain. "We'll walk through a McDonald's, then we'll go to a fondue place." Then we go to the fondue place, and the waiter doesn't understand a word. We can't understand him, and he can't understand us. We tried a couple of languages that we knew.

JOHNSON: Nothing worked?

KOSTIUK: Nothing worked, because he didn't speak French, or German, or English. What was he doing as a waiter in Switzerland? But those are all personal things.

On my first trip to Kiev—and again, I said I had multiple adventures in Ukraine, in the Soviet Union. During the Soviet Union, I wasn't allowed to leave the city. You were confined to the borders of the city according to the regulations. But to get into the observatory, in some ways we had to leave and come back into the city. They kept threatening me that we are leaving the city every time we drove in. We had to leave the city border and then come back in, into the observatory. Little things like that, yes. The Soviet times were interesting.

JOHNSON: I can definitely imagine they would be.

I appreciate you talking to us today, and for staying a little longer than we had planned to try to get this done.

KOSTIUK: Yes, that's fine. As I say, I have nothing else. I planned to probably do some work or something, but I'm retired so I cannot do that today. It was a pleasure to talk to you. You had a lot of patience, I think, as I was rambling along. But you told me to ramble.

JOHNSON: Well, I did. I did tell you that it's okay to ramble, we like that.

KOSTIUK: I did try to ramble as much as I could.

JOHNSON: Yes. Well, we appreciate it.

[End of interview]