

**NASA SCIENCE MISSION DIRECTORATE  
ORAL HISTORY PROJECT  
EDITED ORAL HISTORY TRANSCRIPT**

WENDELL W. MENDELL  
INTERVIEWED BY SANDRA JOHNSON  
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JOHNSON: Today is September 20, 2017. This interview with Dr. Wendell Mendell is being conducted for the NASA Headquarters Science Mission Directorate Oral History Project in Houston, Texas. The interviewer is Sandra Johnson, assisted by Jennifer Ross-Nazzal. I want to thank you again for joining us today.

MENDELL: Do I get a prize?

JOHNSON: Yes, you get your transcript when all this is over with. You have work to do when this is over with. If you want to write a book here's a good start.

MENDELL: Just checking, lollipop or something.

JOHNSON: You get a cup of coffee.

MENDELL: I've already got a cup of coffee.

JOHNSON: Let's start by talking about your background and your education, and what brought you to work for NASA.

MENDELL: Well, I was born and raised in Houston over in Southside Place, if any of you know where that is. It's surrounded by West University [Place] and Bellaire. Little tiny city, nine blocks by one block. I went to Lamar High School and was one of the quote "smart kids," math and science.

I was basically groomed to go to Rice University [Houston, Texas] because in those days Rice was tuition-free, and my dad had gone to Rice University and my uncle had gone to Rice University and my first cousin had gone to Rice University. A lot of my friends at Lamar were going to Rice University. So that was that.

Then, as a junior in high school, there were the smart kids in the senior class I looked up to. One of them was named George [A.] Fisk. George had applied to this place called Caltech [California Institute of Technology, Pasadena, California], which I'd never heard of. He didn't make it in, and the reason supposedly was that his SAT [Scholastic Aptitude Test for college admissions] scores weren't [high enough]. My SAT scores were [high enough]. I decided to apply to Caltech so that if I could get in then I could say, "Well George, you know."

So I applied. It turned out there were two from our high school who applied. The other one was a guy I didn't know. They sent an alumnus to interview us. I found out later that my high school counselor had told the alumnus, the interviewer, that I was the type of personality that would crack under pressure, and that it probably was not best for me to go to a place like Caltech.

After our interview I walked out of the school with him and said, "What do you think my chances are?"

He said, “Well, your grades aren’t outstanding; you’re not the valedictorian.” I graduated just under the [top] five percent. “You don’t have any money, and Caltech is very expensive, so you would need a scholarship. I would say”—he was being kind—“you have a 50/50 [chance] to be accepted.”

The previous summer I had been selected as 1 of 25 high school students in Texas to attend a two-week mathematics workshop at Rice University. We lived on campus, and there were two professors who taught us. There were kids from across Texas, and we had a great time. One of the professors had gone to Caltech, and we’d remained friends. I said to the interviewer, “Well, if I get a professor to write a recommendation would that help?”

“Oh yes, it would. But he has to do it right now because I’m airmailing this and they’re going to make decisions this weekend.”

So I stopped by Professor [Lincoln K.] Durst’s house that night, told him the story, and asked, “Can you do it?”

He wrote a letter to the Dean of Freshmen who was a friend of his. I saw the letter a couple of years later, and it intimated that I had done original research in linear systems of equations as a high school student, which was a bit of a stretch. The next thing I know is that I get a letter of acceptance from Caltech, which amazes me. And then I get a tuition scholarship, which was even more interesting, and then I got a letter saying that the admissions committee has selected me to be what they call “honors at entrance.” That is, I am predicted to be in the top 10 percent of the class at graduation.

All this stuff comes in and we knew nothing about Caltech. I’d never been away from home, and we didn’t have any money. Even with that kind of financial help it was going to be [difficult], but I decided to go.

I discovered a guy in our neighborhood who was attending Caltech, entering his sophomore year. We drove out to California in his '52 Chevy [Chevrolet], which broke down not far from El Paso [Texas], and we had to ride a bus the rest of the way to Los Angeles [California].

At Caltech, everyone in my class graduated at the top of their high schools and/or had won some national science competition. I quickly realized that I was a small fish in this pond, and experienced certain trepidation. Nevertheless, I did graduate and I had gotten involved in a lot of the nonacademic activity on campus, held offices in my student house, served as head cheerleader, etc. I if you went to Caltech and never made a decision, you ended up with a physics degree. That was me.

My grades were less than sterling. I had managed to keep my scholarship by the skin of my teeth, but my grade point average was mediocre. While all my friends were going to graduate school and were destined to be professors or researchers or famous people, it was clear I was going to have to look for a job.

IBM [Corporation] would call every Caltech graduate and offer him a job, and then they would have somebody from last year's class call and tell you what a great place IBM was to work for. A recruiter took me out to dinner and showed me this wonderful new computer called a 1620 which could read punched cards. It looked like a big humidifier.

But my mother wrote me tearful letters saying that she wanted me to leave that evil California and come home to Texas, because there was this new space thing that was going to be built in Houston and my aunt Ailene knew a woman in what we call now Human Resources—in those days we called it Personnel—and that she could probably get me a job. She wanted me to apply so I would come home and be with good people.

So I did that. NASA took forever to accept my application. IBM was pressuring me for an answer. I was hired to be an aerospace technologist. I had no idea what that was. I don't think NASA did either. I showed up and reported to the Personnel Office where a guy looked at my letter. He opened the bottom drawer of his desk, lower left-hand drawer, and he pulls out a sheet of paper. He said, "Now do any of these sound familiar to you? Radio, electricity."

I said, "Yes."

He said, "Okay." He puts it back. He says, "Okay, I want you to go interview with this group." It turned out to be a group that did orbital dynamics, trajectories, optimizations. They were all mathematicians. One of them was actually the older brother of a guy I'd been in junior high school with. They were nice people. It did not sound exciting.

Back in Personnel the guy says, "Well, because you're a physicist, I have to send you to this other place to be interviewed." It was the Lunar Surface Technology Branch. I decided that working to study the Moon would be better than doing optimizations on trajectories. I became a member of the Lunar Surface Technology Branch, which had offices at Ellington [Field, Houston, Texas (Ellington Airport)]. The Center was not built when I came. It was June of '63 and there was nothing on the ground here. People were in offices all over this part of the city, and we were at Ellington. I was assigned to learn about lunar temperatures, the temperature of the surface of the Moon.

There was a guy named Tom [Thomas E.] Margrave who had been doing a little bit of that. He had graduated from [University of] Notre Dame [Notre Dame, Indiana]. He gave me a little simple computer program, which ran on an IBM computer, written in FORTRAN [Formula Translation programming language] and said, "This will calculate temperatures." I said, "Okay." And that was it. I was to learn about the temperature of the Moon.

Whenever engineers begin a project to build something, they need an environment document that gives them the ambient conditions for the design. The Space Environment Division was created to write that document for Apollo. The Lunar Surface Technology Branch had the task to determine values for various aspects of the lunar environment. One of the items was the temperature, so that was my job.

My officemate was a really nice person, very handsome, from Huntington, West Virginia. He had walked through the snow to go to college at Marshall, salt-of-the-earth kind of guy. He was working with cameras and was interested in the unusual way the Moon reflected light. It's called the lunar photometric function. By sitting in the office with him I learned about the photometric function.

I was busy trying to learn about the Moon's temperatures, which were sensed by infrared detectors at telescopes. In those days there weren't scientists who studied the Moon. What little literature that existed was published by astronomers who had instruments for measuring stars that they would point at the Moon and take a measurement. It wasn't great data, but it would give you basic information.

I began to realize that you could also sense the temperature of the Moon with microwave antennas because the Moon emits not only in the infrared but through the surface in radio waves. I began to read about that. Soviet radio astronomers had published an extensive literature on lunar measurements. I had an engineer call me up one day and say, "In your environment document you've written that the dielectric constant of the Moon is 'blah blah blah' and I have a paper here that says it's not, it's different."

He had a Russian paper that had been translated into English, so I read it. I puzzled over the provenance of the published value. I tried to trace back the references and eventually came

to a paper in Russian written in 1954. I couldn't do anything with that except send it to Headquarters of NASA [Washington, DC] for translation, so I took a course in Russian at the University of Houston. After a semester I could pick my way through that paper because it was not grammatically complex. The technical terms were like "radio teleskop," so you could figure it out.

I found that the Soviet system was set up so that there's always somebody who's in charge of everything, and nobody dares challenge him. The guy who wrote this '54 paper was the head honcho and he'd made a mistake, so everybody just kept making the same mistake. It was fun to discover that.

JOHNSON: You were able to convince him that you were right?

MENDELL: The engineer didn't care. I just said, "My number is okay." Explaining the problem would be like blowing in the wind, but it was fun for me.

At this point in my career, having worked at MSC [Manned Spacecraft Center, now Johnson Space Center] for a little over a year, I was feeling unsettled. I had attended one of the most intellectually demanding universities in the United States. My entering class was 150, and the undergraduate student body was less than 1,000. It was very select and very intense. NASA in Houston had been rapidly expanding the engineering staff, hiring people with Bachelor's degrees, mostly from regional universities. The ambience was very different.

I discovered that NASA had a program to send qualified employees for a year of graduate study, with full salary and paid for by the Agency. I applied and was able to attend UCLA [University of California, Los Angeles] to obtain a Master's Degree in physics. My Caltech

GPA [grade point average] did me no favors, and UCLA admitted me on probation. My faculty advisor was somewhat skeptical of my plan to get a three-semester degree in two semesters, given my probationary status. I did it in two semesters, taking overloads, and had a great time, and met a woman that I married. When I returned to NASA, the people I worked with were surprised to see me. They thought I was just going to stay, they didn't believe I was actually coming back. Of course, I had signed an agreement that I would work for at least 3 more years.

When I came back, there were many buildings onsite. A new Building 31 had been built, and that's where I spent the rest of my career at NASA starting in about '66. I was still studying lunar temperatures. More generally, I was working to determine and understand what are called the thermophysical properties of the lunar surface.

Now more scientists were spending time actually looking at the Moon as an object of research as opposed to just something they did because they had a telescope. The key measurement that would allow us say something concrete about the nature of the lunar surface and its bearing strength and all the things that engineers want to know was the temperature of the surface at lunar midnight or before lunar dawn.

If you're on the Earth, the only time you can measure that temperature is at new moon. Of course, at new moon, the Moon is in the sky during the day. If you've got an infrared telescope and the Sun is in the sky, and the measurement is just near impossible. Various researchers, including one at Caltech, had tried to measure infrared emission at the lunar terminator. Their equipment could detect the lunar limb opposite the illuminated crescent; but as they moved across the disk, the surface temperatures became so low as to be undetectable.

By the way, that researcher's name at Caltech was Bruce [C.] Murray, who later became Director of JPL [Jet Propulsion Laboratory, Pasadena, California].



I went out to Bruce Murray's laboratory and met his graduate students, all of whom are now famous scientists at JPL and around the world. Coincidentally, a Caltech classmate of mine worked in Building 31 at MSC.

He was doing graduate work at Rice and told me there was a professor there who was looking for graduate students. It was well-known scientist who had invented a new infrared detector, 10,000 times more sensitive than current technology. I went out there and prostrated myself and said, "I can be your graduate student." He agreed.

That began a relationship with Rice where I went as a graduate student part time. I would go out two days a week to take classes and be at work three days a week. There were some issues. I had supervisor—temporary supervisor fortunately—who didn't want me to go out to Rice because that meant he couldn't see what I was doing. He knew nothing about my field, and, opined, "It couldn't be as important as what we do here in MSC." I got past all that and began work on my doctorate.

After Apollo 14, the missions were known in NASA as the J-series and had advanced capabilities for lunar exploration. Most people remember the surface rover driven by astronauts, but there were also new remote sensing instruments in orbit as part of the Command and Service Module.

When engineers began planning for the orbital science package, they were looking for measurements of scientific interest, but also instruments essentially ready to go. In other words, instruments requiring technology development were not eligible because the Apollo missions were launching every few months.

At Ellington, there was a room with a blackboard that looked like chalk spreadsheet. Each row contained basic information about a candidate instrument, including a contact phone

number, usually a scientist. One of the instruments was an infrared radiometer, and I made sure that I was the contact for it.

My thesis professor from Rice was at a cocktail party when the new head of science at MSC—and that's a whole story in itself—asked him if he would like to be the principal investigator on an infrared instrument that would be put into lunar orbit on an Apollo mission. He was a galactic astronomer and had no interest in Apollo, but he figured that my finger was in the pie somewhere. So he said yes, and I became the coinvestigator on the Apollo 17 Infrared Scanning Radiometer.

Our team only had two people on it, he and I. I was the one who went to all the meetings and did all the project-related work. He would show up whenever there needed to be a signature. However, he did make some critical technical input to the design and calibration of the instrument that was really important. But basically I ran everything.

As the launch of Apollo 17 approached, we had to start practicing our roles in Mission Control because our team is going to sit at a console up on the third floor in a little room. The orbital experiment teams are going to be there, and our console has to be manned 24 hours a day. That means 12 for him and 12 for me. He was upset about that because he had better things to do. I said, "Look, you've got a contract, you've got to do this." Mission flew, instrument is fine.

I was up there at night from 8:00 p.m. to 8:00 a.m. He was there during the day. The astronauts worked and slept on Houston time. When it's night in Houston, they're sleeping. They turned on most of the instruments and just let them go. On my night shift I'm seeing continuous samples of data when I'm up there. When he's up there, the instrument gets turned on and off because of various activities.

There was an extremely expensive experiment called the lunar radar sounder [Lunar Sounder Experiment] whose cost was 10 times our experiment. There were two large teams of scientists, one from JPL and one from the University of Utah [Salt Lake City] with a total of 20 people. The reason was that two teams had turned in the proposal and NASA was unable to decide which one to select, so they made up a hybrid of the two and put both teams on it and then picked another guy to be the Principal Investigator in charge. Typical political decision.

This thing was so powerful and generated so much electrical interference that when it was going to fire up, all the other experiments on the spacecraft got turned off. My professor begins to notice that when those guys are all doing things over there, his goes dead. He decides to investigate.

When I show up at 8:00 p.m., I notice that my console is not being manned. There are two guys at the front of the room, Richard Baldwin from the Space Center here, and Floyd Roberson from NASA Headquarters. They motioned for me to come over. I went down to the front and they said, “Wendell, is there any way you could be on this 24 hours a day?” I said, “No, I don’t think so.”

My adviser had gone down to the Lunar Sounder console and asked, “What is it you’re doing here? What is this?”

They were all excited to tell him.

He listened for a while then says, “That thing is never going to work, and if you actually get any data back no reputable journal is going to publish it.” Smoke started coming out of their ears. He was absolutely right, it turns out. But it was an unpolitic thing to say, so that’s why they wanted me to be on console around the clock.

We got through that the mission, and then I began to work on the results. I had a contract out at Rice, and meanwhile I'd finished my coursework. The infrared experiment became my thesis. I'm working on my thesis and working at NASA, so everything is just meshing beautifully.

Let's switch gears and talk about the genesis of the planetary research group at JSC. When President John F. Kennedy declared a goal to land a man on the Moon, NASA engineers regarded that phrase to be a performance requirement. The simplest way to satisfy the requirement would be to have the astronaut step onto the lunar surface, perform a small ceremony, and climb back into the lander to return. The scientific community lobbied Congress to additionally require return of samples of the lunar surface material. The engineers had to negotiate with scientists to determine what returning a sample really meant.

Meanwhile, Dr. Ebert King, a geologist on our group, argued that a special room be built in Building 31 to house these valuable samples. As his suggestion moved up the management chain, the room became a new wing. The national biological community demanded that NASA quarantine the samples and the astronauts to guard against some unknown pathogen being returned from the Moon. All these demands led to the construction of the expensive Lunar Receiving Laboratory [LRL].

The geoscience community decided that the lunar samples should be processed and examined under a vacuum similar to that on the Moon. Glove chambers capable of maintaining a high vacuum existed at Oak Ridge National Laboratory [Tennessee]. A line of vacuum chambers were acquired from Oak Ridge, and a scientist familiar with operations in the chambers, Dr. P.R. Bell, was hired to be Division Chief of our group and to oversee the construction of the LRL.

The lunar investigators were funded by the Apollo program, using the Procurement Division at MSC. They issued contracts, which are totally inappropriate vehicles for scientists because the contract requires deliverables from the funded party, and the contracting officer appoints a technical monitor who is tasked to monitor the technical progress of the work. The only deliverable from basic research is knowledge, and progress in research is not a linear process. I was technical monitor for three scientists during the first year or so of the contracts.

P.R. Bell was a really smart guy, but his management skills were not commensurate with the complex task he was assigned. Bell was replaced by a world-class geochemist, Paul [W.] Gast. After Gast arrived, he immediately held an all-hands meeting over in Building 37 because the conference room there was bigger than ours. He told the staff that this was going to be a PhD-level research organization, and everybody here would have to be doing peer-reviewed research. If you were just a contract monitor, you should find another job.

Most of the guys held Bachelor's Degrees, and they monitored contracts. There was an exodus, and many went over to the new Earth Resources Group where they could monitor contracts. Meanwhile, Paul brought in a really, really high-quality group of research scientists. Many were young guys in the beginning, but it grew into a group that anybody would be proud of to have in their organization.

I got a new boss, Dave [David W.] Strangway, and he was the first supervisor I'd ever had with whom I could actually hold a technical conversation. He was a geophysicist.

JOHNSON: What year was that happening?

MENDELL: When Dave Strangway came in, it would be about [1970]. He was a PI [principal investigator] on an experiment on Apollo 17 that was a Surface Electrical Properties Experiment, and he brought in graduate students with him. I shared offices with his graduate students, who are now professors in various places. Dave was a really, really nice guy in addition to being very, very smart.

Just as an aside, after Apollo was done the University of Toronto [Ontario, Canada] contacted him and begged him to come back. The reason was that the Geology Department and the Geophysics Department at Toronto were at war with one another and he was the only one that everybody respected enough to maintain peace. So he went back to the University of Toronto.

He spent a few years as department head [chairman of the Geology Department], and then he was going to step down, but there was a competition for a new building to be built on campus. It was to be a large modern building, and it would have to be multidisciplinary. Various departments collaborated to write proposals as to who was going to be in that building. Dave led a proposal with a couple of the humanities departments, and they won. He had to stay as department chair for another year while they built the building.

He then applied to be president at the University of Toronto. He was one of the final two. Although Dave was not selected, the winning candidate died in an auto [automobile] crash. So Dave Strangway became the president of the University of Toronto for a few years. He subsequently moved to the University of British Columbia [Vancouver, Canada], where he was president for about 12 years. He was a capable guy. Really, really, really, really good to have been able to work with him.

After Apollo concluded, scientific study of the Moon was a major enterprise. Scientists came to MSC from around the world. The Lunar Science Institute was established outside the [JSC] gates at the old [James M.] West [Sr.] mansion in order for visiting scientists from other countries to have a place where they could work without having to get inside the gate.

A lot of really famous people, Nobel Prize winners and so on, came there. Every Friday there was a seminar. Speakers were world-renowned scientists. For people like me who wanted to learn and hear the latest ideas, fit was just nirvana.

After a few years, the planetary science community whose research was supported by NASA began to push back on funding for lunar studies, arguing that the rest of the solar system should be the emphasis for NASA missions. Research on the Moon became less scientifically glamorous or less politically “in.”

Meanwhile, in 1976, I finally finished my [PhD] thesis. It took me forever, and I set a record at Rice for the longest time in the department. By the time I scheduled my thesis defense, my advisor had left Rice. He got his old advisor—because he’d gotten a PhD at Rice—to chair my committee, who didn’t know anything about what I was doing. But none of the professors knew what I was doing. I just presented a bunch of slides on papers I had already written, and they all were looking at their watches. Thirty minutes later I was done.

After the last Apollo lunar landing, the mission where I had an instrument that was my doctoral thesis, I was funded for a few years to work on the data returned by the instrument. That funding eventually stopped, and I successfully submitted a research proposal to set up a laboratory to study thermal properties of the lunar surface. I didn’t do a particularly good job in setting up the laboratory equipment and produced few results. I was doing work that few people were interested in, but 30 years later the topic became quite important and I was considered a

pioneer. I remember this young guy came up to me at a conference a few years ago and said, “I’m really glad to meet you because you’re the guy who started all this work, and it’s fabulous.” Mythology is wonderful.

In the 1960s, I had my palm read by a friend of mine who was a retired professor of English at the University of Houston, and she told me that I was going to have two careers. There’s a fork in my palm somewhere. Well, it turns out it’s true.

On one hand, I’m part of the planetary science group. I’m a physicist while most everybody else there is a laboratory geoscientist. In other words, they work with physical samples that they put into instruments to take measurements. At one point, when funding for lunar samples declined, the laboratory scientists began to study meteorites. They always need samples.

I’m a physicist, and the things I know about are related to remote sensing or astronomy. To some degree, I’m a fish out of water in this group, although it’s not like anybody was ostracizing me. Actually I’m one of the oldest ones there. But I don’t have many colleagues whose discipline is similar to mine, and I had to pick up geoscience by osmosis.

In 1980, [Ronald W.] Reagan is elected president of the United States. The party platform is that we’re going to have less government, and we’re going to cut back on programs. All of us who were civil servants at NASA, particularly those of us who had to exist on research proposals, were pretty nervous.

The external planetary science community began to form political action groups. The Planetary Society is a public advocacy group that was formed at that time. There was another PAC [political action committee] or something that was formed during that time of planetary scientists. But as civil servants we can’t lobby, and we can’t have anything to do with that.



Meanwhile, another event that had spurred all this angst was that President Reagan appointed Hans [M.] Mark to be the [NASA] Deputy Administrator, but he didn't appoint [NASA Administrator James M.] Beggs until later. There were some issues about Beggs and his attachment to General Dynamics [Corporation]. So for a while NASA had a Deputy Administrator but not an Administrator.

The Deputy Administrator had written a memo, which got leaked in late '81, that suggested NASA just really focus on making the Shuttle operational and fulfill those promises, and maybe cut back on other things NASA does, such as planetary science. Planets are always going to be there, so that budget could be applied to the Shuttle.

If the funding for research was going to shrink, our research proposals would be evaluated in a more competitive environment. We had to figure out whether our group would have to change our approach to research to be successful in the new environment. Mike [Michael B.] Duke was the Division Chief, and he initiated a strategic planning exercise in 1981. We had to try to address questions about whether we should write group proposals, whether we should concentrate on Mars or meteorites, or how much of the lunar studies we could keep going. We were just trying to explore all possibilities.

Meanwhile, the Space Shuttle is about to finally begin operations. We wanted to find out more about its capabilities to determine whether it could help a long-standing need in lunar exploration. As the remote sensing instruments operated on the J-series of the Apollo missions, the sample scientists began to realize that this new data from orbit could tell them what elements were there and allow them to extrapolate the lunar sample analyses to the rest of the Moon.

The orbital data and the ground truth from the sample analyses could provide a way to construct the history of an entire planet. It was a really exciting concept. However, the Apollo

remote sensing instruments operated only in equatorial-like orbits only for three days. To explore the whole Moon to the spacecraft must be in a polar orbit so that the Moon rotates under the orbit path, and it must be there for at least six months.

The lunar science community began lobbying for a Lunar Polar Orbiter, arguing that it could be a really simple spacecraft with nothing but the Apollo instruments on it. They lobbied the NASA Administrator as Apollo was being canceled. The response was to offer to make Apollo 17 an orbital mission instead of landing.

In about 1975—I think when Dave Strangway may have still been there, I'm not sure—we got [Center Director] Chris [Christopher C.] Kraft [Jr.]'s attention. We wanted to submit an unsolicited proposal to NASA Headquarters to fly a Lunar Polar Orbiter mission, and Chris Kraft got behind it. His reasoning was that a demand for small payloads would emerge in the coming Space Shuttle era.

JSC had no management experience in doing small-payload projects, and JSC had the reputation of being extraordinarily expensive and slow in executing projects. Kraft thought that this small Lunar Polar Orbiter project would be a great opportunity to form a new organization for doing small projects rapidly without all the overhead that the manned space program takes.

Glynn [S.] Lunney was going to be the Project Manager, and was a candidate to head the new office for small payloads [Shuttle Payload Integration and Development Program Office]. I acted as mission scientist for the proposal. We submitted it to NASA Headquarters. Because Chris Kraft was behind it, they couldn't ignore it. John [E.] Naugle, who was the head of Science [Associate Administrator for the Office of Space Science] at the time, didn't know what the hell to do with it.

NASA HQ came up with a political solution. They declared that accepting the JSC proposal would amount to giving directed work to a specific field center. The idea had merit, but there should be a competition between all NASA Centers for this project.”

Three field centers decided to compete. JSC was unique in having the world’s premier lunar science group. JPL had unique experience in building spacecraft for planetary exploration missions. The [NASA] Goddard [Space Flight Center, Greenbelt, Maryland] had experience building robotic spacecraft, but the missions were mostly Earth science or small astrophysics missions.

The proposals come in, and the JSC proposal was cheapest, as I remember, and had a high-quality engineering team on it. However, the evaluators didn’t believe that JSC could perform as described in the proposal, because JSC had a reputation of cost overruns and bureaucracy. JPL was the most expensive because they’re always highest. The project was awarded to Goddard, who had nobody there to actually work on science issues. After one year, nothing had happened. The project was shifted to JPL. They did a design, but the mission was eventually canceled. In Bruce Murray’s book [*Journey into Space: The First Thirty Years of Space Exploration*] he mentions this, and says that he tried to save it but the insiders in the lunar community don’t remember it that way.

All the people I know who were senior people with Headquarters connections at the time say that Bruce Murray did not actually try to save this because he didn’t want to go to the Moon. He wanted to go to Mars and out into the outer solar system because those are the big expensive high-technology new missions.

He had made a famous speech where he told the employees at JPL, “What Congress wants are purple pigeons. Your proposals you turn in to NASA have to be purple pigeons that

have a lot of pizzazz to them.” So for the [Sixth] Lunar Science Conference in 1975 I think it was, I designed a T-shirt that had a moonscape. There was a purple pigeon standing on it looking at a gray mouse, because the Lunar Polar Orbiter was just a poor gray mouse. Steve Sanders told me Bruce saw it, and thought it was hilarious. We didn’t get the Lunar Polar Orbiter.

Now it’s 1980 in my narrative. Our Division under Mike Duke is conducting a strategic planning exercise. One question that emerged was whether the Space Shuttle could launch Lunar Polar Orbiter. Nobody was clear on how missions were going to be funded. Perhaps there would be budget line item for building and flying Shuttle payloads.

Jeff [Jeffrey L.] Warner, who was one of our PhD geologists, and I trekked across the Center to actually talk to people in the Engineering Directorate. That was *terra incognita* to us scientists. We explained the Lunar Polar Orbiter to them and asked if it would be possible to launch something like it from the Shuttle bay.

The answer was yes. Not only that, the Shuttle had the capability to launch something much grander than we envisioned. We got pretty excited because maybe there’d just be enough money floating around to carry out such a mission.

While I was over there, I got their view on what was the future of the human space program. NASA was going to be a transportation agency, basically FedEx [Corporation] in space. Astronauts were going to be truck drivers. The Shuttle was the first element for what was called the Space Transportation System. The second element was going to be a low-Earth orbit space station, which would be like a depot.

After the Space Station they were going to be building Orbital Transfer Vehicles, unmanned vehicles that would take packages and deliver them to places deep in space,

geosynchronous orbit, planets, wherever you wanted to go. The STS was going to all be done by 1999.

Then they told me a really interesting thing that, as a physicist, I should have known. Everyone assumed that the Space Station would have an orbital inclination of 28 degrees because that is the latitude of Cape Canaveral. These Orbital Transfer Vehicles at the Space Station would have to have enough delta-v [change in velocity] to change orbital planes to the zero degrees inclination of geostationary orbit.

It turns out that if the transit vehicles have that much capability, they can also go to lunar orbit. It just happens to be a characteristic of the combined gravitational field of the Earth and the Moon. I realized that once this Space Transportation System was in place we could basically write "Moon" on the address label.

So the big question was whether these Orbital Transfer Vehicles could carry people or carry a Space Station module? NASA planners were sizing them to carry what they thought were going to be future communication satellites, and they are smaller than those we actually see today. The planned vehicles were not going to be really capable enough to support future human activities on the Moon.

We began arguing that the next thing to do was to go to the Moon with people because a transportation system would exist to support a program, assuming that the mass throughput of the system was large enough. Nobody wanted to think about it, because going to the Moon would be like Apollo and therefore unaffordable.

But we persisted because there was a lot of science we could think of to do on the Moon. We created a narrative, but we could not present a credible mission architecture because we weren't engineers. We lacked that skill.

We gave a presentation in February of 1982 to the head of the Engineering [and Development] Directorate, whose name was [Robert O.] Piland. We requested some time from a couple of engineers to help us develop a mission architecture. (We didn't know that term then.)

His deputy was Al [Allen J.] Louviere. They listened to our presentation and Piland says, "Well, Engineering Directorate is really busy, and we just don't have anybody we can spare. I think you should go back and talk to the scientists."

We looked at one another and said, "Wait a minute, we are the scientists." We left. We go back and just are holding our heads. Two weeks later I get an envelope—what we called "holey joes," [interoffice mail] you know that term. Inside is a proposal and sent to us by Al Louviere.

This proposal had come to NASA Headquarters out of Los Alamos [National Laboratory, New Mexico], and it was a proposal to develop a plan for a lunar base. Every Center was asked to evaluate it and send the evaluation back up to NASA Headquarters to Hans Mark. Al sent it to us because no one else knew what to do with it, and he knew we had been thinking about the topic.

I remember standing in the hallway thumbing through this proposal leaning against the door and saying, "This is all the stuff we're talking about. I've never heard of these guys. How can it possibly be that there's all the same ideas here?" So we got in contact with Los Alamos.

The backstory was that Edward Teller and Hans Mark, and Donald [M.] Kerr [Jr.], who was Director of Los Alamos at that time, were out on a sailboat. They're talking about bases on the Moon in the future. Hans Mark and Edward Teller were excited about the subject, and Don Kerr decides that Los Alamos might get some study money if they write a proposal about future bases on the Moon and send it to Hans Mark.

One of the authors of the proposal was Dr. Paul [W.] Keaton, who had been a Division Chief at Los Alamos, a senior management position. He's a fantastic physicist, a high-energy physicist. He had gone to CERN [European Organization for Nuclear Research] in [Geneva] Switzerland for a sabbatical year, and he had come back to be on staff to the Director until a position for him opened up.

While Kerr's staff was trying to figure out a fit in the organization, this thing comes, so they assigned him to write the lunar base proposal. His point of view was different than ours. We framed the problem in terms of the evolution of the NASA human space program.

Paul started with the assertion that 100 years from now space is going to be a very important sphere of operations, and there were going to be people on the Moon and elsewhere in space. He then constructed a narrative connecting the present to that future. He was doing a different kind of visioning than we were.

But we were both on the same page, and we got in touch with Paul. That was the beginning of a triumvirate, a troika: Mike Duke, Paul Keaton, and myself. I was the junior guy, but I was the public face. I was the person who made the presentations and who was the name people would see, because these other guys—first of all they were fairly—had responsible jobs, and that wasn't their bag to go out and stand in front of people and advocate. That's not who they were; they weren't Neil deGrasse Tyson.

We began working together, and we knew that Hans Mark was secretly enthusiastic about the subject. Within the NASA chain of command, we could not directly approach Hans Mark and let him know that we are ready to jump at this. Mike and I had put together this story about lunar bases, but we weren't sure whether we were right. You can have an idea and write it down, but you need peer review if you are serious about taking it forward as policy.

In the fall, or maybe late summer, of 1982, Mike and I went on a five-day trip. We traveled to NASA Ames [Research Center, Moffett Field, California] and we went to JPL to talk to the chief scientist and senior staff. We went to the University of California at San Diego to talk to a very prominent scientist, Professor Jim [James R.] Arnold, and his department. We went to the USGS [U.S. Geological Survey] Astrogeology [Science Center] in Flagstaff [Arizona], and we went to the Lunar and Planetary Laboratory in [University of Arizona] Tucson.

Every morning I would give the presentation to senior staff there. We'd have lunch and talk about it. They would critique us, telling us what they thought was wrong or weak. Mike and I would go modify the presentation and proceed to the next place.

By the time we finished at all five places I concluded that the core of it was fine, and there were parts of it that A disagreed with but B liked, that C disagreed with but D liked. In other words, some of our points were opinions as opposed to facts. Nevertheless, we felt we had a really solid story, that we'd passed really smart people,

Senior lunar scientists liked our story and began to collaborate with us. It was convenient to use the annual Lunar Science Conference as a venue for getting the word out to the broader science community and for exploring some ideas to flesh out the narrative.

The papers delivered at the original Lunar Science Conference at the Rice Hotel in Houston served as contract reports from the scientists who had gotten money to look at a lunar sample. The Second Lunar Science Conference was more like a real scientific conference because NASA had changed the funding mechanism from contracts to grants. That conference has been going on for almost 50 years and for some reason has grown over the years. Today it is known as the Lunar and Planetary Science Conference, and topics cover the whole solar system. In those days it was about the Moon primarily.



We decided in 1983 at the Lunar Science Conference we would have a special session called “Return to the Moon,” and we would have several prominent scientists give a talk about why it was important and what the scientific community could learn. Hans Mark would be invited to give the introductory keynote speech, and I would be the last person to speak discussing our plan in order to begin a dialogue with him.

I submitted two abstracts, one of which addressed objections to the idea of a lunar base that I would hear when talking about it. These objections tended to be that it was unaffordable or that the U.S. did not have the capability to do such a project. For example, in the Reagan era it was thought that the U.S. had gone from an agricultural economy to a manufacturing economy, and now was moving to a service economy.

A pure service economy doesn’t actually produce anything. Everybody waits on everybody else. As a result, there would be no more economic growth, period. The economy would be static, and we could never go back to the Moon because there would be no growth in the economy.

There were other theories of various kinds, such as that the Japanese had all the innovative high technology that would be required to build a lunar base. So I wrote an abstract to go over all those ideas. The title is about that long [gestures]. The title was “*Why are you talking about a lunar base now when the Shuttle fleet needs a fifth orbiter, when the Space Station Project is sputtering, when planetary exploration has had no new start since 1978, when the national economy is in the worst shape since the Great Depression, when the Japanese are pushing for world leadership in technology, when the Soviets and the Europeans are challenging American preeminence in space and when video games are corrupting our youth?*”

A NASA engineer, who seemed to do nothing but walk around the site and come into offices, came into my office and said, “Wendell, people around the Center are really upset with you.”

I said, “Why?”

“Well, you wrote this abstract and you talked about video games. Why would anyone in NASA have—what gives you the authority to talk about video games?”

I sighed. Two days later, President Reagan gave an address at the [U.S.] Air Force Academy [Colorado] where he said, “We’re raising a new generation of youth who are going to run our fighter planes. They’re being trained on video games and they’ll be familiar with all the electronics.” This guy comes back in my office, “Oh my God, you were right.” That was the level of intelligence we were having to deal with.

At the special session in 1983, Hans Mark comes and gives a fantastic pro-lunar space speech, much stronger than we’d ever anticipated. His talk was followed by a series of presentations by prominent members of the lunar science community, and I am last. I presented the outline of our plan to return to the Moon, only to find out that Mark had to leave to catch a plane.

After every Lunar Science Conference, Chris Kraft liked to have a small group of scientists to come and just give him the latest findings. We broached the lunar base subject to Kraft, and he was receptive. He had been thinking about a new focus for JSC, and the lunar program could be a candidate. He was sure that the time of a few engineers would be available to help us. The conversation was like on a Friday afternoon, and on Monday my phone started ringing off the wall with engineers who were ready to come to work on our project. We got the services of two or three guys, part time and we learned all about rockets and space transportation

systems. I still have some of old presentations we put together about lunar bases. All this was great, but we still didn't have any funding source. Corporate NASA wanted to sell the Space Station, and the Space Station was the priority.

Even though we were just a bunch of grunts talking about a lunar base, it was getting newspaper coverage that NASA was working on a lunar base. It was not true, but if I'd give a talk about a lunar base, reporters would assume it meant that NASA is working on it. Even NASA engineers thought the Agency were working on it because they would read about it in the newspaper. But NASA HQ was unhappy because they didn't want Congress to be distracted by a potential lunar program when the agency was trying to sell the Space Station. We're trying to figure out what to do about this because and nobody's going to give us any money to do studies.

In August of 1983, Hans Mark came to JSC as part of an activity to visit all the NASA Centers. He spent one day at JSC, going from one technical division to another and listening to presentations about the organization. Our Division over in Building 31 was scheduled for the end of the day. He was accompanied by a little entourage, including the Center Director, Aaron Cohen, and other senior staff.

Mike Duke, our Division Chief, was out of town, and he had tasked me to give a presentation on a lunar base. It was a presentation I already had prepared for a general audience but for an audience that I assumed was skeptical that a base on the Moon was a good idea. My first chart started with the assumptions that the civilian space program is going to continue to exist, and that the U.S. economy is not going to collapse. After these noncontroversial assumptions, I tried to create a credible narrative that ended up with a lunar base.

For Dr. Mark, I added a final hand-drawn chart with milestones from the finish of the Space Transportation System and a base on the Moon by 2007, the 50th anniversary of Sputnik [Russian satellite].

However, a more urgent topic had to be addressed before I could talk. IRAS [Infrared Astronomical] Satellite had been launched, and was making an infrared survey of the whole sky. The data was showing all kinds of infrared objects, including certain objects in Earth orbit that the military didn't want people to know were there. Before the data was released to NASA, it was edited.

Our Division had started the space debris program. Don [Donald J.] Kessler, known as the grandfather of space debris, was an old friend of mine from early days. In our building, Drew [Andrew E.] Potter was the head of the space debris office. He was an infrared astronomer and understood the potential of the IRAS data. He wanted to ask Hans Mark if the space debris office could access a sample of the uncensored data to determine if new observations of debris in low-Earth orbit could be obtained.

In our little 45-minute segment with Hans Mark, first was going to be Drew, and then me. Of course, Mark was running behind schedule, and our 45 minutes is reduced to 35 minutes. At about 5:30 or quarter to 6:00, the entourage comes in our conference room. Drew got this first chart on the overhead projector and hit the button as soon as they sat down. He explains the problem and Hans Mark wants to discuss it.

Hans Mark and Drew Potter have a discussion about it, but nobody in the room knew what they were talking about. Because the other guys were just engineers and knew nothing about it. Space debris was just a myth in those days. Mark agreed to look into the issue.

There are five minutes left. I decided I will put on the title chart and then go to the little diagram I drew at the end. I put the title chart on that says, "Return to the Moon."

Hans Mark sits up. He says, "Now this is important." I decide to put on my second chart, stating that the civilian space program will continue to exist and the United States will not undergo an economic collapse.

Mark said, "That's wrong."

I said, "What do you mean?"

He said, "There is an enormous amount of Third-World debt which has been assumed by U.S. banks. The Third World is going to default on that debt. When they do, the banks are going to start going under. We're going to have to prime the pump in this country to build up the economy to handle this financial collapse, and I think a lunar base is a good way to do that."

"Look," I said, "I give this talk to garden clubs and to NASA managers. I can't start out my talk by saying that the economy is going to collapse."

He says, "Well, maybe you're right." We continue to go through my charts with a significant amount of give and take. Meanwhile, all these JSC high-level people are in the back of the room. They don't know who I am, and I am the very first person all day to actually have a dialogue with Hans Mark. Jaws are just dropping all over the room. I get to my last chart with the schedule and he says, "I want a copy of that." He asks, "Where do you get your money?"

I said, "We don't have any money. Nobody will give us any money."

He says, "Well, how much money do you need? You need to hold a workshop."

I said, "I know. We want money to hold a workshop, but we can't get money."

He said, "Well how much do you need, \$50,000 to hold a workshop?"

I said, "Yes, but we can't get it. Nobody will give it to us."

He says, “Okay, as of now you have the money,” points his finger at me. He turns to his horse holder and he says, “You are going to find the money for them.” His horse holders were always Air Force lawyers, and this one was the new Air Force lawyer, and didn’t know his way around NASA bureaucracy.

He calls me a few days later. He says, “I’m so-and-so from Hans Mark’s office. Now how much money is it going to be?”

I said, “\$75,000.”

He said, “Okay.” So he goes all around NASA and he can’t find any money, because the Deputy Administrator doesn’t actually have any money. Finally, the Office of the Chief Engineer gives us money and we have \$75,000 to hold our workshop.

Then I have the problem that I want to convene a small, elite group of really seasoned senior people to recommend, “Okay, policy-wise how do we do this? How do we structure it? What is the narrative, and what is the process we go through for it?” On the other hand, I also want to attract a lot of people to be excited about it. These are two different things.

At the end of the year I was in Burbank, California, in the office of the Lockheed vice president who used to be the NASA Chief Engineer, Stan [Stanley I. Weiss] with Mike Duke and Jerry [Gerald J.] Wasserburg, who was a very, very prominent lunar scientist. I explained my conflicting objectives. I don’t know how to spend the money, which one to do.”

Stan says, “Do both.”

With the help of Paul Keaton, we try to convene a small group, at Los Alamos, where it is easier to limit access. What we thought would be 15 or 20 turned out to be 35 because people found ways to put pressure on us for an invitation.

I was very excited to have Dave Strangway attend. I rode the bus from the motel sitting next to him and told him how happy I was that he could take time from being the president of a major university. He replied that he was really glad to get out of his office and come to this event.

The workshop findings were published as a Los Alamos report, *Report of the Lunar Base Working Group* (LALP-84-43). In the report we conclude that there are three basic long-term goals in a lunar base program: scientific discovery, utilization of resources, and colonization of the Moon. The choice of the last term was unfortunate, but what we really meant was settlement, long-term, permanent settlement. Those are the three cornerstones of a lunar-base program policy.

Now it is time for the second phase, a conference open to anyone. We construct the program of the conference, using the report of the workshop as a skeleton. We set up sessions with themes that follow the outline. Attendees from the workshop volunteer to give a keynote speech for each session. We had no idea who was going to come because normally the attendees at a conference are people who are being funded for work on the subject matter. Nobody's being funded. There is no money, there is no program, and there is no project.

We announce a conference called "Lunar Bases and Space Activities of the 21<sup>st</sup> Century." Mike [Duke] is smart enough to schedule the conference room at the National Academy of Sciences in Washington, DC, which is relatively small but is a prestigious location. We had no idea who was going to show up.

One man who had developed a plasma torch, based on NASA technology, for reprocessing mine tailings in Minnesota heard about it on the car radio while driving late one night. It was a time in the top-of-the-hour news bites that are on music stations. More generally,

we got some people who had worked on a funded technology study in the early days of the Apollo program when there was money. Such folks based papers on a big report. Other people had some kind of new idea they wanted to share. We vetted such papers to make sure they had technical value.

Then there was a class of participant that I hadn't anticipated. I hadn't realized that the science adviser for Reagan was against the Space Station, George [A.] Keyworth. He was a former Los Alamos division chief, so Paul Keaton knew him well. Paul recruited Keyworth to come and talk at our conference. As soon as James Beggs heard that Keyworth was talking, he wanted to talk at the conference. Edward Teller was there, and Krafft [A.] Ehrlicke, and Jack [Harrison H.] Schmitt gave a talk.

We had serious luminaries giving talks at this conference, in addition to a group of participants who were enthusiastic. We had about 300 people. We had some kids from SEDS, Students for the Exploration and Development of Space, as volunteers to run projectors. One of them was Todd [B.] Hawley, who was later the founder of the International Space University [ISU].

That conference resulted in the book *Lunar Bases and Space Activities of the 21<sup>st</sup> Century* [W. W. Mendell, editor, [https://www.lpi.usra.edu/publications/books/lunar\\_bases/](https://www.lpi.usra.edu/publications/books/lunar_bases/)], which is actually pretty well-known these days, and turned out to be a seminal book on the whole lunar base advocacy. One problem with a conference proceedings was that people either wanted to write a long report from a funded study in the '60s or just a few pages on a new idea. We dictated that all the papers would be short, and I organized them according to themes. I wrote a little foreword for every section to explain what the context of the topic was and why it was important.



Pat Rawlings [space artist] did little drawings inside and a painting for the cover. The Lunar and Planetary Science Institute published it, printed about 3,000 copies, and first sold them, and then sold them for a lot less, and then started giving them away. Now people are looking for them in used bookstores because it has held up pretty well.

Now we suddenly have a lunar base community. Within the next year I saw for the first time a viewgraph or chart from NASA Headquarters that had a lunar base on it, down near the corner. During this time I become the lunar base guy, just because I was out in public and I edited the book. People think there is a lunar base program, and that I'm the guy in charge of it, and that I'm important—none of which is true.

Meanwhile Hans Mark was really on our side, but he couldn't say anything. He and I would exchange little handwritten notes kind of under the table. He would ask, "How's it going, what are we doing?" I'd tell him. I remember sorting through the mail for our division and putting it in different boxes, and there's this little NASA envelope I'd never seen before that looks like an invitation to a party. It's addressed to me, hand-addressed. It's a note from Hans Mark; I can't believe it. I said, "I know who I'm hiding from, but who's he hiding from?"

Aaron Cohen was on board with us. He says, "Just don't tell me what you do." We were doing a lot of talking at various levels outside of the chain of command. Paul Keaton was well connected, and we could get in all kinds of places in the Administration. National Security Council, a lot of places.

That was a part of my career which is not your NASA standard history. Suddenly I'm off chasing some rabbit, and I remember something that Dave [David S.] McKay said to me as we were walking to lunch one day. "Wendell, you've got to stop this lunar base stuff. You're going

to lose your scientific expertise and you'll never be able to get back into research, because you will just be so far behind."

I said, "Dave, I know, you're right. But about every 10 days something just absolutely amazing happens, and I've just got to follow this string and see where it goes."

There was another time when I was walking to lunch with a group from Booz Allen [Hamilton Inc.]. A young woman, who's a typical Booz Allen bright employee, says to me, "What do you do?"

I said, "I don't really know how to describe what I do." I said, "Let me just tell you what I've been doing today and yesterday." I went through a list of things.

She says, "Oh, you're in marketing."

At meetings at JSC where people did not necessarily know everyone, you have to go around the room and say what your job is. I would say, "My name is Wendell Mendell, I'm in the planetary science group, and I'm the JSC resident philosopher." That would always stop the room. Some people would laugh but others were a bit put off.

Oh, by the way, one interesting happening—early on, like '83, '84, John [M.] Logsdon called us and asked to come visit. We knew who he was, and as we chat with him, and he says he is interested in helping us. I asked, "Why?"

He replies, "Well, you can just sit and watch for so long." And we've been friends ever since.

There's a whole narrative that happened after that, and I could go into it. For me personally, the next big thing that happened to me was in late 1987 when a person named Peter [H.] Diamandis called Mike Duke and wanted to come and meet with him and talk about lunar bases. Peter Diamandis says, "Okay, we've got this new thing we've started called the

International Space University.” There had been a Founders’ Conference in 1987 at MIT [Massachusetts Institute of Technology, Cambridge]. It was basically a group of MIT graduate students who had a vision that they were going to create a university with all the things universities have, but everything was going to be related to space. Interestingly, two of the MIT graduate students involved in that activity were Dava [J.] Newman and Mike [Michael J.] Massimino.

You could come to this university and study not only technical but business, policy, and design, as well as engineering and science. But everything would be related to space, and it would be international. People would come from all over the world. That was a key element. The whole idea was to create an international community of people with a common vision. They were going to start it by having a summer program 10 weeks long. The first session was going to be at MIT, and they found money to hold it.

The general format of the program would be half lectures, academic, and half a design project the students would work on. The very first design project would be a lunar base, starting from a scientific outpost, and over 30 years evolving into a commercial operation. These international students were going to study that, and they wanted us to lead the project.

I said, “Well, this is interesting, I think it’s really interesting.” I said, “But I’m beginning to realize that I’m making promises I can’t keep or that I don’t keep. I don’t want to sit here and look at you and say I’m going to lead this project, and then not perform.” I said, “What I would like to do is to find someone who would lead the effort, whom I would help. I would be the technical adviser.” He said, “Okay.”

Not long after, I found Dr. John [W.] Alred, who was an engineer in what was then Advanced Planning [Advanced Programs Office]. He’d actually done some thinking about lunar

bases and had done a bit of work here at JSC. He had a PhD, he'd been teaching at Texas A&M [University, College Station] for a while, and he loved the idea of working with international students. He loved the whole idea.

After the [Space Shuttle] *Challenger* [STS-51L] tragedy during the downtime, [NASA Administrator] Fletcher, who was not a very visionary person, was trying to figure out what to do. He decided to have somebody take a look at the big picture and asked Sally [K.] Ride head up a commission and issue a report. Sally Ride's report [NASA Leadership and America's Future in Space: A Report to the Administrator] posited four big initiatives that could be the theme of NASA. One was Mission to Planet Earth. One was robotic exploration of the solar system. One was outpost on the Moon, and the other was a piloted mission to Mars.

If you really think about it, one and two are an order of magnitude smaller than three and four. Four is probably 50 percent bigger than three. They're apples and oranges. But then Fletcher decided to form an Exploration Directorate and asked Sally to head it, but she declined, leaving NASA for academia.

The person who was put in charge of it was a JSC former flight controller named John [W.] Aaron, who really didn't have any background in any of this. He went to NASA Headquarters and put together an exploration organization [Acting Assistant Administrator, Exploration]. We had done a lot of work, but that organization ignored it and started from scratch talking about Moon and Mars on an equal footing. We had already thought about that and decided Mars was just too hard; Moon was the first step. We were pissed.

Because John Aaron was from JSC, he wanted engineering support from JSC, and John Alred was picked as the guy to head the support group. John really wanted to teach at the International Space University, but he was being told no. Joe [Joseph P.] Loftus got involved

and managed to broker a deal where John would spend half his time in these 10 weeks in Boston [at MIT] and half his time here. I would tag team with him in Boston.

I'm a scientist. I don't know anything about project management, so I didn't have a clue about setting up a design project. John did. He set up a project plan and had teams in charge of the habitat, in charge of the rocket, in charge of the science, and so on. I was very happy with what he did. There's a lot of things that happened at that time. That's a whole other story I should write about.

In the end we did turn out a report. There were a hundred students there, and some amazing things happened, and it was chaotic. But I thought, "Wow, that was something. I'm glad I did that." In the fall I got an invitation to participate in the second International Space University session in Strasbourg, France. I had no idea that this would ever happen again, but it did.

This time I didn't want to lead a design project because I was not really interested in the topics chosen. I became part of the faculty in the space physical sciences. Then in '90 it was held at York University in Toronto [Canada] and I served as faculty again. Then in '91 they wanted to hold it in Moscow because Todd Hawley and Peter Diamandis and those guys worshipped the early Russian engineers and cosmonauts, and they had always worked really hard to get Soviet participation in what we did.

It was going to be held at the Moscow Aviation Institute [National Research University]. This theme was going to be a human mission to Mars, because that's what the Russians were interested in. The Soviets and us were going to go together to Mars, and the International Space University was going to write the plan in 1991 at the session.

As you know, in 1990 the Soviet Union collapsed, and around December the Moscow Aviation Institute sent a telegram to ISU headquarters and said that they didn't think they could feed people, much less provide security. At the last minute, the aerospace community in France threw together some facilities in Toulouse where their aerospace center is [Aerospace Valley]. We held the session there in Toulouse, and I was the leader of the design project. There were 135 students, the biggest there's ever been, because everybody thought they were going to Moscow.

Because there were 135 students in the design project, I had a staff. I had alumni who were project assistants, I had faculty. It was now a management system. It wasn't like you were sitting down with students doing this, we had layers. It was a pretty intense and amazing effort, but we got it done and wrote a huge report.

There were a lot of really fun things happened. For a while the whole student body was convinced that I was a CIA [Central Intelligence Agency] agent sent by the [President George H. W.] Bush administration to take over the project and make sure that the report was consistent with what the U.S. wanted to do. Later on the Russians, at the very end, wanted to not publish the report because it had content in it that their government didn't like. My blood pressure was through the ceiling. Got it all resolved, but there was a lot of stuff that happened.

I was with the ISU for about 10 years in the summer program and did a couple things later in 2000 and 2004. But other than being on the list and have a title of emeritus professor, I don't really interact with them much. But when we were doing the Space University sessions, the faculty that they had put together were really focused on giving the best academic presentation and training we could within the constraints of the system. That's all we talked about.

Todd and Peter would talk about the “old school tie,” that there would be this network of people around the world who’d gone to ISU. I used to be dismissive of that idea. Well believe me, that’s what happened. There is now a mafia all around the world of people who are in pretty senior positions who are former ISU and they all have a link. I suddenly realized all this in the year [1999] at the International Astronautical Congress in Amsterdam [Netherlands] when I attended a meeting of the AIAA Technical Committee on public policy.

Then around 2000 our Division at JSC underwent a metamorphosis, and we became a Directorate, and I was involved in that transition. I’m the one who created the acronym ARES [Astromaterials Research and Exploration Science] for the new organization. There’s a whole story behind that that is interesting.

We were a very small directorate, and also one that didn’t have a lot of clout. But when [President] George W. Bush made the 2004 [Vision for Space Exploration] speech we thought that our time had come. In fact, when George Bush made his 1989 speech [Space Exploration Initiative (SEI)] I went to Mike Duke and said, “Okay, we started work in ’82 on this, and this is exactly what we wanted. We said we had to get to the president, the president has made the speech. Our work is done, what am I going to do?”

Little did I know—work was not done. When the Space Exploration Initiative collapsed politically I was really, really down. I saw things that NASA did which were really stupid, and I was of the opinion then and now that NASA needs to be broken up and a new organization created that’s focused on science and exploration. That’s another story.

JOHNSON: You mentioned the Ride Report earlier, and of course part of that was that outpost on the Moon. I had read somewhere that you actually helped or were working with the Ride Report.

MENDELL: No, I did not. As far as NASA organizational processes are concerned I don't exist, I'm invisible. I know a lot of people. People talk to me, people call me. But organizationally I do not exist. I created a card for myself, "Chief of Lunar Base Planning." It's a totally made-up title. I don't exist. In NASA, you only exist if you have a position in an organizational line, so I never existed.

I'll tell you a little funny anecdote. In '84 my daughter was graduating from high school. I took her and one of her girlfriends and my son to Los Angeles [California] to spend a week and go to Disneyland. I wanted to show them Caltech. It was the week before the Olympics, so Los Angeles was deserted because everybody had left town. We spent that time there.

As I am planning this trip, I realize that one of my classmates at Caltech who was a couple years ahead of me is now a vice president at TRW [Inc.]. I decide to call him up and offer to come visit him with my kids while I'm in the Los Angeles area. I've talked to him a few times off and on over the years, but we're not really in contact.

I call his office in Los Angeles. When he was at Caltech everybody called him Skip, which was a childhood nickname. When the secretary answers the phone I say, "This is Dr. Wendell Mendell from the NASA Johnson Space Center. Can I please talk to Skip [John P.] Stenbit?" There's this silence and she says, "Who?" I suddenly realize I don't know his first name.

I don't know if I just shortened it to Dr. Stenbit or not. It was clear that she wasn't at all interested in connecting me, but she did go and find out. He was on the phone in a minute, and said, "Great to see you, love to see you. Yes, come on by, we live in Palos Verdes"



We all went over there and had a little barbecue and it was really nice. We visited. We start talking about the future of the space program, and he admits that he is not really interested in the civilian space program. He's interested in the black side. I think he was a deputy secretary of something or other, defense or something, at one time. That's where TRW gets their money. The civilian stuff is peanuts and the engineering challenges are not as interesting to him.

I talk to him about a lunar base. Our story built around the Space Station because that architecture would be consistent with NASA plans. You had to use a space station to get to the Moon, so that was the story. We talked about all that, and we talked about the future of humanity and NASA. We have a nice conversation and agree to disagree.

I go home, and on Monday morning I get a phone call from the local TRW office. Maybe it was Tuesday morning. The secretary wants to talk to Dr. Mendell and is a little discombobulated because I answered my own phone. She was expecting to go through a bank of secretaries. She said, "I'd like to make an appointment with you to have a couple of our people here come and talk to you."

I said, "Sure."

She says, "When would fit in your calendar?"

I said, "Whenever you want." On an afternoon, a day or so later, two guys come down to Building 31 and they're looking around. They find me sitting in my office. They come in and introduce themselves, and they're clearly a little disoriented. It turns out that in TRW there had been a battle raging over whether or not TRW should bid on Space Station contracts. Dr. Stenbit had been the person who had argued strongly that it wasn't worth their time to do that.

On Monday morning he came into the office and announced that he finally understood what the role of the Space Station was in the future of the program, and they might want to get

involved. It was such a 180-degree [turn] that the whole office was taken aback, and the story comes out that a Dr. Mendell had visited him over the weekend and talked to him. They wanted to know who this guy was and what was his role and how he managed this.

In 2004, President George W. Bush makes a big important speech on space policy. I have spent my time on the international scene since '92. I was elected to the International Academy of Astronautics as a full member. I decided that in order to sell my ideas about the future of humans in space, I needed to work through the international community because NASA had basically shot itself in the foot with Congress. Whatever NASA comes up with is not going to sell, so initiatives needed to come from the outside.

In 2000, after some departures from our Division, I got appointed Acting Deputy Division Chief, and then I needed to apply to be the real Deputy Division Chief. In those days you had to write something called a KSOAC [Knowledge, Skills, Abilities and Other Characteristics], which was a list of your qualifications. There were a series of questions you had to answer showing what you had done.

You had to demonstrate that you could lead people and so on. As I start reading over what my experience narrative has been that shows that I can lead an organization, I realize that none of it was in NASA. I ran a youth soccer organization, I ran the International Space University design projects, I chaired technical committees in professional societies. All this stuff that I had done in "leadership" positions was not in NASA. I couldn't believe it.

But the situation was such that I was pretty much wired, and I got the job. Then we transitioned to a Directorate, and the Vision for Space Exploration came along. We were waiting for the phone to ring because we're still probably the highest concentration of lunar expertise, certainly in NASA. The phone never rang.

We find out that the engineers working on the new exploration initiative are not really concerned with learning about the Moon. They assume that if they call on scientists, they will be lobbied for research funding when they actually need information for designs.

Fortunately the good news was that my boss was Steve [Steven A.] Hawley, who was one of the best bosses I ever had. Fantastic, fantastic individual. He could walk in anyplace in the Center. He went over and talked to Jeff [Jeffrey M.] Hanley and they agreed that I would be detailed in Constellation. What was my title? Something about lunar and planetary.

JOHNSON: Chief of the Office for Lunar and Planetary Exploration.

MENDELL: Yes, that's right. When that was announced there were reactions all around the Agency, because they thought I was taking over JPL. Actually there were only two people in the office. There was me, and they had wanted to recruit Chris [Christopher P.] McKay at Ames, whom I know because he's the Mars guy.

So there was the lunar guy and the Mars guy, and that was all. But our box was in the same level with all these boxes that had hundreds of people in them. The perception was that I was leading a group of hundreds of people. After some clarification, the disturbance in the force subsided. I acted as a gadfly, and Jeff gave us some money and I was able to make some good use of it, I think.

Then Constellation died [program cancelled in 2009]. That ended my funding because I was not associated with other programs. Over the latter half of my career, prior to Constellation, my expenses were primarily administrative, particularly training (conference fees) and travel. I

was able to find support for such low level activity because my management believed I was doing useful things. The NASA accounting system has always had some slop in it.

In fact I spent about \$6,000 or \$7,000 of my own money on travel in the '90s just to go to some of these international events where I felt I was making a difference and NASA wasn't interested in supporting them. My international colleagues understood that. I was doing it because I felt it was the right thing to do and not because I was being assigned to do it.

I finally decided that it was time for me to leave, and waited for 50 years to get a really big plaque. I spent about the last year of my time trying to digitize my office, because I had a lot of papers and a lot of books and documents. I couldn't take them with me home, so I tried to scan and digitize everything. My departure might be called a soft exit because my office was not totally cleaned out on the last day. There were some things happening where I was mentoring some younger people who have later become important in current initiatives. But I still go over once a week.

Oh, there was one thing that I had mentioned to you before, but I really should expand on it. After the Space Exploration Initiative failed with the Democratic Congress in '92, the staffers went through the NASA budget, and everywhere they saw the word explore or Moon, they put a big 'X'. Everybody who had been working on that was supposed to be reassigned to the Space Station or some other program.

Some younger people at JSC had done really good work. Aaron Cohen wanted to save them to continue think about future exploration architectures. From a suggestion by Mike Duke to Carolyn [L.] Huntoon, they were hidden in Building 31, because nobody ever looked there. So John [F.] Connolly and [B.] Kent Joosten and Bret [G.] Drake, Eileen [K.] Stansbery and some other people were given offices in Building 31.

These engineering design professionals had been working with our science group. They knew us and we knew them. We had a good relationship. We all realized there's two sides to future planning, the science side and the engineering side, and they had to work together.

They put them in an office that was on the org [organizational] chart and was headed by Dave McKay. The idea was to nurture dialogue between the engineering side and the science side to where they could get familiar with one another and develop common terms of reference. After a really stupid decision by the Division Chief, Dave McKay got so angry that he just resigned his position.

One of the things that he had set up was a weekly meeting where we would just talk about common issues. He asked me to start hosting that meeting, and I did. Because of my personality, which is a little bit off the wall, the meeting became a very free-form, Merv Griffin [television host] kind of talkfest. However, I was pressured to write an agenda every week because some people would not attend a meeting without an agenda. Every week I would construct a meeting notice and mention some topic relevant to human space exploration. My agenda evolved into a blog, and I would write all sorts of cryptic things about rumors I'd heard and oblique references about other things that might be good to say directly.

We had postdocs [postdoctoral fellows] in our building who were there for a couple of years. We also had transient students such as interns. They loved to come to the meeting and hear discussions that would not normally take place in formal settings. Such people would ask to be kept on the mailing list even after they left, and I always had people from the old exploration group on the mailing list. At one point my mailing list got up to about 100 people who were actually all over the world and all over NASA, including NASA Headquarters. I would write these things, and every so often I would hit the send button and instantly have Sender's Regret.

Astronauts, mostly science astronauts, came to the meeting, as well as senior executives. Neal [R.] Pellis has often said that it was the best meeting on the planet. People loved it. Don [Donald R.] Pettit used to come whenever he could, and Stan [Stanley G.] Love, and Linda [M.] Godwin came as long as she could while she was here. The reason was because we talked about all kinds of stuff, some of which was not necessarily about NASA, but was still intellectually engaging. We would be brutally honest about things that we felt were wrong or thoughtless. It was very free-flowing.

In my last year there, as my career and effectiveness were really winding down, I got a note from [NASA Administrator] Mike [Michael D.] Griffin. Before I say that—I would go places and I would meet somebody I didn't know and they would say, "Oh, I love your e-mails." I found out that they were being forwarded all over the Agency and elsewhere. The last year, Mike Griffin sent me an e-mail saying, "Wendell, every week somebody cuts out part of your e-mail and sends it to me. Why don't you just put me on the list?" And I did for the short time I had left.

A person from the JSC History Office came for a period of time until his contract was up. As he left he told me, he says, "This is like history. You need to preserve those." He talked about the Tindallgrams [memorandums of engineer Howard W. "Bill" Tindall, Jr.]. He says, "This is like the Tindallgrams, you need to preserve that." So I've archived 15 years of those e-mails with the mailing list.

Occasionally there would a reply-all to the list from someone like Doug [Douglas R.] Cooke at NASA Headquarters. He would give an alternate view to my commentary. At times like that I would realize that I had forgotten who was on the list. That was another kind of thing

that I became known for—within a sub community once again, not acknowledged organizationally.

I got a call in 1995 from a *Wall Street Journal* [newspaper] reporter in Japan. He said, “I have been visiting with the Shimizu [Corporation] construction company here in Japan, and have found out that they have a subsidiary devoted to building a base on the Moon.” Shimizu is a 200-year-old privately owned design-build company like Bechtel [Corporation]. He says, “I couldn’t believe it. They said that you’re the one that I should talk to about this.”

I said, “Well, it’s true. I’ve been working with those guys since late ’80s, and they’re very dedicated to this. They’re good guys, they do good work.”

He says, “This is just fascinating.” He said, “I’d like to call you and ask some questions.” We talked briefly and then there was silence for a several months. He called me again maybe in early ’96, something like that. He said, “I’m sorry. I really want to write this story, but I’ve been covering the medfly [Mediterranean fruit fly] in California.” He said, “But now I want to get back to this.” He asked me a few questions.

In ’96 I was in Vienna [Austria] teaching at the Space University in the summer, and he showed up there, and we talked for a while. He called me back at a later time and he says, “I have just one question for the article. How many civil engineers are working in NASA on lunar base plans?” This would have been ’96, thereabouts. Notice he asked me about “civil engineers.” A constructor company like Shimizu has mostly civil engineers, but the Johnson Space Center has very few, if any.

I said, “Somewhere between zero and one.” The story appeared on the front page of the *Wall Street Journal*. It was read by [NASA Administrator] Dan [Daniel S.] Goldin. He called [JSC Center Director] George [W. S.] Abbey and asked how many engineers were working on

lunar base at Johnson Space Center, and George Abbey said, “About 100.” But he asked about engineers, which was different from the question I was asked. So Dan Goldin testified before Congress and used the 100 engineers in his testimony for Congress. I knew nothing about the exchange between Goldin and Abbey.

John and Jan Connolly got married and held a wedding reception at Space Center Houston, so I attended with my wife. As we’re moving among the tables and chatting, one of the guys says, “Now Wendell, are you the guy that Goldin was talking about in Colorado?”

I said, “What? What do you mean?”

He said, “Well, he said a planetary scientist.”

I said, “I don’t know what you are talking about.”

I go and a little later somebody says, “Are you the planetary scientist that Dan Goldin was talking about in Colorado?”

I said, “Well, I don’t know.”

At the annual Space Foundation meeting in Colorado, the NASA Administrator traditionally gives a speech at a dinner that senior executives make sure to attend. Dan Goldin began by reading an article on the front page of the *Wall Street Journal* about work on lunar base concepts. When he got to the comment about zero and one, he put it down and said, “This makes me mad.” The people at the conference didn’t know who that article was about, but they knew that person was going to be on a meat hook within a week.

As the stories began to sink in, I emailed Lori [B.] Garver, who was at that time the Associate Administrator for Policy and Plans. “Lori, is Mr. Goldin mad at me?”



“Well, he’s not really mad at you. He’s mad because in Japan the private companies are investing in space things, and in the United States the private companies are just waiting for contracts.”

Fast-forward to 2004. I’m at the [Robert H.] Goddard Memorial Symposium. George Bush has made his speech in January, Goddard Memorial Symposium is in March. I gave an invited talk. At the end of my talk I try to find a seat in the huge room, but it’s literally full. I walk to the back of the room, and a guy there from the Air Force whom I’d just met not very long ago grabs me and says, “Wendell, great talk.”

“Thank you.”

“There are some people from NASA Headquarters here who were sitting near me, and they’re really making bad-mouth sounds about your talk.”

I wanted to stay and go to the reception and schmooze with people, but I had a plane to catch. So I went up to the cloakroom to get my coat, and who should be standing there but Lori Garver and [NASA Chief Historian] Roger [D.] Launius. They say, “Nice talk.”

I said, “Okay, yes, really wanted to stay, but I’ve got a plane to catch. I understand that some of the people in NASA Headquarters in the back row are saying negative things about my talk.”

Lori had by this time left NASA and she starts laughing. “Wendell, I can’t help you anymore. You know, I spent five years under Dan Goldin and of all the time I was there, I’ve only got one thing that I’m really proud of. That’s that I prevented him from firing you.” My heart jumped into my throat. So you never know.

I was invited to Maxwell[-Gunter] Air Force Base [Montgomery, Alabama] in the late ’80s or something by Joan [S.] Johnson-Freese when she was on the faculty there of the Air

War College. She invited me to be part of a three-person panel. I couldn't imagine why. The other two people were John [E.] Pike and Bob [Robert S.] Dickman, who at that time was the [Department of Defense] Space Architect. John Pike and Bob Dickman represented opposing views on defense policy, and they had a lively dialogue onstage while I mostly sat there. On another visit, I gave a talk about NASA and exploration. The base commander invited me to come have lunch at the commissary. He sat next to me at the table and said, "The first thing I want to do is thank you very much for your very candid comments about NASA. We really appreciate you coming in, and being straightforward and honest with us. However, I do have one question."

I said, "What's that?"

He says, "How do you keep your job?"

I said, "Anonymity. I am not in charge of anything, so whatever I say can't possibly have any significance." In retrospect, I believe it helped that I was a scientist. The senior managers in the human space program are engineers and disregard anything said by scientists, unless it is testimony before Congress.

The little asides are to give you a sense that my career was a bit off-center, and not really like other people. I did things that might be viewed as risky or inappropriate. I saw them as honest and just telling the truth. Perhaps I had a bit more of a platform than others, and so people heard it. But I managed to survive. I got my 50 years in, I still receive my pension.

JOHNSON: It's an interesting career, because you had that interest in lunar exploration all the way through.

MENDELL: Right, there is that theme.

JOHNSON: But what's interesting about the different times NASA has looked at that—it seems to be politically motivated. The first President Bush, George H. W. Bush, announced that SEI, and then of course the problems with the funding for that.

Then you said when George W. Bush announced it that you all got excited about it, but then again things happened. But in between you have a president [William J. "Bill" Clinton] that doesn't want to go to the Moon, then you have a president that does. Then after George W., then we have a president [Barack H. Obama II] that doesn't.

MENDELL: Quite frankly, when George W. Bush was elected, he only had two issues with NASA. One was that he wanted to stop the overruns. There was about a \$5 billion overrun on the [International] Space Station. The second thing was to make sure that NASA and the rest of the federal government performed work using the president's management system. You're not civil servants so you may not in your personnel reviews have had to write down how you followed each of President Bush's five management principles.

He sent [NASA Administrator] Sean O'Keefe, who was an accountant basically, to straighten out NASA. He stopped work on the Space Station, putting it on hold. While he [Bush] was governor of Texas he never once came to the Johnson Space Center. So you could hardly tell that that man was going to be positive in any sense about space. What happened was that Sean O'Keefe went native at NASA, much to the concern of the political people in Washington, DC.

But the second thing that happened—and this is my interpretation—is that NASA was never high on the radar of his chief advisers, Karl [C.] Rove and Karen [P.] Hughes. They felt that NASA had no political value for the president and his political career. When the [Space Shuttle *Columbia*] broke up over East Texas [STS-107 accident], the people in East Texas reacted very strongly to it. They were very, very moved. The political advisers noticed that, and it was not what they expected. They thought that this would be the chance to cancel NASA, but it went the other way.

That's when he decided, and his advisers decided, to have the Vision for Space Exploration, which was actually very well stated. However, the money never showed up.

JOHNSON: It just seems interesting that throughout NASA's history the Moon has been almost a political statement—in the '60s we had to go to the Moon because there was a political reason. Every time different presidents have come through, they've either wanted to go to the Moon or not wanted to go to the Moon.

MENDELL: Yes. It turns out that by the end of my career I was pretty well-schooled in policy even though I'd never "studied" it. At the Space University, I heard a lot of policy lectures and met many policy wonks. Of course in that part of my career when I was involved in advocacy, I had to be aware of policy and the role it plays and why it's important.

When Obama became president, the Bush strategy, let's call it, in our view was sound, but the advisers wanted to create an Obama space policy. They weren't interested in whether or not it was right for the nation. They had to change it so it wouldn't be Bush's initiative.

Obama himself had no real insight to that. The villain in all this is John [P.] Holdren, who was the head of OSTP [Office of Science and Technology Policy]. He was the one who didn't really care that much about NASA. Lori Garver became the messenger.

I've known Lori for 25 or 30 years and was very disappointed in what happened. She thought that this was her chance to be a political player if she was could successfully carry out the Administration's space policy. She began depending on the JPL business development office for technical advice. NASA in general was hostile to the policy change, but JPL was quick to support her and get funding for the technical studies she needed. I have no respect for the JPL business office, but the technical people are great.

She was here visiting the astronaut office, and they were pressuring her as to why there'd been this change and [Constellation Program] cancellation. In the end, she summed it up by saying, "We couldn't afford a lunar lander." There's some truth to that. When I was part of the Constellation Program I was part of the lunar architecture group and watched those deliberations and planning.

I would like to mention that in 2006 NASA held a Global Exploration Conference at the [Ronald] Reagan Building [and International Trade Center] in Washington, DC. It was one of the best things NASA has ever done in the area of outreach. It was Mike Griffin's idea to have a closed, invitation-only meeting, limited to 100 people, inviting everybody except NASA and the big aerospace companies.

That in itself created a huge uproar. They invited the international community and the commercial sector, now called NewSpace, and the scientific community plus a lot of people who were not technical, such as space lawyers or advocacy leaders. As the conference approached,

they were unable to hold the door, just like my experience in 1984. Big aerospace companies managed to get some people to come, and NASA had some people come.

The attendees were divided by name into seven different groups. I don't know who did that. Gib [Gilbert R.] Kirkham in International [and Interagency Relations] probably knows all that story. Each of the seven groups was placed in the hands of a facilitator. I was a facilitator. We were given a spreadsheet that had actually originally been designed by Dean [B.] Eppler and John [E.] Gruener down here at JSC for other reasons. You were supposed to list any submitted idea on the spreadsheet, like brainstorming. Each attendee could submit an idea on what he or she believed the future of space should include, with a brief description of what was meant. They had to give a timeframe at what time the idea would be appropriate to have happen. They were also asked for engineering estimates like mass, power, and volume.

Each facilitator had exactly the same set of instructions. The backgrounds of the participants in the groups differed by design, so that the lists should differ. We created a list in our group as did every other group. On the last half day each facilitator reported the results of his or her group. The submissions ranged widely, from "Build a more moral society" to "Buy my robot." There were no constraints.

Most of the attendees were people who felt that they'd been disenfranchised over time because that NASA was a monolith that did its own thing and only asked other people's opinion *pro forma*. For the first time they felt they were being asked at the very beginning, "What do you want?"

I always remember that my group had many internationals. I had some Ukrainians and space lawyers and Europeans. I knew a number of the people in my group. In fact, when the

facilitators were on telecoms trying to get a feel for what they were supposed to do, many of them couldn't get their head around what was being asked of us. I understood right away.

When the list of invitees was released a week before, the facilitators were taken aback because the list did not include the "usual suspects." Laurie [A.] Leshin commented, "I've never heard of these people."

I said, "I know about 95 percent of them."

She replied, "Well, if you know that many, then this must be biased."

I said, "Well, let's put it this way. I've been out meeting people that you guys never talk to." But in the end, all those attendees felt like they'd really contributed something and that they were being asked for the first time and were enthusiastic. The spreadsheet was a huge mess because the content was all over the place. It was carefully edited to make it useful with an eye to not remove any content.

Two things came out of it. One was a sub spreadsheet of science objectives that was used in the lunar planning. The second thing to come out of it was the recognition of six major themes. They became the six themes of the Vision for Space Exploration, and a number of us were asked to do PSAs [public service announcements] for each one of the themes. I was given a theme. It's 30 seconds or 25 seconds or whatever it is, and it was a script. I didn't like my script, so I rewrote it. If you've ever seen my PSA—the day before I was trying to come out of my building, and I stumbled on the front steps and I fell on the concrete and caught myself. I had bad scrapes on my hand, and went to the clinic. But when I did my PSA, I was gesturing and I said, "Because off we go." I go like that [demonstrates], and you can see the scrape on my hand.

That spreadsheet I consider to be one of the best things that NASA has done in terms of just asking the world, “What should people in space be doing?” It’s really underappreciated. It’s just another thing, people put on the shelf. But I really have very high regard for that effort because it was used in the lunar base architecture group. There was debates over whether we should be doing a bunch of science landings or a lunar base, and both of those are in that spreadsheet. Tony [Anthony R.] Lavoie read it carefully and was trying to really be honest about it when he and the lunar base architecture group chose to do the lunar base element.

JOHNSON: We’ve been going about two and a half hours. Is there anything that we haven’t covered?

MENDELL: See, I warned you.

JOHNSON: No, it’s all good information. I had questions throughout, but I think you’ve covered pretty much everything that I have. Is there anything that we haven’t talked about that you wanted to mention before we go?

MENDELL: I’ve given you this narrative, and I’ve covered the things that I was involved in, and then little hiccups that occurred here and there. Of course obviously at any place in my narrative there’s probably another level of detail, particularly in certain places. But it doesn’t do any good to go into all of that. I think just keep the basic narrative. But I’ve gone from A to Z.

JOHNSON: You have, you really have.



MENDELL: I can't think that I've left out anything.

JOHNSON: I appreciate you coming today.

[End of interview]