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EDITED ORAL HISTORY TRANSCRIPT

JOHN B. CHARLES
INTERVIEWED BY SANDRA JOHNSON
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JOHNSON: Today is July 21, 2015. This oral history session is being conducted with John Charles in Houston, Texas, as part of the Johnson Space Center Oral History Project. Interviewer is Sandra Johnson, assisted by Rebecca Wright.

Dr. Charles is the Associate Manager for International Science of NASA's Human Research Program and leads space life science planning for the joint US-Russian one-year mission on ISS [International Space Station] currently. Today I want to talk to you about your background, as far as when you were younger and you first went to school, and when you first started getting interested in working for NASA and how that came about.

CHARLES: I am one of those dyed-in-the-wool born space nerds. I was born in 1955, which was just before the space age began, and I have, as far as I know, always been interested in spaceflight, and NASA specifically. One of my earlier recollections in that context is, in the early 1960s, '61 and '62, the playground outside of Rockdale Elementary School in Rockdale, Texas, had a little culvert through the middle of it, and the culvert had a little cement walkway across it, and the cement walkway had some steel pipe handrails to keep elementary school kids from falling off the side. And I would lay on the cement walkway with my legs up over the handrails pretending I was John [H.] Glenn on the launch pad. I really did. And teachers would come out and say, "Did you fall? Are you okay?" And I'd say, "No, I'm just being John Glenn," and they'd walk away and say, "Oh, okay."

That was 1962, I was 7 years old, and by the time I was 10, I was committed to becoming an astronaut or, failing that, because I was always above the height limit—no matter what the height limit was at that time, I was always taller than the height limit—failing that, I was going to become involved in the space business. Early on, I was fascinated by physics, and I wanted to be a physicist. Then I started out as a physics major in college and realized that's got an awful lot of really hard math in it, and I was not good at the math, and I said, "Well, what else is there in the space business?" Well, obviously life sciences, and I was always interested in the life sciences as well, so that's where I was able to redirect my fairly hard science early years into the life sciences, and was able to get a degree in biophysics from Ohio State [University, Columbus], and then physiology and biophysics as my Ph.D. work from the University of Kentucky [Lexington].

So when I became interested in spaceflight, the question is, when did the clock start? I don't know when that was. It was forever. My wife says when I met her and we started talking about getting married in the late '90s, we talked about life after NASA and I said, "No, there is no such thing as life after NASA." Now in my—I just turned 60 this year—I understand that there is the possibility of life after NASA, but there will not be life after spaceflight. When it comes time for me to leave NASA, I will still be working in the space area, either consulting or explaining. That's why I'm so interested in the oral history project, and in history in general, because it's important to explain to the broader public what it is we have done and what we can do.

JOHNSON: And after that Ph.D., that's when you came to NASA, as a postdoc.

CHARLES: Came as a postdoctoral fellow in 1983, partly inspired and partly guided by Story Musgrave, a scientist-astronaut, who was a faculty member at the University of Kentucky. That was before his first Shuttle flight, but he had been hired as an astronaut in ’67. I think I met him in early ’77, when I’d actually been accepted to the University of Kentucky, and I made a trip from Ohio State down the road to University of Kentucky for essentially one day because he was lecturing the medical students, and I sat in on the lecture and then had a few minutes with him afterwards, and then drove back to Columbus.

Then my time at the University of Kentucky was in the department of physiology and biophysics, and my major advisor was Dan [Daniel R.] Richardson, and Dave [David C.] Randall was on my committee; they were both the cardiovascular guys. Richardson was more the peripheral, the blood vessels guy, and Randall was more the heart kind of guy. Cardiovascular discipline in a graduate school or a department always has the heart guy and the plumbing guy; there’s always two of them. And they were both on my committee.

My major work was next door from the medical center in a building that is now demolished that was called the Wenner-Gren Research Lab. That was a building run by the department of biomedical engineering, and it had a large animal centrifuge, a 25-foot-radius centrifuge that they were spinning dogs on, under Air Force contracts to understand the effects of G-loading [force of gravity] with different onset and offset rates, that would help inform protections for fighter pilots doing aerial combat maneuvers.

This seemed like an obvious place for me to go, and my advisors immediately understood that that was my major interest, was to work in the Wenner-Gren lab doing cardiovascular work on the centrifuge. So that was always understood to be my focus. When I got there, I was one of the first, at that time, I think they were saying interdisciplinary students. I was in the physiology department, but working with biomedical engineers. So Charlie [Charles F.] Knapp, who was the director of that research project, was sort of my de facto advisor. He was the guy whose research lab I was working in. I thought that was a good preparation for coming to NASA, because it gave me experience in something that was obviously space-y, spaceflight-ish, and that would be the centrifuge, the G-loading.

It also gave me experience in large projects, because it was a large piece of hardware with a large group of people often required for the feeding and maintenance of it. Also gave me insight into the surgical aspects, because we implanted probes inside of our dogs. I actually assisted in the surgeries, I never did any of the surgeries. We had an excellent surgical tech who was the second-best surgeon on campus, the only better surgeon being his boss, who was a thoracic surgeon, I think, at the medical center.

I had lots of experience, lots of exposure to every aspect of the investigation on the implementation side, from selecting the animals and implanting them, then I was responsible for their recovery post-op, and then their exercise and conditioning, it was my job as well. Also, planning the investigation from proposal all the way through final product, collecting data, punching the data, analyzing the data, and interpreting the data, and overseeing, or helping to oversee this fairly large research group gave me a little bit of experience in group management, which is important for an organization like NASA. So, it was pretty much an ideal setting for me to get the experience I thought would make me more attractive to NASA.

When I got my degree, I already had a postdoc lined up through the National Research Council, NRC. It was an NRC-NASA postdoc, because at that time NRC did postdocs with NASA. Came here and convinced Mike [Michael M.] Bungo to let me work in his lab, in the cardiovascular lab. So that was how I got my foot in the door.

Carolyn Leach Huntoon was instrumental in finding me. I was at one of the Aerospace Medical Association meetings, I think, in 1980, and it was in Houston, and I went to the registration desk, and I asked if a certain NASA scientist had registered. They said no, he didn't register, he's not coming. And he was the guy I was hoping to talk to about coming here to JSC. But in a little clump of people standing right next to that was Carolyn Huntoon, and she overheard me. She said, "Well, he's not coming, but maybe I can help you." And it turned out she was one of my protectors and facilitators here at JSC for the rest of my career, and I still stay in touch with her offline now, after she's retired. We both have property in Louisiana, so occasionally we talk about meeting up.

JOHNSON: That's pretty amazing, just from that one conversation.

CHARLES: It has always impressed me how many things happened in my life, and I assume everybody else's life, through accidents and spontaneous interactions, which also makes me think that if everything seems to happen that way, it's really not that uncommon. I don't want to sound fatalistic or predeterministic, but it sounds like it's going to happen. If you don't fall in love with that girl, well, there's probably another girl that's just about the same, and pretty nice, and you'd probably have a good life with her as well. If you don't make a comment that Carolyn overhears, maybe somebody else will overhear it. The threads all seem to come together in the same way, I think. It's an interesting perspective now that I've reached that point in my life where I have a chance to look back over my own history and see what has happened and what the odds might have been. It's kind of interesting to see how things always sort of come together.

JOHNSON: It is. We talked before a little bit, briefly, the last time, about those first years when you came here as a postdoc. Are there some things that come to mind during that period between ’83 and ’85, of things you were working on, or any incidents, or any anecdotes that you can think of that we haven’t talked about before?

CHARLES: Many. Many. The question is how many of them need to be immortalized in black and white. You have to have a project when you come as a postdoc, or at least at that time. And my project was to do a very thorough assessment of cardiovascular reflex control in spaceflight, in the presence of the changes that occur in weightlessness. The changes that occur in weightlessness are due to the headward redistribution of body fluids, I may have mentioned before, and we’re not on video, but I always do this hand motion showing the fluid distribution from the lower body to the upper part of the body. Then how those changes are reflected in changes in the way the cardiovascular system controls itself in that new environment.

I was especially interested in a fairly poorly understood set of reflexes that changed how the veins—the large flimsy collection vessels in the body, not the arteries, which are high-pressure and shunt the blood from the heart into the periphery, but the veins—which are sort of the rain spouts, the gutters that bring the fluid back up to the heart. Turns out they were not just the flimsy polyethylene-like bags that we always thought they were; they actually had muscles in them, smooth muscle, and that smooth muscle was enervated, and there was some reflex responsiveness in those, such that when they got distended they would reflexly contract, and it was under some neural control.

Coming from the University of Kentucky under the partial tutelage of Dave Randall, I was interested in neural control of the cardiovascular system. And Dan Richardson was also interested in peripheral circulatory control mediated by the nervous system. So it was an area that I was as good at as anything I was doing at that time. So I wrote a proposal, said I'd like to do lower body negative pressure on astronauts and measure forearm blood flow, which was a nice vascular area, and in fact it was not just the veins, but it was also the arteries. You could actually measure the reflex response of the arteries, controlled by the brain, in response to the shifting of fluid that occurs. And wouldn't it be cool to understand the stepwise de-adaptation of the cardiovascular system in spaceflight and weightlessness by reimposing this pretend gravity load of lower body negative pressure and watching arterial response.

I'm putting my hand around my forearm, because that's where we used to put the strain gauge, a mercury in silastic tube resistance gauge that would tell us how big the arm was or how small the arm was. We'd also put a cuff above that, and by inflating the cuff to a pressure just below blood pressure, you could get blood flow into the arm but no blood flow out of the arm. Then by watching the arm swell with the strain gauge, you could see how much flow was coming into the arm, into the vascular bed, and that would tell you what the blood flow is through those muscles, and that was an important number. You get blood flow through these muscles, and these muscles, and these muscles, and that's where the blood goes when it comes out of the heart. It's interesting to see how much is going through here, and then how the reflex changes over time are manifesting themselves.

I had this idea, and I used to draw cartoons of it, of a dude in an LBNP [lower body negative pressure] device with a strain gauge around his arm and the cuff on his forearm. That was my goal. I showed up at NASA as a postdoc with Mike Bungo. Mike Bungo had joined

NASA, I think in 1980, three years before I did, and he was a big-time internal medicine specialist, especially in cardiovascular problems. They had brought him down here because they were trying to rejuvenate the cardiovascular function. After Skylab, a lot of the expertise sort of drifted away, because things didn't seem to be happening for a few years. So, the cardiovascular lab, which was fairly dynamic, really dynamic in the Skylab era, got dissolved, and the floor space got apportioned out to other laboratories.

They brought Bungo back to rejuvenate the cardiovascular function, and I seemed to show up at the right time to be part of this growing cardiovascular lab. Then the funny thing was, Bungo was specializing in echocardiography, which was brand-new at that time. Portable, commercially available ultrasound devices for imaging the heart were fairly new in the early '80s. Bungo arrived and bought an echo, and I arrived, and we had, as I recall, two nurses and the cardiovascular lab was wherever the echo was.

If we were doing preflight measurements on an astronaut, sometimes we used the conference room; sometimes we had them lie on the conference table for the recumbent part and then stand up and lean against the wall. So we pioneered the development of what we called the orthostatic stand test, just because there was no hardware required. You could have somebody recumbent on a couch or recumbent on a cot or recumbent on the floor or on a conference table and make some resting measurements, then ask them to stand up and lean against the wall in a certain way and get standing measurements, and that would be our standardized test of astronauts or bed-rest subjects or anybody. Resting and then standing was the difference, and that tells you how the cardiovascular system responds to a standard G-load, a gravity load of 1-G, because gravity's the same all around the Earth, so you test astronauts before they fly and then you test them after they fly, and you can see by the difference how their cardiovascular system has decided to modify its behavior, its function, in spaceflight.

That was a standard clinical test, heart rate and blood pressure; blood pressure by a cuff, heart rate by ECG [electrocardiogram]. Bungo wanted to add his echocardiography, because heart rate and blood pressure just tell you that, yes, something has changed, and with the echocardiography you can say, "And here's what it is, the heart has changed its function in this way and that way."

I came along being more interested in the periphery than the central circulation and said, "You know, if you just do blood pressures during the ultrasound measurements, you get blood pressure, and by looking at the heart you can calculate how much blood the heart is pumping out each time, not just whether the walls are thicker and whether the walls are moving correctly, but also how much stroke volume we're getting." By multiplying that stroke volume by the heart rate you can get cardiac output, and by the ratio of cardiac output to blood pressure you can get peripheral resistance, the vascular resistance of all the blood vessels in the body. So Bungo was a cardiac guy and I was a peripheral guy, just like any good department of cardiovascular physiology.

That was the substantial effort for the first several years, from '83 to '85, was getting the ultrasound device—he had identified the ultrasound device, we started doing pre- and post-flight data collection, and then also Bungo was interested in flying it on the Space Shuttle and getting in-flight data, and that's where a biomedical engineer named David [A.] Wolf showed up. Dave Wolf was recruited about the same time I was, in sort of the run-up to becoming an astronaut. He was a biomedical engineer, first with Wyle, which at that time was called Tech Inc., and then later as a civil servant. He was the engineer on the flight echo [echocardiogram] project; that was our project.

So, for a while Bungo and Wolf and Charles were the cardiovascular guys at the JSC cardiovascular lab, as we developed this echocardiography capability. That echo flew in 1985, [M.] Rhea Seddon flew it the first time on [STS-]51D, and it flew again on STS-32, and it flew on SLS-1 [Spacelab Life Sciences, STS-40] and -2 [STS-58] as the backup device for the large echocardiograph. In fact it was used because the large echocardiograph, I think, failed on SLS-2 and Dave Wolf was able to use his brainchild in flight as the mission specialist on SLS-2 to do the measurements that were needed for that mission. I'm not sure where it is now; I hope it's in the Smithsonian [Institution] someplace.

That was the early project, '85 or so, when we got several flights done. And in the mid-'80s, after the first few Shuttle flights with a surprising incidence of motion sickness on some of those early flights that actually led to changes in mission plans, the Space and Life Sciences Directorate created the Space Biomedical Research Institute. That was at the instigation of General [James A.] Abrahamson, who at that time was the AA [Associate Administrator] for Space Flight at [NASA] Headquarters [Washington, DC].

The goal was to solve the motion sickness problems, because they had lots of hard work planned on these very short Shuttle flights, and you couldn't have half the crew being laid up for motion sickness if there was important work to be done. They created the Space Biomedical Research Institute, which focused all of the vestibular and neurosensory work into an institute, which started out to be separate from the Medical Sciences Division, but became the same thing after a few months as being a separate entity, meaning that we had two parallel entities within the same larger organization focused on medical research for spaceflight.

That quickly became untenable, and so the Space Biomedical Research Institute got moved back inside the Medical Sciences Division as a branch, and it was SD-5, that was the branch mail code. Eventually, within a few months, Bungo was named Branch Chief for that institute, and it just became a regular branch again. The point is, we had a lot of attention on the medical aspects of Space Shuttle flights early in the Space Shuttle era because of concerns that astronauts were not going to be able to function effectively on these—especially the DoD [Department of Defense] short-turnaround, high-workload missions.

So Bungo went off to be the Branch Chief, I inherited the cardiovascular lab from him, and for the next eight or nine years, that was my domain. We did pre- and post-flight testing of astronauts with the stand test. We also developed what we call the cardiovascular lab in a pouch, which was a blood pressure device which could record blood pressure and electrocardiogram in a tape recorder that could record the data, and a set of accelerometers that could sense G-load and posture. And when [Space Shuttle] *Challenger* [STS-51L] happened and after the *Challenger* accident, people started wearing spacesuits, my first concern was how am I going to get my data onto this recorder from people wearing a spacesuit, if my recorder is outside the spacesuit? And they won't let me put it inside the spacesuit, because the spacesuit's a pure oxygen environment; who wants to have a spark source inside of a pure oxygen environment?

Several of us investigators made enough noise that NASA actually modified the suit with something that we called the "hole in the suit." It was a little fitting on the right thigh that was inside of a pocket that had a watertight seal, a watertight removable plug in it, and you could pull that plug out, modify that plug, and run your hardware, electronics, and pneumatics through that. You could have body-worn instrumentation inside the suit on the astronaut, and then bring the signals out and send them to a recorder in a pocket on the outside of the suit.

In the post-*Challenger* era, that enabled a lot of our research, including a DSO, a detailed supplementary objective, that I was responsible for that was looking at the cardiovascular responses to the first episode of orthostasis, of standing upright after spaceflight. Astronauts landed in chairs upright, just like we do on airplanes now, and Sonny [Manley L.] Carter had made the point earlier on that the hardest thing he had to do on his first Shuttle mission was to stand up after landing, because after even three or four days of weightlessness, you become accustomed to being weightless, and standing up is hard work again.

That was important because everything in the Shuttle Program had to be operationally oriented, and we decided that, as far as the cardiovascular and neuromuscular and neurosensory concerns were focused, it was an issue of post-flight emergency egress. When the Shuttle lands, it may land at KSC [NASA Kennedy Space Center, Florida] with helpful ground staff just waiting to get you out, or it may land in a prepared strip someplace in Africa or Asia or in the Pacific, in which case you may not have people standing by that know how to get you out. If there's a problem that made you land at a different landing site than you expected, you've probably got problems with the vehicle, and if there's problems with the vehicle, you probably want to get away from the vehicle as quickly as possible. How quickly, then, could deconditioned astronauts be expected to unbuckle themselves, stand up for the first time after three days or two weeks of weightlessness, ambulate to the side hatch, climb out the side hatch, and run upwind 200 yards from a potentially burning, exploding Space Shuttle?

That became the focus of our research. All the work in our Biomedical Research Branch was focused on emergency egress, and that gave us permission to study just about everything we wanted to study anyhow, because it all focused down to what happens, what is the condition of the astronaut at landing, and how did that astronaut get into that condition from whatever the baseline state was before flight. So we did a lot of work, like I say, on this reentry monitoring using the instrumentation that we brought out through this hole in the suit to measure the cardiovascular changes immediately after landing, as well as the responses to the environment during reentry and landing.

We also did work on fluid loading countermeasures; it was a continuation of the work that Mike Bungo and Phil [Philip] Johnson had done early in the Shuttle era. That was to replenish some of the lost fluid volume with just a simple expedient of salt tablets and water, to show that we could restore some of the cardiovascular function immediately after landing by just fluid loading before reentry. I also was able to do some lower body negative pressure work in space.

It turned out that Phil Johnson had proposed a test of the lower body negative pressure countermeasure, that is LBNP, lower body negative pressure, plus fluid loading, as a demonstration of a countermeasure that would restore astronauts' cardiovascular function to some degree immediately post-flight. The fluid loading countermeasure we did was the fluid loading part of that without the LBNP part, because the LBNP part was too cumbersome to do on routine missions, and we could get an early start with a fluid load. So, we did the fluid load starting on STS-4, I think, was the first flight. It ran for another year or so as an experimental project, and then became operational.

The second part of that was the addition of the lower body negative pressure, and the idea there is to restore the fluid volume distribution in the body using this lower body negative pressure. And I haven't described LBNP, I think, yet. LBNP is a technique whereby one forms an airtight seal at about the top of the hipbone and decompresses by only about 1 PSI, 1 pound per square inch, in an enclosed chamber that encloses the lower body. Now, 1 PSI—if we have sea-level atmosphere, we have 14.7 PSI, so you're just taking off one-fifteenth of the atmospheric pressure.

People say you pull a vacuum in this chamber. No, you don't come anywhere near a vacuum, you just pull a little bit of delta pressure, and that's enough for the pressure around the upper body to squeeze the fluid into the lower body, as if the body was standing upright at 1-G. So by having 1 pound per square inch pressure difference, we can have a fluid redistribution that approximates that in a person who goes from recumbent to standing upright. So again, the 1-G stand test. We'd been doing the 1-G stand test for operational purposes, and now we had a chance to do it actually in zero-G to assess the progressive effect.

Phil Johnson's investigation was to demonstrate the usefulness of this countermeasure by doing it in flight; that is, the fluid loading during lower body negative pressure to try and restore the fluid volume, and doing it in flight, and then potentially delivering an operational countermeasure to the Shuttle Program for use on the Shuttle or on the Space Station, if people wanted to use it. I think he died in 1987, and I inherited that investigation. I became the PI [Principal Investigator] for that, and that was my major flight investigation for the remainder of my scientific career.

We were able to fly at LBNP with that countermeasure on several Shuttle missions, starting with STS-32 in 1990, and going all the way up through STS-73, USML-2 [U.S. Microgravity Laboratory], the last one that flew it. It was actually flown only as ballast on that flight, because by that time Headquarters had rethought an approach, and even though we got the hardware on that mission—the last of, I think, a dozen missions—had the hardware on that mission, had the crew trained, had baseline data collection, then our friends at Headquarters decided we weren't going to do that investigation on that flight. I was sort of uninvited from that flight, and the hardware flew as ballast, like I like to say; it was in a locker, but nobody took it out. That was one of the last flights of the lower body negative pressure experiment.

The last flight we actually did it on was IML-2 [International Microgravity Laboratory 2], that was Rick [Richard J.] Hieb and Chiaki Mukai. That was ironic, because Chiaki Mukai had worked in the cardiovascular lab. After she was selected as a Japanese astronaut, but before they started flying Japanese astronauts, she was in my laboratory, and so she was involved in testing and using LBNP, and I thought it was a very nice little capability for her to be using, being the test subject for LBNP on the STS-65, which was IML-2, the second of the International Microgravity Laboratory missions. And then, like I say, we had one more flight after that that it actually didn't get turned on during.

At that time I was doing cardiovascular studies, understanding the effects of spaceflight and the spaceflight environment on the cardiovascular system. We were also gearing up for the [International Space Station] Phase One Program [Shuttle-Mir] that I think I discussed with you previously, that is the Norm [Norman E.] Thagard flight on the Mir [space] station. And as these always happen, the decision was made to fly for medical purposes, without actually spelling out what those medical purposes were. The agreement was made at high level to fly an American astronaut on the Mir station to acquire data, and then once that agreement was signed, the managers turned to the rest of us and said, "Great. What data are we going to collect on this important life sciences mission?"

All we had were the investigations we were routinely doing on the Space Shuttle. Mine included reentry monitoring and lower body negative pressure, and both of those got added to the manifest for Norm Thagard's flight. The last time I actually was involved with any LBNP in the 1990s was Thagard's flight using the Chibis device, the Russian lower body negative pressure device called Chibis, on the Mir station, and then my own device in the Spacelab module of the Space Shuttle that docked with the Mir station to bring the crew members home. We had a week of post-Mir data collection while the Shuttle was docked, and we used the facilities of the American Spacelab module to acquire ground truth, validated data to compare with the Mir data, and also to compare with previous Space Shuttle data. Then Thagard and the other two, I think, wore the reentry monitoring hardware during reentry and landing of the Shuttle, so that was a dataset we acquired then as well.

At that point, I was promoted to management. The reason I was promoted to management was because I had written a proposal for continuation of my lower body negative pressure work, and it was not reviewed well by the peer review panel, so I did not pass peer review. I'm not saying it was a wonderful proposal, but I am saying that Headquarters, Life Sciences, had told the peer review panel to look for new things to be doing in space and not the same old thing. So, I came in with a proposal that talked about all the previous flights that we had done, all the data we acquired, and all the work I wanted to continue doing, and it was not very hard for the peer review panel to say, "This looks like the same old thing. Let's not select it, let's select something new." There's a whole other interview we can do sometime with the guidance that Headquarters or Life Sciences managers like me, now, give peer review panels about what to select and what not to select.

I will tell you that during the stand-up of the Human Research Program, we spent a lot of time deciding what kind of guidance we were going to give the peer review panels, which really depended on what kind of program we wanted to have. Did we want to build on existing infrastructures and make progress? Or were we looking for the next new thing, disparagingly you might say the next shiny object, the next new topic, that would perhaps give new insights but not really lead to any near-term products. There's a whole different interview we can schedule for the future.

JOHNSON: On that lower body negative pressure unit, just out of my curiosity, I know I've heard of it, other people have talked about it, but when they're in space and they put that on, how long does it take to actually pull that fluid back? And then how long does that effect last? Obviously, when they take it off, they're going to go back to the fluid shifts, so how effective was it, or by you not getting to go further with those studies, what do you think you haven't found out about it?

CHARLES: Well, actually, I don't feel too bad about it. I'm not happy with the way it ended, but I think the project itself demonstrated the value of lower body negative pressure in that context. And how long it lasts, I can tell you how long the effect of the treatment, that is the combined fluid loading during LBNP, lasted. It lasted about 24 hours. My goal was 48 hours, because, as you recall, the Shuttle was really good at not landing on time, so if you had the entire crew lined up to do this four-hour treatment—it's a four-hour treatment—seven people on board, only one LBNP means—it's easy to do the math—that's 28 hours of treatment sequentially.

If you put two LBNPs on board, it's still 14 hours of crew time taken up, so it's a huge, huge overhead, and you can't do it on the day of landing. You can't do it right up until reentry. You have to do it the day before landing, so right away there's a 24-hour dead space there that the effect starts going away. If it's not good for at least 24 hours, it's not good. Because the Shuttle, like I say, was real good at not landing on time, it had to be good for probably 48 hours, because we routinely waived off for at least 24 hours. It is inconceivable that any manager would say, "Now we're going to waive off for 24 hours, break the LBNP out again, and we'll run everybody else through it again in the last 24 hours before we really try to land again." So, it really needed to be good for 48 hours. We showed it's good for 24, and we showed it's not good for 48; the effect goes away.

Now, we got a lot of benefit from doing this, I mean a lot of insights into the cardiovascular system, we got a lot of data on early changes in the cardiovascular system, and we also showed that this is not a useful, an effective countermeasure for that problem, for the problem of orthostatic intolerance. It's not effective in a couple of regards. Number one, unless you land exactly on time, the effect is sort of dissipated. Number two, the problem's not that big. Orthostatic intolerance post-flight turns out to be more of a nuisance than a catastrophe. If you land in your Space Shuttle sitting upright, or if you land in your Apollo capsule lying on your back, or your Soyuz capsule lying on your back, you're already protected from orthostatic intolerance. The only time you have orthostatic intolerance problems is when you stand up.

The way that we made astronauts orthostatically intolerant is to ask them to stand quietly for 10 minutes and see how long it took them before they fainted. You will never stand quietly for 10 minutes unless some physiologist is asking you to stand quietly for 10 minutes. As soon as you stand up, you're moving; your legs are pumping, your muscles are pumping, your veins are being squeezed, you're moving around. That is also a good thing to do if you're feeling lightheaded. Some people, a small number of astronauts, fainted even despite that, but the vast majority only fainted when we made them stand still so they could faint.

Like I say, it was a problem that was a nuisance more than a catastrophe. The overhead was tremendous. The astronauts said, essentially, we'd rather have the disease than the cure. And I said of course, I understand.

The Russians, incidentally, do the same kind of treatment on their astronauts, even now on the Space Station, before reentry and landing. They do about a four-hour block of treatment in their Chibis device, which is a lower body negative pressure device. They also do fluid loading. They also do a little bit of exercise in it. But they don't do four hours the day before landing; they chop it up to about a month's worth of time that adds up to four hours, as sort of a gradual reconditioning in preparation for reentry and landing. The Americans do not do that on the Space Station. I poisoned the well for LBNP so much that the flight surgeons and the astronauts said not only no, but hell no, we're not going to do the LBNP thing that the Russians are doing.

In fact we have 15 years of a very nice controlled experiment of the Americans not doing it and the Russians doing it, and there's not that much difference, as far as I know. I haven't really looked at the numbers, but my qualitative informal sense is that the treatment they're doing is really not making that big a difference.

JOHNSON: What about for long duration, if you're going to be up there like they are now, six months or a year? Is there any benefit of doing it periodically while you're there? Or is having that fluid shift for six months at a time or a year at a time—I know that's part of what you're studying, everyone's trying to figure out how is that going to affect them, and we talked about some of those risks and things before for long duration. Is that LBNP useful, or the Russian version of it useful for just moving things back where they're supposed to be for a while on the long duration?

CHARLES: My guess is the answer to that is no, for the reason you say. As soon as you turn it off, the fluid goes back where it was. So unless you're wearing the LBNP device, and the Russian device is like a pair of pants, like in the Wallace and Gromit movie, "The Wrong Trousers," it looks just like that. They built them originally in the 1970s to be worn continuously, and they built them with legs so you could walk on the treadmill while you were wearing them. The idea was to restore cardiovascular function, exercise, and fluid distribution. Of course they are so cumbersome that you can't walk on a treadmill, but they do do squats and knee bends while they're wearing them.

But I think the important answer is that the effect is fairly transient. As soon as you turn the LBNP off, the fluid goes back to where it was before. Even if you do it for an hour a day, every single day, that's 23 hours a day that you're not doing it. If you only do it at the end of the mission, well, that's six months that you didn't do it, and then you do it for four hours at the end of the mission. That's really probably not going to be enough to make a structural change in the cardiovascular system.

I think I told you last time, we are using the Chibis device, the Russian lower body negative pressure device, to acutely, briefly reverse the fluid shift and make measurements of the eyeballs, the ocular changes, because the headward fluid shift, which is every day, all day, 24 hours a day for six months, may be the cause, or at least implicated, in this ocular manifestation that we see, these ocular changes that occur in astronauts. Everybody assumes it's fluid shift. Here's this technique for reversing the fluid shift. Wouldn't it be a good idea to see if the fluid shift really is implicated in this change? So, we're acquiring data on Scott [J.] Kelly and Mikhail Korniyenko on this [one-year] mission, right now, to test that hypothesis. The first data take was in the first week in June. I haven't seen the data yet, but I'm hopeful we'll get an answer from that. It may be that LBNP or the Chibis device, or some variant of lower body negative pressure, at least demonstrates whether the fluid shift is implicated in this clinical syndrome or is not implicated in it.

Now, there are other ways to shift fluids, though, and the obvious one is artificial gravity. Build a rotating spaceship, spin it, and that way the acceleration, the pseudo-gravity, causes the fluid to go back in the lower part of the body and may reverse or prevent these changes from occurring. All you've got to do is build a large centrifuge on a spacecraft, that's all. It turns out that's really hard to do, but it's not as hard as we've been told it is to do. It may be that, in fact, the Human Research Program right now has an effort to understand the actual implications of building a rotating spacecraft or a large centrifuge on future spacecraft, so we can actually decide whether it's a good idea to have artificial gravity for not just the fluid shift and the ocular manifestations, but for muscular conditioning and for activities of daily living, and all the things that are better in gravity than in the absence of gravity. So, that's a possibility.

I also fantasize about LBNP as a transition to artificial gravity. If we could build LBNP devices that included exercise devices inside of them, we could get some of the benefits of occasional lower body negative pressure during exercise. I also think, it turns out that LBNP is a better restraint system, just functionally as a restraint system, than putting shoulder harnesses on somebody and cinching them down onto a treadmill. When you cinch somebody down with bungee cords and a football player shoulder pad-like device, you're putting all of the force on their shoulders. When you stand up, the force that's holding you on the ground is distributed over your entire body, not on two square inches on top of your shoulders, or if you put a hip harness on and you distribute the weight between the two shoulders and the two hips. Either way, astronauts have a tough time loading themselves at more than 70 percent of their body weight onto the treadmill.

If we run on the ground at 1-G, but you're running on a treadmill in weightlessness at 0.7 G, and you're not doing anything else under any kind of G in spaceflight, you can't expect the treadmill to be an effective treatment, as effective as whatever effect is you get on the ground. But with something like a lower body negative pressure device, you put a reasonably tight waist seal on, and that means that the person is being loaded onto the treadmill by the entire upper body surface area. That force is not focused shoulders and hips, it's distributed over the entire surface area, and you can actually get over 1-G of loading onto a treadmill. It's been shown in laboratory studies. Alan [R.] Hargens out in California has done that work under a NASA grant, and we know that's possible.

It's very complex, it's not a matter of floating into the module, putting the bungees on, and starting running on the treadmill. You've got to put on this LBNP waist seal, make sure it seals around the exercise device, turn the pump on so you can decompress that chamber by one pound per square inch. It's a very complex, tedious piece of equipment, but it's a whole lot more simple than building a rotating spacecraft. But, it's a whole lot more complex than not building a rotating spacecraft and just using an exercise device with a bungee cord.

We are in the midst of having that debate. How much do we need artificial gravity? Do we need it enough to build a rotating spacecraft? Do we need it not at all, because the treadmill and the other devices we have are adequate? Or, do we need it a little bit, and is that little bit enough to justify this piece of hardware that's more complex than a regular treadmill but less complex than a rotating spacecraft? We're in the midst of that debate; there's no answer yet. So, stay tuned. That'll be my fifth or sixth interview.

JOHNSON: I was reading, too, about someone who was working on a gravity chair that rotated.

CHARLES: Same idea, I think. It's a short-radius centrifuge that's inside of a module, and you put somebody in it on a bicycle or on a chair or something and spin them, perhaps at 2-Gs, perhaps for an hour a day. And there are those that say 2-Gs for an hour a day is like 1-G for 24 hours a day, or something.

I should also say, for the record, that there are no data one way or the other on that point. There's no answer right now to the question of how much G is enough. When people say half a G is half as good as 1-G, the answer may be no, or it may be it's better than 1-G. Nobody knows, because we have data at zero-G and we have data at 1-G, but we don't have any data in between. As soon as I say zero-G and-1-G, a scientist is going to say, "Yeah, and what's the curve like that connects those two points?" And the answer is, it's whatever you want it to be, because right now there are no data that tell you what that curve looks like.

So it may be that 0.1-G, through some miracle of nature, is as effective at whatever you're looking for as 1-G, and all you've got to do is build a short centrifuge and generate one-tenth of a G. Or it may be that Mother Nature is perverse and says you have to be at 0.9-G before you have the effect of 1-G. Or it may be that 0.5-G really is half as good as 1-G. Nobody knows. It can be any curve you want it to be, because right now nobody knows.

That's one of the purposes of the work that we're talking about doing, and that others are actually doing on the Space Station right now, with short centrifuges that fit inside of a rack and that can accommodate cells or plants or mice, so we can start getting some data, any data on fractional G levels to understand the fractional benefits of gravity, of an acceleration like gravity, on the changes that occur in spaceflight. So, stay tuned. Nobody knows. Nobody, literally nobody knows yet. Anybody that says they know today is kidding, because nobody knows.

JOHNSON: But it's exciting.

CHARLES: It's very exciting. And don't forget the first module descoped from the Space Station was for artificial gravity, the Centrifuge Accommodation Module. That would've answered this question. It was descoped to save money and to save the cost of the Shuttle launch that would've launched that module. We could've had the answer by now, but we don't.

JOHNSON: Another problem of spaceflight is space adaptation sickness, where they have nausea. One of the things you worked on after Shuttle-Mir, since we've talked about Shuttle-Mir quite a bit, you became the chief scientist for STS-95 and John [H.] Glenn's flight. I know in those early flights the idea was that most of these astronauts didn't have that problem as much because they didn't move around as much. That was one of the things that, with John Glenn flying again, was going to be interesting to find out, if he had the same problem that a lot of other astronauts did—and some astronauts get it and some don't, in those studies. If you would, talk about that flight and when you first were assigned to it, and what you were looking for and what your duties were as far as being the mission scientist.

CHARLES: Yes. As I said earlier, I was inspired to get into the space business by John Glenn, or at least that's how I recall it, and that was the first orbital flight. Of course in the '60s, I was still in elementary school, and we would go to school in the morning and come back in the afternoon.

Usually, in those days, the flights took place during the time I was at school, so I really didn't get a chance to follow, and I don't recall us having enlightened school administrations that let us get out of class and listen on transistor radios to the launches. So, I didn't really know much about it, but I read about it as much as I could after the fact in magazines and newspapers. John Glenn obviously was an inspiration for people of my generation, and I was very excited to see him come to fly again.

I will tell you that my recollection is, when it was bandied about as a possibility I thought, well, that's a stunt, and clearly it is a stunt. He apparently was removed from the flight rotation for probably very good reasons after his Mercury flight; he was a national figure and nobody wanted to risk him again. Besides, their flights were already booked, there wasn't any empty seats for him to fill. Then he injured himself in '64 in a bathroom fall, when he hit his head on the sink in the bathroom and actually injured his organs of balance. He probably was not flight qualified after that, then he moved on to the other sphere of business and then politics.

Over the ensuing 30 years, the standards for spaceflight were relaxed enough that we could entertain flying people like him. I personally thought it was a stunt to fly him; I also personally thought it was a good thing to do. I thought the man deserves a victory lap, he's done a lot for us, he's done a lot to inspire us, and if we're flying schoolteachers on the Space Shuttle, why not fly him?

He thought so too. He thought it was a good idea to fly him, and apparently he lobbied every [NASA] Administrator from Jim [James E.] Webb on forward on flying him again. I remember in 1972, reading a quote in Newsweek or someplace that said he was looking for a way to fly on an Apollo mission, and of course that was not going to happen. There were no extra seats on the Apollo missions. Dan [Daniel S.] Goldin finally succumbed to him, for whatever reason, and Dan Goldin, I think, is on record as saying that John Glenn is the most persistent man he's ever met. He finally brought Goldin a proposal that justified flying, oh, let's say some septuagenarian astronaut on the Space Shuttle as a way of testing the hypothesis that the changes that occur in normal aging are the same as the changes that occur acutely in spaceflight, briefly in spaceflight, between launch and the first few days in weightlessness. The hypothesis being that somebody who's already successfully aged probably wouldn't go through those changes, because he will already have changed.

I think it's good that Glenn came up with a hypothesis that makes it sound scientific, but once again, as I said before, the decision was made. Dan Goldin actually was on record as saying that he had the idea peer reviewed, so it wasn't just his whim and it wasn't just Glenn's fantasy: there's actually scientific justification to it. But, I'm not sure what that means in Goldin's mind. I haven't tracked down what that peer review is, and that was my job for a while, to understand how these things are peer reviewed. I think he went to probably, appropriately, an august body of senior researchers and said, "What do you think about this?"

And they said, "Yeah, it probably has some value to it."

And Goldin said, "Great, peer review. Done."

As I said, once again, the political decision was made and then the managers turned around to the scientists and said, "This guy's going to fly on the Shuttle. What should we do with him?"

And of course we had our set of investigations that we were doing on routine Shuttle missions, and so the answer was, "Well, if we want to compare him to younger people that are flying on the Shuttle, we should do the same tests on him as we're doing to the younger people that are flying on the Shuttle." And that's a full manifest of investigations.

So, he did volunteer, enthusiastically, for everything that we threw at him, literally. John Glenn is, to my knowledge, the only spaceflight crew member who has ever complained bitterly when investigations were removed from his manifest instead of added to his manifest, because he wanted to make sure that there was no hint of this being a junket or a victory lap or a joy ride. He wanted a full schedule of things to do. I'm just not recalling now all the details, but I don't think he was any more motion sick than anybody else. I think the bottom line was that he was as "successful" in adapting to spaceflight as anybody else had been, which means there was nothing intrinsic in being 70-plus years old that would disqualify you from flying in space, if the purpose was to fly in space and adapt as well as anybody else and be able to do whatever else anybody else did in space.

In that sense it was a success, it was what we call an n=1 scientific study, which isn't widely regarded as rigorous, but you can write case reports of small sample studies, and this is an example of that. At that time, we were able to put together a set of investigations that demonstrated this, and he flew and successfully did them, and I think he got his victory lap. The story I heard after that is, Annie Glenn told him, "Never again. This is your second and final spaceflight." And I think he was happy with that.

JOHNSON: What was it like meeting him after you had fantasized about being John Glenn when you were six and seven years old?

CHARLES: He is probably one of the nicest men in America. Very, very nice, and I have a few specific—obviously I talked with him during training. I was not, at that time, doing the science, I was more facilitating the science, so I was a fly on the wall, I was sitting behind, and I was making introductions. But, he got to know me, and it was a real rush, a real thrill to be called to the phone because John Glenn wanted to talk to me. I recall at one point in the post-flight period, when he came back to the cardiovascular lab and was being debriefed on his results, he kept saying, "I've got to go, I've got to go. Anybody else need an autograph?" We did group pictures and autographs for everybody, and he'd say, "Oh, I've got someplace else to be. Anybody else need an autograph?"

He was just that connected, that in touch, and you know he'd been—at that time, that was the late '90s, 1998, and he flew first in '62, so there's 36 years in between there. Every single day, people wanted a piece of him. People wanted his autograph, his picture, they wanted to tell him their great investment idea, everything. I'm not sure what he was like in the early years, but by the time I finally met up with him, he was the most congenial, involved, interested guy I can imagine in that situation. I would have thrown my hands up decades before he did, but he was very engaged with making sure people got what they needed from him and had a good experience. That's my positive recollection of him.

JOHNSON: I think most people's recollections are very positive.

Was there a lot of media during that that you had to deal with? Because there was a lot of media about him flying again. As the mission scientist, since that was the whole purpose of him flying, did you have to deal with a lot of media?

CHARLES: I don't recall it as well as I recall the Mir stuff, and the [STS-]107 [*Columbia* accident] stuff, but yes, there was a lot of media attention at that time. I don't recall if we did daily news briefings or just periodic ones; the mission was a week long. There was a lot of run-up to the beginning, and then there was a post-flight session afterwards when we actually all convened at the National Institutes of Health in Bethesda, Maryland, and went through all the results publicly with all the scientists from NIH [National Institutes of Health] and John and Annie Glenn, just for fun.

Getting back to John and Annie, I was just engaged at that time to my current wife, and I said, "Would you like to go to this big event we're having in Bethesda and meet John Glenn and be part of this?"

And she said, "Yes, I would." So we flew in to DC and we went to Headquarters, and we rode the van with Dave [Dafydd R.] Williams, and I've forgotten now who the division chief was at that time, but we were driving from Headquarters up to Bethesda in a NASA van. Somebody was driving, and it was snowing, because this was November. This must have been November of '99. My wife and I were not yet married, so we were only fiancées at that time, and we walked into the auditorium, the program was already in progress, and I think John Glenn was on the stage, and there were some empty seats in the front row they had saved for the big shots and me and my wife. They were scattered in the first few rows, so I told my wife to sit in the front row and I would sit in the second row, and I sat next to Annie Glenn, who is the sweetest, most wonderful person.

I said, "Hi, Annie, you don't remember me, I'm John. I worked with your husband."

And she says, "Oh yes, I know."

And there came a break as they were changing something, sort of an intermission, and I said, "Annie, I'd like you to meet my fiancée, Kathy, who's sitting in the row in front of you."

Annie said, "Oh, oh, we should change seats. I'm sure you want to sit next to her."

Kathy said, "Annie, believe me, he'd rather sit next to you than he would to me." That was a nice little bonding moment. I have not spoken to Annie since then. That was 1999. But, I will always feel that sort of connection. I will always imagine that she feels that connection to me, although I'm sure in the last almost 20 years she's met other people and felt nice about them too.

JOHNSON: That's a nice memory to have. Let's talk about what you did after that. Were you assigned to the 107 flight, or were you working on that relatively quickly after that?

CHARLES: It was pretty close after that. I think there was some interest in doing—let's see, that was STS-95, and that was in the '98 time frame.

JOHNSON: But, 107 was scheduled for 2000, and there was a lot of delays with it.

CHARLES: I think right about then we started gearing up for 107. It was supposed to be a placeholder because the Space Station was not as fast coming online as we had hoped, and the research on the Station that was going to justify the Station's existence had not started appearing. There was the interest in doing some, we called them gap-filler Shuttle missions, and 107 was really the only one of the type. There were several planned. I think we decided that that was R-1, Research-1, which I retroactively specified John Glenn's mission as being R-0, and nobody else really liked that idea too much. But, there was actually talk of another post-*Columbia* kind of mission using the SPACELAB modules; that would've been R-2. For a while we were planning R-1 and R-2.

That started gearing up at about that same time, and because I didn't have anything else I was doing full time, I was the liaison between Life Sciences and the Mars planning people, but my primary work was as a mission scientist for the Shuttle missions. I got involved with the group that was starting to make those plans. There was a commitment to make the mission fly, and that commitment was based on the fact that we had a set of investigations that would have justified flying it. It was not just life sciences work, there was a lot of backlog of Space Station work in flame physics and crystal growth and actually Earth sciences related to packing of materials in weightlessness, as well as biological research. So, we had a full complement of investigations we could do on this mission.

I was asked, and I'm not sure how the decision was made, because I was obviously a life sciences guy, but I was asked to be the chief scientist for all the NASA investigations on 107.

JOHNSON: In a previous interview, you said you were Code U mission scientist.

CHARLES: I was the Code U mission scientist.

JOHNSON: What did you mean by that?

CHARLES: Code U was a mail code at Headquarters. That was the office of whatever it was called, [Office of Life and Microgravity Sciences and Applications]. Joan Vernikos had been the [director]. Code U was our mail code at Headquarters, and it's one of those wonderfully obscure mail codes. We all talk in mail codes; everybody on the inside knows what we mean. But, strictly speaking, I was the NASA mission scientist. I say that because up to 20 percent of the payload was commercial, was non-NASA. SPACEHAB, the company, now defunct, built the SPACEHAB modules to fit in the payload bay of the Space Shuttle. It actually originally designed them to be tourist modules, but after *Challenger*, it was obvious there was not going to be any tourists on the Space Shuttle. So, they came to NASA and offered them as an alternative to the Spacelab modules.

These SPACEHAB modules were available for logistics, and this was the first research mission using the Research Double Module, the RDM configuration. The back end was a standard SPACEHAB module used for general purposes like logistics, but the front end of the module was a two-module connection that was outfitted for science, in the sense of it had more outlets, more data connections, more fixtures for putting racks in place and things like that. This was the first of the research missions that was going to keep the scientific community engaged while we were preparing the Space Station.

The plan was then for me to be the code U mission scientist, the NASA mission scientist on SPACEHAB, and in exchange for delivery of this module, got the right to sell access to this module commercially, to the fullest extent they could, to make their money back, essentially. To try and kick start commercial activity in spaceflight, commercial research and activities.

JOHNSON: Was that the first time that it happened?

CHARLES: That was one of the first times. I don't want to say it's the first time, but it was the first time it was a big effort, as I recall. Obviously SPACEHAB was assigned a responsibility for managing the research mission because it was their module, and they got priority in populating, that is, manifesting that mission, because that was the deal, to make money at it. It was understood that NASA would also have access. SPACEHAB was able to sell lockers up to about 20 percent, as I recall, of the module's capacity. And I like to say that we had lower priority in crew time and in resources and things like that, because we were not this high-priority commercial stuff. We were the bottom 80 percent.

This mission was largely NASA-funded, because SPACEHAB, as I understood it, couldn't really fill the module. Maybe that was the plan all along, but we were really second-tier citizens on this mission. Anything we needed had to be weighed against the commercial priorities, and we sort of along the way became the de facto mission scientists, because SPACEHAB was really good at building hardware, they were moderately good at selling lockers. They had no inclination whatsoever for mission management, except for "here's the day the thing's going to launch, here's the things that are going to be on board," and whatever happens after that was really not important to them.

Of course that's the part that's the most important to us in the life sciences and the research business. I became the de facto mission scientist, we became the de facto mission management organization, and at that time it's very clear that NASA was contracting with SPACEHAB to manage the mission. We also stood up a shadow mission management organization to actually manage the mission, because SPACEHAB was up and out, and we were down and in.

After a while, the commercial guys, the commercial payloads, would look around and say, "Who's the science manager? Oh, John is." They would call me up and say, "Look, I'm having problems with crew time," or whatever. "Can you help me out?"

I'd say, "I don't really work with you, but yeah, let me see what I can do." It was a de facto kind of thing. I learned an awful lot about mission management watching LeLe Newkirk, Kathryn [E.] Newkirk, who was the mission manager for that, and also watching how this commercial entity manifested its missions and worked these missions.

My take-away lesson learned from that was that SPACEHAB was interested in filling lockers, but not really with facilitating the research on the mission. There could have been two very similar investigations in two adjacent lockers, and SPACEHAB did not really think it was important to tell them about each other. There might have been a synergy possible; maybe this one was measuring something that this one would be interested in, and vice versa. If they found out about it, SPACEHAB would not have been mad, but SPACEHAB was not configured to tell them about each other. They didn't have working group meetings. It was like, you're a passenger in an airplane, you put your luggage in that compartment; I'm a passenger, I put my luggage in that compartment, and that's all there is, we're putting things inside of lockers.

My job became really one of synergizing, of facilitating connections between corresponding payloads. If any were identified on the SPACEHAB side that were synergistic with any on the NASA side, I tried to make those connections. I tried to make connections between the NASA payloads that I was responsible for. I give great credit to all the NASA scientists and the NASA project managers and project engineers on the NASA side who didn't know me from Adam and didn't know why a life scientist was in charge of representing all of these physical sciences investigations.

I tried very hard to be conciliatory and supportive, and learn about their investigations. I also asked all the NASA organizations—who were used to managing their own missions, but in this case were assigned to be part of my big group—I asked them how they wanted to be interfaced with. I was used to a fairly traditional management structure where there's a "program," which would be like, in this case, the Life Sciences Program, represented by the mission management team, and then below that there are "projects," and there's a project manager, and then below that were the investigations, each with a Principal Investigator—and LeLe was the mission manager and I was the mission scientist.

We grouped the investigations in disciplines. In the life sciences we had, say, the metabolic discipline and the other disciplines that included investigations, that is, specific experiments. I turned around and went to the physical sciences guys and said, "Here's how we're organized, and I would like to do the same with you. How would you like to organize yourself?"

And they said, "Well, no, we're all projects. We're all standalone projects." In my mind, a standalone project included several investigations, but as far as they were concerned, this investigation was its own project, equal to our project of many investigations. There were several of those standalone projects, because [NASA] Glenn Research Center [Cleveland, Ohio], at that time it was still Lewis [Research Center], had a couple, flammability and the packing of the granular materials, and [NASA] Ames [Research Center, Moffett Field, California] had the biological investigations, and they all thought they were projects. I had to figure out ways to tread lightly on delicate sensibilities and people that were used to being at the project level, which I considered experiment level.

I kept asking them, "No, really, who's your project manager?"

They'd say, "Me."

I'd say, "No. There's got to be somebody above you who coordinates all of your investigations."

They said, "No, me." I learned a lot about project management and experiment management on spaceflights from that experience.

Also, I give them big credit for letting this life scientist guy represent them, and I made sure that when it came time for public affairs, I invited them to come and talk about their experiments, I did not talk about them. I did not represent them publicly. I introduced them and said, "And now so-and-so is going to tell you about the flammability study," instead of me being the guy taking all the credit, supposedly, or limping through very poor descriptions of what it is that we're doing in flight.

That was an example. I don't know if that was the origin, but that was an example of my current modus operandi, which is, as quickly as possible, delegate to somebody else. My management laughs at me, because as soon as I get a task, I start figuring out who should be doing that task instead of me. Barbara [J.] Corbin, who is one of my bosses right now, says, "I knew you were going to say that. As soon as we tell you to do something, you start saying, 'Well, I guess I can get so-and-so to do that.'" I want to make sure that the people that know what the topic is do the talking about it and do the planning for it, and I'm the gatekeeper and the facilitator, so I try to help.

JOHNSON: Well, you have to build your team so that everybody knows what they're doing.

CHARLES: I've never had any meaningful management training. I've had a little bit of management training. The last serious management training I had was in 1994, and that was called the MIP training, the managing the influence process, because NASA finds itself not only with line managers, but with people that are not line managers, people that are influence managers, like me. I've never done a performance review of anybody in my life. If I'm lucky, I won't before I retire. I've never had a line organization answering to me, so I've never been able to direct people, to say, "You, go do this thing." I've always had to rely on my charm and persuasion to convince people that it's really their idea to go do this and that thing.

I think maybe that's one of the reasons they asked me to do the mission science job for 107; also the fact that I was the only mission scientist they had at that time, so it was an obvious fit. But, it reinforced my idea that I don't need to be pretending I'm the expert when I'm not. I just need to be finding the experts and letting them do their thing, and try to keep them on the rails while the world changes around them, and help them get their products and trust them to tell me when they've got what they need.

JOHNSON: I read that there was a little bit of concern in Congress, before 107 came about, and the science community that science missions were being put on the back burner because of the technology in building the ISS, and science was like the stepchild, life science. Were you concerned during that period? The Glenn mission, there was a lot of science, but there was that gap before 107 finally flew. During that time period, did you see that happening? You said you were the only mission scientist available at that time. Was science getting left behind at that time?

CHARLES: Most definitely. The science budget was being robbed to pay for Space Station overruns, and I can't tell you all the episodes of that, I just know I was not high enough at that point in the organization to know the details. I know I was just always hearing that, oh well, our budget was robbed again to pay for this and that Space Station problem. I did track the projected launch dates, and if you wish, I will find that chart and send it to you, because it's a very nice saw tooth curve of calendar date across the bottom and the time until launch along the vertical axis. You make progress as the calendar moves along, you get closer and closer to launch date, and then it resets as the mission gets slipped further in the queue, and then it resets when the launch is delayed, and then it decreases as the launch date approaches, and then it resets again. I've got a very enlightening little chart that shows several years of how the 107 mission just kept getting pushed further and further back in the queue because it was not important.

There were higher-priority things to be doing, like building a Space Station, obviously a very high priority. And the other operational things the Shuttle was supposed to be doing, delivering payloads or doing other tasks in space. I get it. There's no doubt that this research mission was the lowest priority, and those poor astronauts that were assigned to it had to stand by and watch other people fly multiple times before they got their first flight. Even though they were assigned fairly early in the flow, they were shunted aside while other people got a chance to fly multiple times in the interim.

There was no doubt it was that way, and I don't think it was avoidable. I'm not sure how one would do it differently under the circumstances now, except perhaps not to bow to congressional pressure and not to put a research mission in there in the first place, if you realistically can't fly it. In fact it may be that we don't do anything different, because this helped keep the scientific community engaged even when there were not flights, because they were planning for a flight. So, that was some benefit.

JOHNSON: In the planning, when it was first proposed, I know part of it was Al [Albert A.] Gore's Triana satellite, which was interesting when I was reading about how he came up with that idea. But things shifted, and five years went by before a science flight, and it was going to be the last one for a long time because of the Station. Was there a lot of competition on getting different studies on that flight? And if so, you were talking about peer reviews, you said that sometimes the managers advised those peer reviews to get the science that they wanted get done.

CHARLES: I don't recall there being a lot of competition. I think we populated the mission fairly early in the flow, and then we had to keep those investigators happy while they weren't flying. They may have gotten opportunities on other missions to fly other experiments, or other aspects of other experiments, but I don't recall any ongoing competition. It seems like we lost Triana fairly early in the planning process. After that it sort of settled down. I mean, after that there was really no motivation to fly. At least with Triana on board, it was the vice president's pet project, and that would help to keep us in the flight queue, but after that went away, then it was the redheaded stepchild, nobody really had any particular interest in this one except for the scientists that were on board and the senators that those scientists liked to call up and complain about their poor treatment by NASA.

Triana just launched recently, and it's now on Station and sending pictures back. It's now called DSCOVR [Deep Space Climate Observatory]. I saw a picture from it the other day. I hope Al Gore feels vindicated.

JOHNSON: What I read is that he dreamed about it, or it came to him in a dream, or something?

CHARLES: I don't know if it was a dream or in the shower, but it was one of those moments. I think we all get inspiration at times like that.

JOHNSON: Yes, we do. As far as the peer reviews, did you have any involvement in picking those?

CHARLES: I don't recall any involvement in that peer review activity. Those investigations that came to us were peer reviewed by the sponsoring organizations, however they prefer to do it, and I treated them all as full-fledged investigations.

JOHNSON: One of the quotes I read in an article was that you said the 107 mission would be doing simulated Space Station science, and that was the purpose that you wanted to see happen as far as that flight. Can you talk about that? Maybe some of the things that they were doing as far as life science?

CHARLES: Well, life sciences and the other kinds of investigations—let me digress for a second and say a few words about dedicated missions. The Shuttle had several dedicated space life sciences missions, SLS-1 and SLS-2, and then Neurolab, and one of the lessons learned, not actually from the implementation of those missions but from the planning for those missions is that you don't want dedicated life sciences missions because the investigations step on each other. It may be that you have a mission that is dedicated to life sciences, let's say human research, but there's only so many hours of the day that you can ask the astronauts to be test subjects, through ethical guidelines, and there's only so many times you can stick a needle in somebody's vein or put them on a bicycle or something like that.

After a while, after you've done enough of those things, you're not measuring the effect of spaceflight on an individual, you're measuring the effect of spaceflight plus repetitive exercise bouts plus repetitive venipunctures plus repetitive sticking your head inside a rotating dome, all those kinds of things. Each investigator is hopefully aware of the context, that that is not the only thing the astronaut's doing.

The investigator community themselves realized early in the planning for SLS-1, which at that time was called Spacelab-4, that they were interfering with each other. They couldn't do all the things they wanted to do just because somebody else's final state, after the astronaut's finished doing that investigation, becomes the next guy's initial condition. That initial condition doesn't mimic what happens in regular spaceflight, it mimics what happens when your astronaut has just finished drinking a gallon of some sort of metabolic tracer fluid or something. Well, that's not baseline. How am I going to understand the effect of spaceflight after whatever the astronaut's done?

We learned, I think, early on that dedicated missions are probably not the most conducive to meaningful results if they focus specifically on astronauts. It's better to have a diversity of activities so that the astronauts do a cardiovascular study and then go off and do a flammability study and then go off and take pictures out the window, then come back and do a vestibular study. It sort of fills up the time with meaningful work without actually having all the investigations stepping on each other's toes.

We did SLS-1 and SLS-2, and we did Neurolab, and even a mission like STS-78, LMS, the Life and Microgravity Sciences mission. It was the follow-on to the SLS missions, and it was half microgravity sciences, which is the physics, and half life sciences. Even in that case, there was some discussion of the fact that there were so many exercise-based investigations that the astronauts were not really allowed to decondition as other astronauts had. They were exercising so frequently in flight for this and that investigation that all the investigators were measuring was the effect of exercise and not the effect of spaceflight as reflected in exercise.

One of the reasons that 107 was an attractive design was the fact that it had a diversity of investigations on board, not just life sciences, but the microgravity sciences and the other kinds of things as well, which really gave us a well-rounded payload and really gave us an example of the kind of work that would be done routinely on the Space Station. We're not going to have dedicated life sciences increments on the Space Station, we're going to have a full array of investigations that all have to be done according to some schedule on the Space Station. So, we were demonstrating how that might work.

I will say that I don't think the Space Station has reached the level of productivity and throughput that we had in 107, just because 107 was a short mission, a 14-day mission. It was short in that sense, and it had some focused objectives that needed to be done every day, or every few days in spaceflight, whereas corresponding investigations on the Space Station—and I'm thinking of this fluid shift study, which uses lower body negative pressure and another suite of hardware—is done three times on a one-year mission. If it had been a Space Shuttle mission, we'd do it three times in two weeks. And then the question might be, are you measuring the effect of spaceflight or are you measuring the effect of having done the same thing a few days ago?

I think in terms of the pace of the work and the workload that was assigned, we showed what the Space Station could do inside of a module. People would come inside this module and do their tasks and have a fairly tight schedule of activities that would be done in this work space, as I imagined at that time, and I think now, would be a model for the Space Station work. I think it is the model for the Space Station work, except I think that, again, the Space Station probably has a slightly more moderated pace because people are there for the long haul and not for the short term. There are rest periods and a diversity of activities, like public affairs activities and things like that, to break up the duty day, that we really didn't have that much of on 107. Does that address the question?

JOHNSON: Yes, I think it does. I was reading some of the reports that were coming out during the flight, and the investigators were predicting 100 percent success rate, as far as all their investigations. They were all very excited about the science that was coming out of it and they anticipated, of course, the bulk of it that they would get after landing. You mentioned that part of it was that the scheduling of this work during the flight made it successful, so that you weren't overtaxing the astronauts and breaking it up a little bit. Talk about, if you would, the relationship between the crew and the investigators. They had a lot of time because of all the delays to build those relationships and to work together. Do you think that had a lot to do with that success rate?

CHARLES: I think so. I think the astronauts were motivated to be successful because they did have enough time to bond with the investigators, and we certainly had enough face time with the investigations and the projects and the astronauts, so that they got to know what the purpose was, they got to know what the motivation was, what the end goal was for the set of investigations. I think we were lucky in that we had a good crew, a group of astronauts who cared about the research. They may not have preferred to have been on this research mission. In fact, one of the things that we did, I think I may have told you about this before, is that we essentially down-selected astronauts. I didn't get a chance to pick which astronauts would be on the mission, but I got a chance to pre-brief large groups of astronauts who might end up on the mission, and to tell them what we were about.

On previous flights, especially Spacelab-J [STS-47], back in the early ’90s, I heard stories that the astronauts were assigned to the mission, and as they were going to Huntsville [NASA Marshall Space Flight Center, Alabama] in the airplane for the first briefings, they got a book of information on the mission and what it was about. There were some very invasive investigations planned for that mission, including my lower body negative pressure study. We had many invasive, complex investigations planned for 107, and I petitioned the astronaut management repeatedly, please don’t give me astronauts who don’t like life sciences work, because that’s a major part of what we’re doing. We had other managers who would also make the point up and out as well to astronaut office management.

The purpose for saying this was that I was given the opportunity on one, or maybe two occasions, I think one occasion specifically, where I was able to pre-brief a cadre of potential 107 mission specialists about the investigations we were planning and say, “Look, if you don’t like venipunctures, this is not the mission for you. If you don’t like ultrasound, if you don’t like slime, if you don’t like dealing with animals, please find another mission. There’s lots of other missions you can fly, seriously. This will not jeopardize your career by not being on this mission, it might actually enhance your career.” So, we got a cadre of astronauts who were assigned, and apparently were assigned fully informed, is what I should say, that were interested in doing the work on the mission.

We had Mike [Michael P.] Anderson and Laurel [B.] Clark and Dave [David M.] Brown and K.C. [Kalpana] Chawla, and they were wonderful. Dave Brown was an MD test pilot; he could have any mission, and he seemed to like the one we were on. Laurel Clark I think was an obvious fit. Mike Anderson was there as the payload commander, he had experience in organizing this kind of stuff. And everybody loved K.C. It was a good crew. Rick [D.] Husband as the commander was very supportive, very conciliatory, understood the value of the mission. He was a test pilot, fighter pilot type, but he didn't strike me as being the hard-charging, gritted teeth into the wind kind of guy. He was the kind of guy that would take the task and do a good job at it, and make sure people had a good time while they were doing it.

I thought the cadre of astronauts that we ended up with were the right ones for the investigations. They did bond with the investigators, they did appreciate the investigations, they cared about the investigations, and they did what they could to make it work. I don't recall specifics, but there may actually have been a little bit of insight on their part, so when we seemed to be running out of time, they understood that certain things needed to be done whether time permitted or not, and they saw to it that things got done that needed to be done.

There was one or two slip-ups, I recall. One of the astronauts, I forget now what the details were, but the cells were not preserved correctly or something, and they felt really bad about that, because they thought they had done it correctly but they followed the wrong set of procedures or something. There was just those kind of inevitable little slip-ups. But, overall I think everybody was very happy with the crew and the crew's activities that we had on that mission.

JOHNSON: Following the accident and during your interview that we did for *Columbia* [July 15, 2003], you mentioned that about 30 percent of the science was going to be recovered or useful at that time, that's what everyone was thinking those few months right after the accident. Some of the investigators were getting a chance to look at the recovered items to see if there was anything recoverable. Since that time, did that percentage increase any, or was that an accurate estimate.

CHARLES: I have not revisited those numbers, and those are the numbers I recall. I can't imagine how it got any better after that. There was the occasional bit of recovered science. There was one investigator who was pretty sure he could have recovered his science if NASA had just let him have the hardware. He saw news photos of his hardware laying in a parking lot in east Texas and was saying, "Just let me have the hardware, and I'll pull the specimens out, and you can have the hardware back. I just want to get the specimens out and see what the effects of spaceflight were in this particular setting."

NASA's accident investigation mentality was, "No, nobody touches anything, because that little experiment may have been instrumental to the loss of the vehicle." Once a disaster happens, there is a formula that you have to follow, and modifications to that formula are extremely difficult, meaning impossible. Even though, in retrospect, and even from a different perspective, it was obvious that this or that item were not involved, this or that item are sequestered.

Don't forget, as I told you before what had happened, they came in and sequestered all of my notebooks on experiment progress in Mission Control. Now, rationally there is no purpose to sequestering the scientist notebooks about investigations that had nothing to do with thermal protection systems. But they don't know that. It's like CSI [Crime Scene Investigation], everything is suspected until it's cleared.

JOHNSON: The yellow tape is all the way around.

CHARLES: The yellow tape goes around, and everything inside that yellow tape is part of the investigation. We had flown a stack of our program patches, I had actually designed some patches for the Code U payloads on this, and apparently they had been packed as a stack, like potato chips in a can, they'd been shrink-wrapped together. They were showing me pictures of the patches that had been recovered post-flight. I said, "Great, can I get them?" And they said, "No, you can't get them. They're not yours anymore. They belong to the investigation." Obviously the patches were not implicated in the disaster, but they were part of the debris, and so that debris is wherever it is now.

JOHNSON: They kept everything. Do you know of any significant setbacks or anything in any of these investigations that happened because of the loss of their data?

CHARLES: No, I don't—I answered "no" quickly, but even on thinking about it, I cannot think of anything that was not recovered eventually, not repeated or worked around eventually.

JOHNSON: I know because of the relationship that these investigators had built, I imagine that the people that you worked with day in and day out here, there was a lot of effects on the science community and the people that had worked and the trainers for the science experiments. Did you try to make sure that your group and the people that you worked with regularly took advantage of everything that was available to them after that?

CHARLES: That's interesting you mention that. The first thing that happened here locally, on site, was the Employee Assistance Program immediately reached out to everybody and said, "We've got counseling available, feel free to talk to each other, and we all have emotions." I and others reached out to the investigator communities who were not part of JSC and said, "Does your organization, your university, have the same kind of assistance in case you need it? Because we all feel the loss." And as I recall, all of them said, "Yes, we've been already contacted by our university." It was reassuring in the sense that those at least that I followed up with had been offered that kind of emotional and psychological support and assistance.

We also had a follow-up Investigator Working Group [IWG] meeting. When you have these consolidated payloads, you have Investigator Working Group meetings where you plan how all the investigations are going to be dovetailed with each other. You do that over the period of several years before the flight, and you do it every six months or so, and people report progress on their experiments and problems they're having. The Shuttle Program would come and tell us what the newest restrictions were on the flight and which attitudes you're going to be in and all that kind of stuff.

We had a follow-up IWG that was, at least initially, dedicated to discussing, not reviewing results, because people didn't have results, but just giving everybody a chance to talk about what the mission meant and what the mission loss meant. I gave a little two-minute introduction, a little speech of mine, a reflection; I think I still have it on my hard drive someplace, and allowed everybody else to just stand up and say what they thought, what they were feeling about the mission.

We may have, I don't recall now the agenda, but we may have gotten to scientific results later in the day or the next day. The first several hours were strictly anybody can say anything they want to, if it makes them feel better. So, there was that kind of stuff, both formally and informally.

JOHNSON: It helps, as a community, to be able to share those feelings. Why don't we just stop now? Is there anything else you want to talk about 107? I know we skipped the whole in between, but we have that on the other interview, most of it.

CHARLES: I can't think of anything else 107-related, but I will be thinking back on this. If something else comes up, I'll tell you about it.

JOHNSON: Okay, that'd be great.

CHARLES: Because we'll have another opportunity.

JOHNSON: Yes we will. Thank you.

[End of interview]