The oral histories placed on this Website are from a few of the many people who worked together to meet the challenges of the Shuttle-Mir Program. The words that you will read are the transcripts from the audio-recorded, personal interviews conducted with each of these individuals.

In order to preserve the integrity of their audio record, these histories are presented with limited revisions and reflect the candid conversational style of the oral history format. Brackets or an ellipsis mark will indicate if the text has been annotated or edited to provide the reader a better understanding of the content.

Enjoy “hearing” these factual accountings from these people who were among those who were involved in the day-to-day activities of this historic partnership between the United States and Russia.

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Go to Oral History

JOHN B. CHARLES

August 28, 1998

Interviewers: Rebecca Wright, Paul Rollins,, Glen Swanson

Wright: Today is August 28, 1998. We're speaking to Dr. John Charles as part of the Shuttle-Mir Oral History Program. Rebecca Wright, Glen Swanson, and Paul Rollins.

Thanks again for stopping by and visiting with us today.

Charles: Glad to be here.

Wright: We'd like for you to start by telling us what were your roles and responsibilities in this program.

Charles: I started out as the cardiovascular discipline lead for the Shuttle-Mir Program that was Norm [Norman] Thagard's flight, the whole STS-71 Mir 18 experience, which meant I was the lead and the point of contact for the cardiovascular set of investigations that were part of the human life sciences investigations. I eventually became the deputy project scientist for that assisting Peggy Whitson. Then for the NASA-Mir Program, which is the second wave, the Shannon Lucid and following flights, I became the project scientist for the human life sciences investigations, all the disciplines of the human life sciences, including cardiovascular, and neurosensory and musculoskeletal, and all that kind of stuff.

After a couple of increments of that, I was asked to function as the mission scientist for the NASA-Mir Program, and I was the mission scientist specifically for NASA 4 and NASA 5, the Jerry Linenger and Mike Foale missions. As a mission scientist, I was responsible for not only the human life sciences, but all the other sciences, the microgravity, the biology, the space sciences. Everything else that was in the area of science, I was at least nominally responsible for.

Wright: Each one of those roles were extremely busy. Is that correct?

Charles: That is correct.

Wright: Were you able to build from that first experience to the last experience?

Charles: Yes, it was a good progression. I was able to move from my experiment to other experiments in my area of specialty, to other experiments in my discipline, which was human life sciences, and eventually then into the whole science thing. Luckily, in addition to being a researcher myself, I've always been interested in all of science. Before I settled in physiology, I dabbled in things like geology and physics and astronomy, and have an extraneous or personal interest in other areas of science. So I felt at least minimally qualified. At least I knew when to nod when somebody was talking. I knew when to look surprised if something was new, and stuff like that.

Wright: Tell us about the experiences with the first experience when you were supporting Thagard. It was all new for everybody. How did that all come together for you, and how were you able to help make that work?

Charles: We were called upon, on short notice, to provide a payload of life sciences investigations for Thagard's flight. We always seemed to be caught by surprise. They may spend a lot of time getting these programs going, but we always seemed to be caught by surprise at the last moment to generate a payload of investigations. That seems to be a recurring pattern throughout my career.

The payloads that were generated for Thagard's flight were, for the most part, continuations of payloads, life sciences investigations, I should say, that were managed by in-house investigators, and they had been developed for something that preceded that, which was called the EDOMP, the Extended Duration Orbiter Medical Project. The idea of that was to qualify Shuttle crew members for long-duration Shuttle flights, eighteen-day Shuttle flights, which had the requirement that they be able to egress individually, unassisted, after a long-duration Shuttle flight. So we had put together a set of investigations that we thought would address that question, but in actuality were, I think, good investigations to understand human physiology in space flight.

Because that was wrapping up at almost exactly the same time as the Mir 18 STS-71 experience was getting going, and because there was a call from headquarters to generate some experiments for this mission that we had agreed to do, that was the obvious set of experiments. So it was a relatively simple matter to transfer or translate those investigations from a long-duration Shuttle flight into a long-duration Mir mission. So that was a learning experience in terms of getting a set of payloads together on short notice for that mission.

There were the inevitable alterations in mind-set as you go from Shuttle to Mir, and from eighteen days to three months or longer, and a lot of surprises dealing with our new partners, the Russians. It took quite a while for us to understand the vagaries of the Russian program. In addition to my other interests, I've also always been interested in space flight history which, of course, includes a lot of Russian space flight history. So I wasn't unprepared for the surprises that came along, but I was unprepared for the magnitude of the surprises that came along.

Some of the magnitude of surprises, it was, again, a matter of mind-set. It took a long time for me to understand that what I was seeing was really the truth. I kept trying to interpret what I was seeing in light of what I thought I understood. We had, I think, all of us collectively believed that the Russian space program was a monolith, and that there was a decision-maker who then directed certain things to happen. I don't know why we believed that, because our own space program certainly is not that. But we had

assumed that the totalitarian Soviets were of the monolithic mind-set.

I think it's very clear now, in light of some space history publications, in fact, several things have come out in *Quest* and otherwise have shown us that not only is it not a monolith, but the Russian program now, like the Soviet program before, is composed of factions that compete vigorously with each other. In the life sciences arena, we experience that. Specifically the factions that compete with each other are Star City, the Russian Air Force-managed Cosmonaut Training Center, Energia, the owner of all the hardware and half the cosmonauts, and the IBMP, the Institute for Environmental Problems, which constitute the front line of liaison for medical research.

We had many, many difficulties trying to negotiate what I thought were fairly simple, straightforward investigations into this Russian group of competing interests. I guess the thing that I can always remember--and this may be something I strike out later, who knows--but the thing that I remember is that the Russians are very good at negotiating, I suspect because they've negotiated all their lives for every scrap of food they've eaten, they've probably had to negotiate, whereas we Americans always seem to present ourselves as fat, dumb, and happy. I know we took a cross-cultural training course which, as I recall, distilled down to essentially one or two lessons, and one of the lessons that I recall was that the Russians don't believe in compromise, they believe in winner take all. So when we sit down to negotiate with them, the Americans like to compromise and meet a happy medium, and as we do that, the Russians say, "Well, they gave that away. That must mean there's lots more to take." Whereas we can't understand why they're being so difficult to deal with. "We gave you half. Why can't you meet us halfway?" And the Russians don't seem to have any conception of meeting halfway. Either they win or they lose, and if you've given up, well, that means you lose, and they, by definition, win. So there was lots of learning along that lines.

Also the negotiations were complicated by the fact of the three parties--Energia, Star City, and IBMP--they seemed to make it a point to only bring two of those parties to any negotiation, or one party. You'd spend hours or days perhaps negotiating points that were, to the American point of view, to my point of view, trivial, and you'd finally come to an agreement with the parties, so the next day, one of those parties would be absent, and the third party would show up. That third party would claim no knowledge of the preceding, and no interest in the result, and even with exhortations of the other Russian, who was present for those negotiations. You'd start all over.

So I came not to look forward to the negotiation sessions, and, of course, every experiment, every detail of every experiment has to be negotiated in the life sciences, in the microgravity sciences, in the space sciences, and biology. Sometimes the negotiations are facilitated when you have a counterpart who was

strong and able to dominate the other parties, and also interested in what you want to get done.

The negotiations also seem to be facilitated when the counterpart, when the NASA person and the Russian counterpart, the strong Russian counterpart, had a pre-existing relationship, for example, in the area of bone mineral studies using X-ray devices, called DEXA devices. There'd been a longstanding relationship between the NASA individual and the Russian. In the area of, surprising enough, water monitoring, there had been a longstanding relationship. So those negotiations almost seemed to be foregone conclusions, but the areas where the Russians either didn't have a longstanding relationship or had, we believe, some vested interest in not being as open as we might wish, the negotiations could be painful and protracted. So that was a lesson I learned, I think, from the Russian negotiation process, and it was reinforced every time my responsibilities increased. I just saw that it was the same thing in a different area. It wasn't just a peculiarity of us and the guys we were dealing with. It seemed to be intrinsic to the systems, both the American system and the Russian system.

We seemed to set ourselves up, because I know [not] all of us in NASA are good negotiators. There may be certain people in certain specialties that are good negotiators, but I think even in terms of the contracts and the folks that oversee the technical details of the interaction, I believe that we got hoodwinked on several occasions by thinking that the words on the paper meant something that the Russians did not think they meant. I mean, they may have been very full and complete in English, but when they got read by Russians in Russian, they did not carry the same meaning. So we spent a lot of time trying to get the Russians to do what we think they originally agreed to do in the first place, and which they apparently very sincerely believed they had never agreed to in the first place.

That dealt with things like data sharing, which was our primary concern, making sure we got the participation of the cosmonauts in the medical studies, making sure that the medical studies the cosmonauts participated in were the ones that we designed, and not some variant, and making sure that we got the data back from those participations.

Then we also had some interesting experiences with differences in the approach to medical caretaking. On some occasions the Russians seemed to be rather cavalier about what their cosmonauts participated in, and in some they seemed to be extremely overly interested.

One instance I recall was in the cardiovascular testing that took place after the Mir 18 flight. One of the Russian crew members was doing a stand test, a test of cardiovascular function post-flight. He was having, apparently, some--I wasn't there--but I'm told he had some difficulties. He was starting to feel uncomfortable standing upright, perhaps leading to eventually to pre-syncope. That's usually reflected in high heart rate and dropping blood pressure. So the Russian flight surgeon, the man who I know and

admire and love and respect, the Russian flight surgeon began massaging his carotid arteries to help him make it through the test. Of course, that invalidates the test, because we're not interested in studying how a guy does when somebody's helping him. You try to understand what the physiology of that individual is doing, and not the physiology of that person plus another guy that's helping that physiology. And our investigator was dumbfounded. She couldn't say anything because it was the middle of a study, and she didn't know exactly what was going on in the first place. That pretty much invalidated that data collection session and all the data collection sessions on that person before, for the flight and for before the flight. And the Russians had no difficulty with that, because, "The guy was uncomfortable, and I'm here to make him feel better."

We had other instances where the Russian crew member was reluctant to do procedures that we thought were very benign. For example, one that's called magnetic resonance imaging, a typical, standard clinical, very expensive, but a standard clinical technique. There's MRI units all over, and they use a strong magnetic field to rearrange the molecules in the body, and then when the molecules go back to their normal state, they send off radio signals that can be detected that tell you how much they changed. From that they can build these really wonderful color pictures essentially of the inside of the body, non-invasively. It's as close to a "Star Trek" kind of technique as we're going to get in my lifetime.

One senior cosmonaut refused to get into it because it had radiation in it. And it didn't have. There's no radiation involved. But he said, "Yes, you call it magnetic resonance imaging now, but I know you used to call it nuclear magnetic resonance, and it has the word 'nuclear' in it, and I'm not going to do it." Of course, the "nuclear" is different, it means the nucleus of the atom, and not nuclear radiation, nuclear decay. And there was no amount of negotiation that could change his mind. So we lost a subject from that investigation, a fairly important investigation.

Through a set of instances not like the first one, but like the second one where cosmonauts would arbitrarily draw lines in the sand and say that certain things were possible, or certain things were not possible, or things were too risky, I think we came to understand that they were negotiating not with us but with each other, because there was a transfer of funds involved. Unlike the American astronauts who show up as directed to do the studies, and there's no hint of compensation for the study, the cosmonauts were expecting to be paid for their participation. And if they weren't, then their managers were expecting to be paid. And other people's managers were expecting to be paid, too. There was this 400 million dollars that supposedly had gotten transferred to the Russian program as part of the Phase One effort. That got transferred in chunks, and those chunks were anxiously awaited by the Russians, because they had mouths to feed and houses to build and cars to buy, and things like that.

So I don't understand all the details, but I have now come to accept the fact that those kind of things, those negotiation sessions, were mostly about money, and they may be looking me in the eye and shaking their finger at me and saying, "You do not do this," or, "You cannot do this," or, "That is not authorized," but they're really talking to the guys on the other side of them. They're saying, "We, Star City, think that we ought to be getting a bigger chunk of the money than Energia has got," or IBMP is saying, "Look, we're not getting any of the money at all. You guys have gotten all the money, and we're trying to pay our staffs and do the science that you guys are taking advantage of," but they're talking to the American across the table.

As the American, I would frequently take offense or be baffled or feel guilty, because I wasn't understanding what he was trying to tell me. So there's lots of that kind of thing going on, and I can still look back at historical records or anecdotes, or stories of negotiations and see, "Ah, well, see, that's when they were yelling at the Americans because they weren't getting the money that Energia promised them." So there's lots of that kind of stuff.

It's difficult for me to separate out what I learned in Phase One Mir 18 versus Phase One Mir 21, or the other flights, just because the characters changed and the topics changed, but the emphasis always seemed to be the same. Toward the end of our negotiations, toward the end of my involvement over a year ago now in the Mir Program, I used to go into these negotiation sessions not as a lead, but as a listener for the science, knowing in general what the pattern of the response of the process was going to be. We were going to make a request which we thought was eminently realistic, the Russians are going to say, "Oh, it's very interesting, we have to discuss," and they're going to come back with a list of demands that seem to be irrelevant at the very best.

Another very interesting phenomenon that I did not understand until somebody explained it to me later is that this whole idea of a zero-sum gain, if the Russians--I hate to generalize. I hate to besmirch a lot of honest, noble Russians, but my understanding is if they have to give up on something, that means that they expect something else. So there was a process, and I think the thing was that dealing with the data from the sensors on the Priroda module, the recently launched Priroda module, the Earth resources module, we had agreed with the Russians that a lot of the data to be coming from those sensors were going to be shared with the Americans who were co-investigators or principal investigators for those investigations requiring certain scans of certain parts of the Earth, and certain wavelengths to get the information the investigators required.

The Russians took a long, long time to give us that data. They kept saying, "All the things are not calibrated," or, "They're not functioning correctly, and we don't have plan for this. [Or,] we're not sure that

really was part of the negotiation." They eventually, after months of repeated meetings and negotiations, they eventually said, "Okay, we're going to give you that." Almost the next thing they said was, "But we're not going to have somebody participate in some medical study that we've agreed to," essentially. So they gave us something, but then they took something back, because the books had to balance. There's "If we give you something, well, we have to get something back, and here's something we're not too crazy about, we're doing it as a favor to you, so we're not going to do it anymore." We were sort of stunned. We were nudging each other, saying, "See, zero-sum gain. They gave us something, they took something away." That really sort of typified--that sort of set in my opinion of the process of negotiating with the Russians.

I've read articles in *Air and Space* magazine about the loan of the Tu-144, the Russian supersonic airplane, to NASA for research, and I read the story of their negotiations, and I said, "Yes, change the names, and the policies and the plans are the same." I hear about [Vice President] Al Gore talking to [Viktor] Chernomyrdin about negotiating things, and I know what's going on. I just know how come things don't work.

Another example, early in the program, and I guess consistently throughout the program, we had a tough time getting our hardware through Russian Customs. I still get a cold feeling in my stomach when I think about the Customs desk at Sheremetyevo Airport in Moscow, because I know it's just as likely that we can go through completely unhindered or we can be stopped and the hardware impounded for weeks and weeks and weeks. Data-collection hardware going over there to collect data on cosmonauts. Doesn't matter. The hardware's impounded.

We worried and complained about that, and complained through our management, and I guess, eventually--it took a lot longer than it seemed to need--the complaints seemed to filter up to the point where I would hear on the news reports that Al Gore was going to talk to Chernomyrdin about Customs and getting stuff in to the country. Then they'd have their little *tête-à-tête*, and they'd smile and shake hands, and Al Gore would say, "Well, fixed that one. It's solved. No problems." And it didn't make any difference, because one level below Chernomyrdin, nobody heard or nobody cared.

That was an interesting phenomenon, too, was that we were frequently told by the working-level troops at Star City or at IBMP or someplace that whatever agreement we'd made to get whatever data we thought we needed, whatever resources, or whatever services, "Oh, that was signed at too high a level. How can you expect us to know about that? That wasn't our level. That was two management levels above us, so there's no way we would know about that." There was very definitely on many occasions a lack of downward migration of data, of agreements. Somebody may sign on a piece of paper and say, "Okay, I agree. I finally agree with all of your negotiations." It's like he folded it up and put it in his

pocket and go home, and that would be the end of it. He had his piece of paper with his name on it, but the next level down didn't. If the next level down got any of these documents, they always seemed to be two or three revs out of date: the old version, the version that we don't want anymore because there's mistakes in it, or we can't do that, or something.

Another lesson that I think we learned--I suspect you've heard all this from John Uri before--another lesson that we learned is that you never give your Russian counterpart a document unless it's *the* final document. You don't give him a draft, because that draft ends up institutionalized. The first document they get, they take as the document. If second and third versions come along, they say, "I've already got that," and they throw it away, or they give it to somebody else who takes that as their version. "Look, I've got an extra copy of this. There may be some differences, but it's not important. Here's your copy."

So you may be dealing with somebody, number one, that's got a document that's out of date, because we at NASA believe in killing many trees, as often as possible, and making as many versions of a document as can physically be run through the Xerox machine. Then you have the Russian that got [what he believes is] the one and only original document, somebody else may have a second rev or a version of the first that's badly copied and unreadable, or has pages missing, and then you're trying to negotiate based on what your current thinking of the document is, with people who don't think there is a different version of the document. Like I said, I can see evidence of that, with hindsight, in a lot of other things that NASA has negotiated with the Russians.

The Russians are good negotiators, we're bad negotiators. We may be getting better, but the Russians aren't getting worse. They're still getting better, too. They're very, very smart. They're very smart people, collectively and individually, very smart and very focused, and they know exactly [what] it is they want, and they know what we have. They know what we have, and they know what they want, and they know how much we're going to give them. There's really no mystery to them. They know if they persist--and they're very persistent--they're going to get what they want. If they give us something, it's pretty much out of the goodness of their heart.

Wright: You mentioned that every detail on every science project and that was done, but did you also have to negotiate for efforts that dealt directly with Norm, or with any of the other astronauts, or the Mir residents as you were coming through, or even of the other folks from NASA that were over in Russia? Did you have to do negotiations and help support the team effort that was over there as well?

Charles: Yes, I did on a couple of ways. The way that I can think of right now is during Norm's flight I

was, like I said, Peggy Whitson's alternate, and Peggy was the ops lead, because it was an all life sciences kind of mission. We were Project Science. Peggy was the ops lead for most of Norm's flight, and when she needed a break, I went over there for six weeks as the ops lead in the TsUP, in the consultants' room. We had the MOD folks along as sort of observers just to sort of take notes and see how things were done. It was very interesting that later the role was sort of reversed, and MOD was the ops lead, was telling us how things have always been done. That was kind of amusing to me, because I'd taught this guy how to do a lot of it.

At one point I was amused because Victor Blagov called me in, and I took some of the MOD folks with me, and then I said, "Don't say anything. Just listen. Let me do the talking." Again, very ironic in light of later events. But he was asking my permission to have Norm Thagard involved in doing some switch throws during the EVA that the other cosmonauts were doing in flight. I was thinking, "Of course Norm's going to do that. We're there to learn, and you're not going to stop Norm," but he had to go through the formality of asking the senior NASA guy at the moment in the building if it was okay. So I said, "Well, it's probably going to be okay, but I have to check." So that was my one instance of negotiating for the entire team, I think.

But there were daily, almost daily, negotiations with the time-liners, for example, getting the time-line plan for the following week, and then revisions to the time-line as things broke or things get didn't get done or other things came up. There was a formalized process with Nadia. Nadia Salmatieva was her name. Nadia was the “time-line lady,” and she would write out the time-line on this huge long piece of paper for the week, and we'd go over day by day and discuss it. I think Tuesdays or something was the day that she had to come in and talk to us, if she can get it ready for the rest of the week for the next week.

So there was that kind of ongoing negotiation, where the mission science rep at TsUP would have the list of activities to be performed, the times they were supposed to be performed, to work with Nadia to get that stuff time-lined, and we did that. Mission Science* did that for most of the increments until the MOD folks realized that they were being time-lined by someone that wasn't an FAO and wasn't an MOD person, and, "We can't let that go on!" So they MOD folks nudged us aside and said, "We'll take care of this from now on. We'll take your inputs, but we'll be talking to Nadia."

* “Mission Science” was responsible for the scientific content of each mission. John Uri was the Phase 1 Mission Scientist, and I was the Mission Scientist for NASA Increments 4 and 5 under his lead.

That was kind of funny, because the Russians also are strong believers in personal relationships.

So Nadia would look for the mission science folks she has been dealing with, and wonder who this other guy is and why he's insisting on talking. So there's always that kind of thing. The Russians are very much one-on-one kind of people. I think John Blaha made some comments about dealing with Russians one on one versus two on one, versus many on one, or many on many. There's different ways the Russians deal with large or small groups of people in private or public settings. So there's lessons to be learned about getting just day-to-day kind of things done in that regard.

So that was an example, I think, of my involvement in negotiating larger activities when I was representing mission science there in the TsUP, and that was the second six-week rotation when I was doing that. There was a lot of that broad purview being focused onto specific time-line events and time-line activities.

Wright: What about when you were back here? How much were you able to support the team that was over in Russia?

Charles: Well, having been over there for extended periods and over here for extended periods, I would try to do everything I could to support them, and it was never enough. I knew from experience [while I was] over there that the folks over here were trying, but it never seemed to be enough. The answers always seemed to be late and wrong.

For example, a crew member would call down during a com pass and saying, "Do I flip the switch up or down?" "The light doesn't go on when I flip it. What do I do?" Of course, we wouldn't have the answer, wouldn't know the answer in most cases. We'd have to call back to the States, but there was this nine-hour time difference, and usually when we got our calls it was midnight in the States, so we'd call the folks in mission control here, and they would try to find the discipline lead for that question, and then the discipline lead would try and find the engineer, the engineer would have to call the technician. So in some cases it was hours and in some cases it was weeks before we got an answer.

So I guess the answer to your question is that the folks here, like myself, would always try to be helpful and try to be supportive of the operational things in TsUP at Moscow. Many times, I think, the impression was that it was not helpful. I know my impression was the folks back in the States were not helpful when I was over there. And when I was over here, I knew we were working as hard as we could to get the answers, and why are those folks in TsUP so impatient? Don't they know how long it takes to get answers?

I think that was a real educational or enlightening experience, too, was to see how slow we were in getting answers. A lot of it was also the problem with the NASA and MOD mentality. If there's a problem

on the Shuttle, immediately an entire army goes off in multiple directions and try and figure why that widget is colored green instead of blue, and why the switches don't feel the same way when you flip--lots of effort devoted immediately.

Part of that was the American on the Mir had that same attitude, having flown the Shuttle before, and the two cosmonauts, were used to working with the Russian system, were aware that answers didn't have to come immediately, because, "We're going to be here tomorrow, and we'll be here next week. What's the big deal?"

A lot of the Americans had that culture shock with the Russians in TsUP about the same kind of thing. It was, "What's the rush?" The Russians will say, "What's the rush? They're going to be there tomorrow." Like an American will call down with a problem with the camera or videocamera, I think, is one instance. The Russians say, "Oh, yes, I think we have the manual around here someplace. We'll dig it out for you."

And the Americans say, "Okay."

And the Russians will say, "Well, not right now. Later, tomorrow, next week, after the weekend, something. We'll get it for you. It's no big deal."

The Americans say, "Oh, no, you don't understand. The astronaut wants the answer. That means that the next time he comes on the loop, I have to have the answer."

The Russians say, "Why? He's not going anywhere."

So there's a lot of this kind of culture shock. That was the difficulty, I think, of us supporting them, the Americans in TsUP, and then supporting the mission, because we all had different ideas of what the time frame was for an answer that was required. So like I say, I know when I was over there I thought that Americans were completely unresponsive, and when I was over here trying to be responsive, I was wondering, "What's the big deal? Why are they in such a hurry? The cosmonauts aren't going anywhere."

Wright: We understand from Dr. [Michael] Barratt that every time he called you, you'd pick up the phone and give him the same jovial answer. Do you remember those times? Because he said no matter what the time of day, no matter what the situation as, that he'd call you and you'd pick up the phone, and you'd say, "Yes, commissioner."

Charles: [Laughter] That's right. The funny thing--he said that. That's great. I'm glad he remembers that. During Mir 18, Norm Thagard's flight, Barratt would go down to the floor [of the control room]. The consultants' room was sort of up above the balcony and back down a hall, but every time Mir 18 came over the horizon, it was time for one of the com sessions, Barratt or one of the other flight surgeon would

go down and would be our CIC, our Cap Com, would go down to the floor and be waiting to talk to Norm during the ten seconds or fifteen seconds that they gave him to talk to the American.

There were these old phones, no dials, it was just the old pick up the phone and talk kind of thing, maybe one or two buttons, I forget now. It looked just like the old phone under the head of Shakespeare in Bruce Wayne's study on the old TV show [*Batman*] on the commissioner's desk. So we'd be listening to the air-to-ground and hear Norm say, "I'm having trouble with such and so. Can you tell me what it was?" I would hear Barratt say, "Standby. I'll check, talk in Russian or something, talk to the Russians." And the phone would ring. Of course I knew who it was. And just this mental image of the phone ringing, I always imagined myself as Batman. When the phone range I'd say, "Yes, Commissioner," or "Yes, Batman," whatever the mood was in the moment. That always seemed to catch Mike off guard and gave him a chuckle, and he'd try to remember what it was he was going to ask. We'd say, "You were probably going to ask about the cooling loop," or something like that.

That was, I think, an example of the kind of "us against them" camaraderie. We felt--I know I felt, I shouldn't speak for the rest of the team, but I know I felt very much like a stranger in a strange land when I was in Moscow. I really admire and respect folks like Mike Barratt and Dave Ward, the two first flight surgeons who threw themselves into the process vigorously, not as docs, not as flight surgeons, but as our liaisons to Star City and to the Russian process. They learned Russian. Mike Barratt, I think, speaks colloquial vernacular Russian probably as well as any American. Dave Ward seemed to struggle with it, but certainly got his point across. And they also tried to understand not just the medical thing and the astronaut, but understand the entire payload. They were our liaison. Many times they knew more about the payload than we did, or knew more about the experiment than we did, so they were very, very useful as resources.

For example, when I was in the TsUP during Mir 18, I may have been sort of the ops lead, although we didn't really call it that, I was just the lead, the head of the American team, but we really sort of had parallel things going. Barratt was the flight surgeon beholden to no one, and I was the science lead beholden to no one, and we tried not to step on each other's toes, and we also tried to support each other.

So it was very, very easy to support folks like Mike Barratt and Dave Ward, because they were so enthusiastic. Nothing ever got them down, and they always understood what was going on, or at least understood better than anybody else what was going on, and they were always a good source of information and a good source of insight into what was happening. So it was easy to have that kind of easy, silly camaraderie. When I'm comfortable in a situation, that's always what I try to do, too. I try to keep things light, because there's so much heavy stuff going on, that it's kind of fun to sort of catch a guy off guard,

and you give him a break when Norm Thagard, for example, was calling down and complaining, as Norm seemed to do a lot on Mir 18 about things, and Mike was trying to be conciliatory and trying to assuage his concerns, and tell Norm it really is all right. Norm was saying, "No, it's not all right. It's bad. You haven't done what you promised me you were going to do," and things like that. So I was glad to help Mike. I'm glad Mike remembers the lighter moments. I hope he remembers me always in those ways and not the times I failed him in other areas.

That was kind of fun, though, because that phone just reminded me so much. It was 1960s technology, and it would sort of blurble or bleep or ring, or some sort of annoying sound. All I could think was that little sound bite from the 1960s TV show. so it was kind of fun.

Wright: That was, of course, the first experiences, but as you worked through the years and even though there were a few years, I'm sure they were busy years, how was it different for example, the last mission? What do you see as some of the changes, or were the experiences vastly different, or were some of the same situations existing as you moved through at that time?

Charles: Well, the first mission I supported was the Mir 18 mission in '95. That was Norm Thagard's three-month flight, and that's the mission I supported in Moscow. The second to last time I spent six weeks in Moscow was for John Blaha's mission in '96. So it was only a year different, but by the second wave, by the NASA-Mir Program, which was different from the Shuttle-Mir Program, an arbitrary distinction, but an important distinction, nonetheless, it was the MOD-run show, and we had a cast of thousands. At one point we had, I forget, four ops leads: one prime, one alternate, one in training, and one sort of rotating out. It was one of those cases where it was almost comical. The old saying about too many chefs--too many of these testosterone-charged guys and women trying to be in charge and trying to make snap decisions, and show how they can be good flight directors, and things like that. A very visible, very important job they were doing. They were all very smart, very good, very capable at it, but their inclination always seemed to be more, "Got to do something right away, and it's got to be quick, and it's got to be fast, and it's got to be good, and we've got to shine." They seemed to be inclined to make snap decisions based on what we science types thought were inappropriate understandings or inadequate understandings.

It was probably a reiteration and microcosm, of the old engineering-versus-science mentality. Engineers, especially the MOD engineers, do a lot right away, fast, and then move on to the next thing, and the scientists have to think about things. There's a shade of grey. The engineers says, "Yes or no?" and the [scientist] answers, "Maybe." That just makes them nuts, because, "Maybe? How do you put a 'maybe' on an integrated circuit? There is no ‘maybe;’ it's ‘yes’ or it's ‘no.’ Either it's working or it's not working, or

you can get all your data, or you can't get any of your data." I said, "You can get some of your data some of the time. It's fuzzy logic."

I kept calling it fuzzy logic, because I figured that was an engineering term they would understand, but there was a real difference there between when we were doing only the life sciences experiments, only on Mir 18, and we were sort of the entire show, and there was maybe three of us in the consultants' room, and the time when there was maybe twelve people in the same consultants' room, fighting over chairs, over the same table, with largely NASA-imposed reporting and other time-line schedule pressures.

On Mir 18, once a week we called in and gave a report. On Mir--I guess it was Mir 22--it was Blaha's flight, there was daily reporting and multiple copies of daily reports, and reviews of daily reports, and multiple telecons each week, and just more and more interest, and more visibility, and more responsibility back to the folks in Houston. So it was really more high paced and more distant. And there was more science in more areas, more disciplines being covered, so it was more responsibility back to different organizations in the U.S., back to Ames and back to Marshall in Huntsville, and Lewis. I don't guess we had any Langley stuff. But lots of stuff like that. So there was just a higher paced, more hectic kind of thing. It does have the advantage of focus, though. That was probably the only nice thing about the two six-week periods I spent in Moscow, was the fact that I had one job to do, and not like I do here, I've got three or four different jobs.

You have one job, you go in, the job for today is to get the time line ready. So you go in today and you work with Nadia, the time-line lady, and you work with the Americans, and take all the E-mails, and sift through them and find things. At the end of the day you have a time line ready, and then you can go home and sleep. Then tomorrow you come in and do another job. You have to get a radiogram ready for procedures, something like that.

It was like a certain clarity of purpose and a certain singleness of focus that was lacking in the States. So that was a common feature to both of my experiences in Moscow. Otherwise, the first flight, all medical stuff, mostly medical stuff, Norm Thagard, and we were in charge. The second time it was, "Oh, yeah. We've done this a lot of times before, here's our multiple procedures for doing what you all used to do," sort of off the cuff, and it was different experiments, lots of disciplines, and this huge MOD overlay on top, and then the Russians have gotten wise to us, too, so they understood. They could slow us down by asking for copies of documents and procedures and stuff. So there was lots of negotiations and arguments that way. It was really sort of like apples and oranges, almost, with the few commonalities.

Wright: Most of what we've talked about, of course, has affected you professionally, but was this your first time to go to Russia when you started with this program, or had you been there before?

Charles: No, I had not been there before. I'd always been left behind as unnecessary on previous trips, but with the start of the NASA-Mir Program, the Shuttle-Mir Program, I guess, over the course of my--I forget how many years it was I was traveling to Russia. I made thirteen trips to Russia, and that was plenty. It's been, I guess, about a year and a half, coming up on two years, since I've last been to Russia, and I really don't mind. I don't miss it.

There's a few small things I miss, like a certain woodcarver I used to see out the flea market, who always had copies of statuettes of cosmonauts, and I'd always buy his newest cosmonaut. And believe it or not, I enjoyed a lot of the food, especially when I was taken to people's apartments and houses and they put on big spreads of their own local, regional specialties. I always enjoyed that.

But a lot of the stuff, going into the nineteen-hour flight [to get] there and the nineteen-hour flight back [to the States], the airport, the silliness of exchanging money, and the general unpleasant demeanor of a lot of the folks I came into contact with personally and professionally, I don't miss it at all.

The first opportunity came as a part of the setting of the NASA-Mir Program. I hadn't been as part of any of the other working groups before that. I made several trips that were just, I guess, one- or two-week trips before I made the six-week trip over there in '95.

Wright: Do you feel through your efforts there's a time that you remember that you would say is the most significant accomplishment, or the most significant contribution that you were able to make as part of this program? One negotiation was the best, or one answer that you were able to give that seemed to solve a problem?

Charles: I hope I solved a lot of problems. I can't tell you any specific instance where I did that. I hope that I did so by presenting what I thought was a reasoned and reasonable American position in the negotiations. I hope I was credible to the Russians. I know I established, I think, good relationships with my counterparts, at least in the life sciences area. I have very fond memories of them personally and professionally. I hope that I was able to be credible inasmuch as I would draw lines in the sand that I would not back away from, because to do so would render the experiment meaningless or undoable or something.

I recall one instance in negotiations over here [along] with my American counterparts in the life sciences area where the two or three factions, I think there was two people from IBMP, and one from Star City, and probably not an Energia rep at that point, who were negotiating with us about, I think, a sleep study, and it was a study of some of the characteristics of sleep in extended weightlessness. It was a peer-reviewed, approved investigation. We had promised these investigators we would get their data. We had

given them [the Russians] money to get them ready, and there was some difficulty with that. I can't tell you exactly what it was. It seemed to be compensation to the cosmonauts for this, and the Russians kept talking about “motivation” for the cosmonauts to do this study. I'd say, "What do you mean?" knowing what they meant, because we talked about it before.

They'd say, "Well, the cosmonauts have to be motivated."

I'd say, "How so?"

Eventually it came out that the Russians hadn't seen the money that was promised to them. Energia had the money, but it was not getting down to IBMP, and they were out of money, and they had been out of money for months and years, and nobody being paid, and they couldn't run these--they had to borrow other facilities from other institutions to do some of this stuff, and they couldn't pay those people.

At one point I did my best Nikita Khrushchev banging the shoe on the table thing. I said, "Look, that is not my problem. That is your problem. This study will be done. I will not accept no for an answer. This study will be done, and you will solve this problem. You will talk to each other, you will get the money, you will talk to somebody else. I don't want to hear about this problem anymore. You will fix your problem." And I got up and I left, and I had no intention of going back at all to talk to those people. They were scurrying around, because that was not me. That was not typical of me, but I was really just “up to here” with it at that point. All very apologetic. I walked around outside the building, fuming for, I don't know, forty-five minutes or two hours or two days. I'm no sure how long it was. Eventually, one of the Americans came up and said, "I think they want to talk to you."

I said, "I'm not talking to them until they solve the problem."

"Well, we've moved into other topics, so you can come back in and talk."

But that changed my perception of the negotiations. I think that changed their perception of me, because I never went back to being as chummy and friendly and all that sort of stuff. I was always much more reserved after that, because I wanted them to understand that at least one American wasn't going to be pushed around like they were used to pushing around other Americans. I know other Americans have tried to make similar shows of strength in similar ways. Some folks may say it's pointless and it's counterproductive, and other folks say it was the right idea, the wrong way to do it, but I think we all had to sort of decide on our own what the best way for the circumstance was.

I know, after that, I was not interested in negotiations much after that. I was pretty much interested in saying, "Here's our experiment. Somebody signed off on this experiment. How are we going to implement this? I don't want to hear about funding, I don't want to hear about motivation, I don't want to hear about difficulties you have. I want to hear about getting this experiment implemented."

Shortly after that, I'm not sure how soon after that, I moved on to the Mission Science thing where I was out of that particular negotiation. I think the guy that followed me had his own way of exerting his influence and making his presence known. So that was an example, I think, of my imprint on the way that some of the negotiations were done.

Wright: Do you feel that the science world, the science investigators, the science arena has benefited from all of this effort from people like you that get involved in the program?

Charles: I think it has benefited. I think that investigators--probably out there you'll find some that think that they have been poorly served. I know when I was an investigator and having to work through all these intermediaries just to the American side when I was, for example, an investigator on the Shuttle flights, having to work through several intermediaries to get to the astronauts on our own Shuttle and our own national space program, I felt like there was an awful lot lost in the translation from this party to that party. I have no doubt that there are investigators in universities out there in other disciplines, for example, biology or crystal growth or whatever, that think that this biologist, this life science guy, John Charles, who was doing our negotiations for us, just didn't really grasp the big picture, and really didn't give us as much as he could have. I also believe that we gave as much as we physically were able to give, as much as could be gotten from the Russians.

I think that's probably typical of any scientist's involvement in any space flight program. Like I say, when I was a scientist, I always felt the intermediaries were doing a bad job. When I was an intermediary, I'm thinking I'm doing the very best job possible. I think we intermediaries, like the folks in the Mission Science Office for the NASA-Mir Program and in the Mission Science Office, whatever it's called, for the ISS [International Space Station] Program will always be considered to be doing a bad job by both ends of the extreme. The scientists will always say, "Why aren't you getting me more of what I need?" The planners and the MOD folks and the crews will always be saying, "Why are you making so many demands on us? Why can't you take what we give you and be happy with it?"

For each of these, we have to wear the other hat. So we're talking to the crews, we have to have the appearance of these fuzzy-thinking, starry-eyed, egghead scientists who always want one thing more, and when we turn around and face exactly the other direction and talk to the scientists, we have to have sort of a steely gaze and the indifferent attitude of the planners who say, "Look, we've given you everything we can logically give you. You're asking for too much."

For example, the sleep studies that came into the Mir Program that I was responsible for fitting in, at one point they wanted fifty--five-zero--consecutive nights of monitoring. I'll pause to let that sink in.

Fifty nights of monitoring in fight. They ended up getting, I think, thirteen. And thirteen, eventually, I think, became five on a few occasions. That was a huge, huge undertaking for the cosmonauts. But, see, the scientists would walk away saying, "Oh, those NASA folks, they don't have the foggiest idea of science, good, meaningful science, like we do, because they wouldn't give us our fifty nights of monitoring. They had to settle for five."

And the cosmonauts are saying, "One night of monitoring is too much."

We had whittled it down. We, the Mission Science folks, had whittled it down to thirteen before we even talked to the cosmonauts or the astronauts about it, and they said, "Thirteen nights of monitoring! You're out of your flippin' gourd! There was no way I want to do it, and even if I did, there was no way you could interpret it. There's no value to that. We'll give you one, occasionally. One person, one time, one flight."

So we have to go back and say, "That's not really meaningful. We need to get more. These PIs are being funded, and they're peer-reviewed, and you really don't have a choice about this. Either you don't do the experiment or you do the experiment, but you always have the option not to participate. Nobody can force you to do this stuff, but I'm going to tell your boss if you don't, because these guys are being funded, and, yes, you have the right, but you've got to keep the big picture in mind."

And eventually the crew members, we got a few crew members that did some portion of it. It's a very complicated protocol, for example, the sleep study, [very complicated,] involving lots of electrodes in the head, and lots of logging and lots of tests, laptop-computer kind of performance tests before and after the sleep bout. These things always sort of sometimes got forgotten, like, "Oh, I forgot to write my dream log," or, "I forgot to take the performance test," or, "Gee, we lost pressure in the cabin so I didn't get around to doing this thing. Hope you don't mind."

So the data set that was handed back to the scientist's was never what they expected. I can only hope that they were able, with the benefit of hindsight and reflection over time, to realize that what they got was really pretty much the most significant data set every collected in that environment.

Interestingly enough, I always have recollections about the sleep study, because I am a firm believer in the importance of circadian rhythmicity and sleep, and well-rested crew members. From my Shuttle experience, I know after one Shuttle mission, I think STS-26, I was at the landing site to do some cardiovascular measurements, and I recall watching the crew come down the ladder from the Shuttle, the stairway from the Shuttle, all bright and bouncy and happy and victorious. And as they came into the clinic and the door shut behind them, I remember I watched the color drain out of their faces, and the vigor went out of them. They were exhausted. At that point, I thought, "Am I measuring the effects of

exhaustion or the effects of cardiovascular changes? Probably exhaustion. That is really not what I'm here to measure. I'm here to measure the effects of space flight on the cardiovascular system. But these guys are exhausted. We're working them too hard." At that point, that's really the clear demarcation in my mind of the importance of making sure the crew members are well rested, well nourished, and well hydrated, and happy and productive in space.

As a scientist, as a PI, I would always say, "Public affairs? That ought to be time they're doing my experiment, not talking to schoolkids, or talking to the President, or looking out the window. That's nuts! They have eight hours of sleep a night, which is probably too much. We ought to be making them work from the moment they wake up until the moment they go to sleep." And at that point I sort of became a convert to the astronaut position, which is, "You don't make anybody else work that hard for four days in a row, or eight days in a row, whatever a Shuttle flight is. How can you reasonably expect us to?"

I say, "Oh, yeah? But every else is not in the space Shuttle flying above the Earth at huge expense to the government, with the only opportunity to make these measurements. Of course I'm going to make you work that hard, that long."

Then I finally realized, and I know I'm not the first one to realize it, I know lots and lots of folks realized it before, but it finally became clear to me that people that are working that hard aren't going to be good subjects for the medical studies anyhow. They're exhausted. They're going to make mistakes. The data you get back, even if they don't make mistakes, is not going to be what you're looking for. So it became clear, especially on the longer flights, that we needed to be doing more about sleep quality and normal work cycles, and making sure they're healthy and happy and productive in flight. So I've been a big believer in the sleep studies, both the ones we did on the Mir Program and the ones that we're doing on the Shuttle now [which I am overseeing in one of] my other assignments.

Those were always the hardest ones to defend, because, by definition, they impact sleep. Astronauts say, "If I'm having trouble sleeping, your experiment doesn't help. If you acknowledge that I'm having difficulty sleeping, why do you insist I do this experiment?" And there's that convenient "can't make an omelet without breaking eggs" argument. "I can't make you better until we understand what's wrong." And astronauts don't like that logic. They say, "Look, it's plenty wrong. Just leave it alone. Don't make it worse." And the whole idea of making it better by making it worse doesn't really translate well into astronaut thinking or MOD thinking. So there's always that ongoing battle about, "One night of sleeping measurements is all you're going to get."

But the thing I recall about the sleep study on the Mir Program, it was one of the first bouts of sleeps that, I guess, one of the astronauts had done it first. Maybe [Jerry] Linenger had done it. But it was

time for one of the cosmonauts to do this sleep monitoring study, and I think it was the next day after that that he lost control of the Progress vehicle and it crashed into the Space Station. So people always said after that, "See, that sleep study is the problem." Now, it turns out that, in retrospect, the analyses more recently don't even mention the fact that he did a sleep study. That's always whispered. But now they say, "Oh, it [had] been months since he trained, and the hardware wasn't appropriate, and the lighting was bad, and the simulation showed it was going to happen anyhow. In fact, the previous instance had gone wrong." But they always say, "Don't forget that sleep study, too." So you're sort of scarred, you're sort of tarred by that. That was on my watch, so that was one of the things I was pushing real vigorously, was the sleep study. It's hard to keep your credibility when an astronauts ruins a mission because of your damn science.

Wright: How dare they, right?

Charles: Yes, that's right. "Just leave us alone and let us fly our mission, and don't worry abut your damn science."

Wright: I have to make the statement that listening to you, there weren't many days that were boring while you were a participant in this program.

Charles: Yes. There was nothing boring about the program, unless I just chose not to let it bother me at some point. There's lots of non-boring instances. A lot of it might have been boring in the sense that, "That's another negotiation and I know how it's going to go."

I fancy that I have this ability, after I get to know a situation, to sort of see it as theater. I know in a lot of the meetings that I've sat in in JSC [Johnson Space Center], when I get to know who the players are around the table, I know who's going to misunderstand something and who's going to have a different agenda. I can sort of almost, if not predict what the interaction are around the table, I can sort of understand them as they unfold. I've explained to people, "She didn't hear what was said, and he thinks they're saying something else. Now watch. He's going to react differently, because he was thinking of something else," and then it'll sort of happen.

So the negotiations would sort of get to be that way. I would sort of know when the Russians were going to come in wanting money, and they're going to say, "We can't do the sleep study," or, "We can't do the MRI study," or something. You sort of have to watch it unfold, and it usually unfolds very slowly, so you sort of start waiting for the other shoe to drop, and, "Okay, let's get to the part where you say you can't do the study so we can address that."

Wright: Cut to the chase.

Charles: Cut to the chase, yes. And the Russians love to make speeches. They love to speechify. One of the first guys that I dealt with in Mir 18, my cardiovascular counterpart, would always speechify. He was one of the most annoying people I've met. He was one of the least well liked Russians amongst the Russians as well as the Americans. The translators would say, "Oh, we can't get started yet. He hasn't annoyed everybody in the room three times."

But this guy, when I would ask him a question I would say, "Do you have data on heart rate in space flight?"

He would say, "Well, the Russian space program, which began with the launch of Yuri Gagarin in 1961, as the first Russian to fly in space aboard the Vostok 1 vehicle, which was launched by an R7 booster out of Baikonur, the first of many glorious successes in the Russian program, the second of which was Gherman Titov in Vostok 2," and he would give me a litany of things that dealt almost not at all with the question. At the end of it, he would say, "Of course I am a qualified surgeon, unlike you, so I know a great deal more about the human body and its responses to space flight."

I'd say, "Yes. Can you tell about heart rate in space flight?"

"I just did." So there was that kind of response. That was probably peculiar to this guy, but that is not unusual in the Russian business.

So that's the kind of thing I guess I don't really miss about the dealings with the Russians, but they all have to speechify. Bless their hearts, when they're having meetings, everybody gets a chance to make a speech. So if you've got a group meeting where they're trying to decide is it safe to fly or safe to do an EVA, are we going to accept this experiment, at least one rep from every organization has to get up and make a speech about the glorious days of Yuri Gagarin and how "We've solved many problems and the Americans were just newcomers at this, and perhaps they'll learn after experience with us."

That guy will sit down and the next guy will stand up and say, "I agree with my comrade. We've been doing this since Yuri Gagarin, and the Americans are wet behind the ears, and by the way, we haven't seen our money yet." And just all these extraneous things. That sort of filters into, I think, the toasting they do as well, with vodka. There's always florid fancy toasts.

I'm not sure if this relates to your question anymore, but I remember one experience getting ready for Mir 18. The experiment was to use the lower body negative pressure device, which was my area of specialization. We were going to use the Russian “Chibis” suit. They call it pneumatic trousers. It's rigid pants that you can decompress inside, and it sucks some of the blood down and makes the body react [really, the cardio-vascular system] as if the body is standing upright in gravity. You can do this in

weightlessness so you can stress the gravity functions of the body in weightlessness.

But we finally got a chance, finally convinced the Russian specialist in Star City that it was okay for us to come to his lab, and okay for us to see this secret hardware that he's got that we're going to have to use, and had it demonstrated for us. As we were leaving the room, I said "I appreciate your showing us this, and we look forward to working with you." He said something in parting, something along the lines of, "We have learned many things about the human body in space flight. You, too, must learn those things," and then he left. It wasn't like, "I will teach you here. Let's be friends. We'll work together." It was like, "Call me when you get to be an expert," that sort of thing.

Interestingly enough, this guy turned out to be one of my best friends on the program, because maybe he had been called out of some other meeting or something else to come out and talk to this American that he didn't know and didn't care about. So he had to come in sort of late and hurriedly to give us this briefing. Later he got involved in the program with us, and he was one of my best friends and closest colleagues. So that personal thing, the first meeting was not very pleasant, but with the repeated meetings we got to be good friends and colleagues. Now we try and stay in touch as much as possible. But I always remember that. "You, too, must learn these things." It wasn't like, "I'll help you." It wasn't like, "Here's a book, read up on it." It was like, "You, too, must go off and learn these things, and come back and we'll talk."

Wright: "Call me."

Charles: "Call me. My people will call your people."

Wright: It was on your watch that the collision took place?

Charles: I think it was. Maybe it was a near miss. It's really sad, because it was only a couple of years ago, but I forget if it was a near miss or the collision. Let's see. Linenger was the fire, and Mike Foale was the collision. I was the mission scientist for both of those. I was over there for--no, I was over there for Blaha's flight, so I wasn't over there for Linenger's flight. [It's embarrassing. It's embarrassing.]

Wright: One could say it kind of starts running together.

Charles: I never believe anybody else who says, "Gee, I don't recall if it was the collision or the near miss." But in my case, I don't remember if it was the collision or the near miss, but I know that when the collision occurred, it was--yes, that's right, that was the collision. It was the sleep study. The commander had done the sleep study.

Wright: How did it affect the science, the collision?

Charles: Good question. That's an excellent point, and I wanted to make that point. I'm glad you said that. It had the effect of shutting down the science program, and that was just one example. I mean, the fire did the same thing, and any time an air-conditioner sprung a glycol, they did the same thing. I was like the MOD guys would say, "We've got a problem. Let's shut down the entire science program, and then we'll decide how to fix the problem."

We would say, "Well, you know, that's nice, except when it's all said and done, Congress isn't going to ask, 'How many times did you fix the air-conditioner?' Congress is going to ask, 'Did you cure cancer? Did you see pictures of Jupiter?' Stuff like that. Why don't we decide what the problem is and then shut down enough of the science program to give you the time to fix that problem, but not shut down all the science, because some of the science can keep going anyhow. I mean, you've still got to sleep, and we still can make measurements. You've still got to exercise, we can make measurements on that. Let's not just throw everything out until you decide at some time, days, weeks, months in the future, that it's okay to start science again, because we've got a schedule, we're behind schedule, because of the last time you did this." So there is this, again, this science versus engineer, MOD versus mission science kind of different way of seeing things.

I remember one of the ops leads over here who was rotating back over here, who took a personal interest in making sure that we scientist types didn't get too far afield in this emergencies. He kept saying, "You guys keep wanting to do the science, it's like rearranging the deck chairs on the *Titanic* as it's sinking."

I thought, "Well, that's probably a pretty good idea [or analogy] as far as MOD is concerned. Science is pretty much a time-filling activity. It's not the important reason. You fly to fly, and, oh, yes, if you got some spare minutes in flight, then you do some science." Speaking unfairly, of course, our impression of the MOD folks was that their idea is to punch holes in the sky. You light a big rocket, you send the guy up, and he does important heroic things. Then if he's not busy doing whatever it is that's important and heroic, maybe he can grow a plant for you or make a sleep measurement or something like that, but that's really not the reason we're flying. We're flying because we fly. That always seemed to us to be very, very strong MOD logic. "We fly because we fly. What's not to understand?" We'd say, "No, you fly to do things." They'd say, "No, no, no, no. We do things when we have time."

Of course, we have the other idea that the air-conditioner's broke, so it gets hot. "Let's do some science." Or you're drifting out of orientation, "Well, we've got some things you can do without power. Here's what you can do." Or, the Progress bounced off a solar panel. "Well, okay, one of the Russians can

go do that, but here's some science we can do in the meantime."

So they'd say, "That's completely unrealistic." And of course it is unrealistic. You want to make sure the vehicle is not losing air and not filling up with carbon dioxide before you do your science.

Now, interestingly enough, we had some difficulty explaining that to some of the American scientists back in the universities and other places here who perhaps weren't as in tune with the mission as we were, living with it every moment, but nobody in the science flight ever argued [or disputed] that the first priority was to keep the crew alive. You never give up [e.g., "lose"] a crew member to do science. That's one of those rules that you draw in the sand. Death is not an acceptable alternative if it means science. So we understand that. We just didn't feel like the appropriate perspective was taken. We felt like the perspective was always, the impulse was always, "Cancel the entire science program until we get this problem resolved," and the problems were never resolved. There's always another glycol leak, always another solar power loss, there's always something.

So our concern was, yes, we understood priorities, keep the crew alive, keep the vehicle intact, keep the pressure up, keep the power coming, but there are certain things you can sort of interweave in that. Like maybe two guys can work on the solar cells, because the third guy, the American's just in the way anyhow. Let him go off and tend to the plants. We tried to keep that perspective. If they refer to it as rearranging the deck chairs on the *Titanic*, that's fine. If that's a convenient mental image for them, that's okay.

Wright: Were there many deck chairs left after the collision? Were you able to take what you had and correct the situation?

Charles: On Foale's mission, a lot of the science got canceled. Some of it did continue. I think some of the biology stuff and the things that were [mounted] outside [of the station] were exposed to space environment, of course, continued to work, and through a lot of heroic efforts of the science folks and the MOD folks, a lot of that stuff was recovered.

A lot of the experiments that were pretty much written off were recovered and, I have to say, the Russians on many occasions showed us ways to do that, too. For example, the greenhouse, the growing of plants on the Mir, the Russians seemed to have a very strong attachment to, probably because one of the Russian counterparts was very vigorous and very forceful and very well liked in the control room, or not liked, maybe they just did what he wanted to get him out of the room, but the Russians would always say, "Oh, by the way, even though we're cutting power back, we have enough power for the greenhouse to keep running." So the Americans would say, "Okay. Well, that's a victory. We'll take that."

As I recall, after the difficulties in Foale's flight, we lost a fair number of scientific objectives, and I can't tell you off the top of my head now how many and what percentage. I know we went through a lot of exercises trying to figure out what percentage it was. I have to say that we were amused that our management was always trying to find the silver lining. It's a very good feature to have in your management, especially when they have to go up and talk to Congress. But they kept saying, "Well, if you gave up that requirement, then that means it was never really on the books anyhow. So we can say he got like 100 percent of his stuff done."

And we'd say, "Look, he didn't do half the experiments. Half the experiments are in the module that's blocked off. You can't say he did 100 percent of his payload."

"Well, can you say we did 80 percent of his payload?"

"Well, no, because 80 percent of the experiments are locked up."

There is a lot of this. We as a science community felt we had lost a lot. The management, the MOD folks, and the NASA-Mir program management felt like a lot was accomplished. I mean, that's sort of like the glass half empty, the glass half full kind of thing. Given all the constraints and the difficulties of the mission, yes, a great deal was accomplished, because at a few points those guys were struggling to stay alive, so the science really isn't the first priority. Now, I think if I was to go back and look at the statistics--and John Uri's probably got a much better handle on this than I do--the NASA 5 flight probably was one of the ones with the lowest number of accomplished experiments, just because a lot of them were lost. Nobody can really blame us for that. But there always seemed to be this interest in putting the best possible face on, almost to the point where we thought it was irresponsible. You can't claim victory when victory wasn't actually demonstrated.

But we had a lot of interesting discussions with our management about how best to quantify science. Did you get 80 percent of your science? Well, what does that mean? Every experiment is different. Now, if 80 percent of the plants live, is that 80 percent return? Well, if 80 percent of the plants live but they're sterile, then it's zero percent; I mean 80 percent green things, but zero percent next generation. So how do I quantity that? That's one of those fuzzy logic kind of things.

We eventually, I think, came to a--I forget now. I know at one point we discussed trying to quantify the number of sessions for the experiments that were accomplished, and say that 80 percent of the sessions were done, or something like that, as an indicator. They kept asking, "What percent of your science was accomplished?" We'd say, "Well, 80 percent of the sessions, 40 percent of the recordings, 16 percent of viable plants," and give them lots and lots of different ways, but there's no way--I always wonder at anybody that says they got such and such a fraction of an indefinable quantity accomplished, because

there's no way to quantify an indefinable kind of a quantity.

A sleep study may be completely lost if you don't get one post-sleep measurement, for example. But you did 80 percent of the work of it, or 90 percent, or 99 percent of the work, but you didn't get this one measurement, so the rest of it is just noise on a tape then. So is that 100 percent? Is it 99 percent successful? Or is it zero percent successful? Well, they put an awful lot of work into not getting the right data. How do you quantify that?

Then different studies, I mean, one experiment you may flip a switch at the beginning of the mission and turn it off at the end of the mission, so it's running the entire time. Well, how do you compare that where you have another study where you have to draw blood samples four times a day, and spin them down, and freeze them, and then make sure the freezer stays powered so that when you get back to the ground those blood samples mean something? How do you compare those apples and Volkswagens?

So I guess probably Mike Foale's mission is the one that shows the lowest completion rate of a lot of the investigations just because a lot of them were lost. A lot of samples were lost that had been collected ahead of time. But it certainly wasn't due to a lack of effort on the part of the crew members to do as much as they could, given the difficulties of the mission. I think, although I left the program by that time, I think Dave Wolf and Andy Thomas probably had some of the most successful missions, although Shannon Lucid's and John Blaha's were also very successful, because they didn't have a lot of extraordinary difficulties. I think a great deal probably was learned from Mike Foale and from Jerry Linenger's flights about how to do missions, and so they certainly should not be minimized in terms of a wasted mission because you didn't get all the plants grown or all the crystals grown or something like that. But the science return from those two missions might be a little bit lower than we might have hoped.

Wright: I guess most importantly is that the science was able to at least carry on.

Charles: Yes. I think that's a good point, is that the science program did continue, and, if anything, we used it as an opportunity to train the MOD types into what was meaningful, and we also used it as an opportunity to train the PIs as to what was possible. The PIs hopefully won't come back with any more fifty nights of recording, and they will learn to ask for atmospheric composition measurements if they're concerned about sterility in plants. You hate to think that the program was mostly pilot studies, because pilot studies by definition are not conclusive.

But on the other hand, if the whole Mir Program in terms of science, was a pilot study for ISS, making ISS productive, then it's worth it science-wise. I know for a fact that we got a lot of independently useful productive scientific objectives met on the Mir Program as well. So it's a win-win in terms of the

science.

The heartaches are very real. I think I remember the heartaches as much as anything: the collision, the fire, clearing the decks and trying to fix problems, and debating amongst ourselves and with each other what the right problems to address were, and things like that. There's a lot of heartaches, but I think in the final analysis a lot of meaningful scientific objectives were accomplished. I hope the investigators agree, and I hope the investigators can get meaningful publications out of them, and can convince NASA and the public and the rest of the world that science is useful and meaningful.

Wright: I think this is one point that we all need to remember, just because the mission may be over but the science is not.

Charles: Well, in a very real sense, the science is not over, because a lot of the investigators are just now getting, or haven't yet gotten their final data sets. I think there is still final baseline data collection on the last cosmonauts that just landed. Then after that, NASA typically gives the investigators a year of exclusive access to the results so they can get some initial publications and claim priority, but in many cases that data is not completely analyzed for years after that just because it takes a broader view and multiple perspectives and different rearrangements of many of the results to make sense of it. So the science program is not just getting geared up, but it's definitely in full swing at this point. Phase One may be winding down, or officially over, or whatever, it certainly does not mean that Phase One science is officially over.

Wright: Paul, anything you'd like to ask?

Rollins: What brought you to NASA? With your interest and your background, I assume you could have worked almost anywhere. What made you choose NASA?

Charles: You're very generous. I tailored my education to come to NASA. From the earliest age I wanted to be an astronaut, and since I always seem to be just above the height limit, no matter what it was, I was always just above the height limit, I realized early on I was not going to be an astronaut. But I was always interested also in biology. I thought, "Well, wouldn't it be fun to be one of the docs who works with the astronauts." I decided medical school wasn't for me, but research was an area that I was strong in. So I tailored my education with my choice. I wanted to be a geologist during the Apollo days, and I wanted to be a physicist during the Skylab, and finally after that I wanted to be a physiologist. So I tailored my education to put me in the right place at the right time with the right background to get hired as a

physiologist for NASA in Houston. I wanted to come to JSC in Houston.

I'd always been a space buff. I was interested in space flight from the very earliest. I was inspired by John Glenn and the folks before that. I used to play John Glenn when I was a kid on the playground and, now, of course, I'm working on the mission that John Glenn's flying, so it's a nice fulfillment. I got to know the guy, and he's just as nice as they say he is. So that was really one my motivations, but I always wanted to be involved in the space program. I wanted to be involved in sending people, if not myself, then sending other folks into space.

Also, I'm very interested in the physiological aspects of space flight. Weightlessness has always fascinated me, and so how does the body respond to weightlessness has fascinated me, and my little niche was the cardiovascular system. It seemed to be an area that was simple enough for me to understand, so I was able to focus on it. So I just worked on getting my Ph.D. at Kentucky and working on actually a centrifuge that spun large animals like dogs, funded by the Air Force. I did some Air Force-funded studies which were relevant not only to the Air Force, but relevant to understanding cardiovascular physiology in extreme environments and helping me learn how to manage large complicated research programs, which has also been very useful here. In fact, that was probably one of the more attractive aspects of that was the fact that I could use a big piece of hardware, with a lot of folks that scurry around it, and lots of computers, and lots of surgery, and animal work as well.

Then I was lucky enough to get a postdoc here, and lucky enough to get picked up as a civil servant when the postdoc expired, and then seemed to have the right interest at the right times to show my interest and to get progressively more responsible positions. So although there's lots of heartaches and lots of headaches, and I have a very large bottle of Excedrin back at my desk, this is the kind of work I like to do. It also gives me access to the space history things, and I'm very glad Glenn's here now that we can do more of that kind of stuff, because I've been a space history buff all my life as well. So here I am in the midst of it.

Did you get a chance to go over to Building 45 and go through the archives? Did you go down to Rice and go through the archives? It's like getting paid to do my hobby.

Swanson: How open were the Russians to give you any past information from earlier medical studies, for example, the Salyut, some of the real early long-term studies? Did that crop up at all in some of the work that you've done?

Charles: That's a very good question, and that's the kind of question that we get asked a lot. The Russians are very good at publishing their results, unlike a lot of the Americans. The Russians publish almost all

their results. The Russian publications, of course, they get translated, it may take months or years before they get translated, and they have to find their way to the investigators that are looking at them. Being here at JSC finally put me in the right place to see the Russian-translated publications.

The problem we had with that was that there didn't seem to be a lot new or interesting [findings] in the Russian results. The Russians would publish lots and lots of data that we already had, like heart rates and blood pressures. They would use protocols that we didn't think were meaningful or we didn't understand. Their lower body negative pressure, this technique that I was very familiar with, never seemed to be stressful enough to provoke the responses that were of interest. They never seemed to follow a protocol that was meaningful, like they would do it twice during a ninety-day mission. Well, you don't learn anything that way. That's an example.

There seemed to be different protocols every time they flew, so you couldn't really make comparisons. I mean, this guy had this response on day forty-five, but this other guy on this other mission had this response on day eight-five. Well, what does that tell you? It tells you just generally in flight their response looks roughly like this. The Russians didn't seem to believe in statistics, and, of course, the Western medical research lives and dies by statistics. The Russians would do one subject one way and another subject another way, and you immediately had two independent variables which is uninterpretable by Western thinking, and the Russians had no difficulty interpreting it.

For example, the Russian lower body negative pressure countermeasure they do before landing. We always ask them for the studies that led them to that, and they always promised to give them to us and they never did. We eventually found something that I believe was published in the translated literature which was that study. It was four people--four, unlike Americans which require dozens to have meaningful--four people, each of whom did a slightly different variant. So one guy did an LBNP and an exercise, one guy did LBNP and exercise and fluid loading, and one guy did exercise and fluid loading, and one guy did LBNP and exercise and muscle stimulation. And the Russians will say, "Well, see, this guy did this, and this guy did this, and this guy did this. Therefore, the answer obviously is this protocol for this much time."

We would throw our hands up and say, "Well, that's how you came to that conclusion, but that doesn't mean anything to us."

So, the Russians, when they knew we wanted their data, of course, it got to be expensive. They would hold back. They'd say, "We can't find it," or, "We've already told you that," or, "It's been published," or something like that. We eventually found most of what we were looking for, I think, in the published literature that was translated, and most of it wasn't of any value, of not very much value to us.

So when people talk about the Russians having these huge databases--luckily, people don't seem to say that much anymore. I think the word had finally gotten out. The Russians have huge databases of people that flew in space and came back warm and pink. If that's what you're looking for, if you're looking for a success, the guy was alive after the flight, they're very good at that. They've got lots and lots of that data. But if you're looking at what happened to heart rates, what happened to blood pressures, what happened to certain ion concentrations with time in flight in response to certain provocation, they have almost none of that. In fact, one of the things that the Russian investigators told us is they were glad to see us come along, because they could finally start doing research in flight, because research was the lowest priority. So I think the data from the NASA-Mir Program is probably about as good as you're going to get from long-duration space flight on the Mir as anything that the Russians have published.

There have been little bits and pieces of stuff that the Russians published from their Soyuz experiences and from other flights that are good, but in general it doesn't compare to the stuff the Americans have done, I'm saying that as an American who's done some of that stuff, but we have certain standards. We have certain Western ways of looking at this. The Russian data may be very good by an Eastern way of things, and I think there's a lot of this Eastern philosophy in their medical stuff. Like the guy that was doing the carotid massage on the cosmonaut. That made perfect sense to their way of thinking, and that is absolutely the worse thing you can do by the Western way of thinking. So again, by whose rules?

The Russians think they have lots of data by their rules, and they've flown successfully. They say, "Look, the proof is in the pudding. We've flown all these Space Station missions. We know how to exercise people, we get them back on the ground, and after thirty days or so, they're fine."

We say, "Well, yes, but can you tell us why you exercise that way?"

"Yes, because it works."

"Well, how did you know it worked?"

"Well, just look. It works." That's sort of the Russian circular logic. "We do it because it works because we do it."

We say, "Show us statistics."

They say, "Bah statistics! That doesn't mean anything."

There have been a few occasions in a few areas, especially in the are of metabolism. I give Carolyn Leach Huntoon a great deal of credit for introducing the idea of statistics in the Russian space medicine program, because the folks she worked with ended up doing T-tests on some of their metabolic studies they did in flight, and that was the first evidence I've ever seen in my reading of statistics in the

Russian space medicine publications. And I know it's because she hounded them to do it, or maybe she did them for them and gave them back to them. I don't know. So the Russian data, when you could get it, was interesting, but it wasn't definitive in many cases. Now, there may be some areas where it was good or unique, but overall it was nice to have, but certainly not life or death.

Wright: I'm sure your memories will coming back and jogging your brain off and on for years, but we certainly appreciate your sharing as much as you did with us today. We wish you luck in all those new fields that you'll be going into in the next years.

Charles: Thank you very much, glad to do it.

Wright: Thank you.

[End of interview]