# ORAL HISTORY TRANSCRIPT

NANCY GRACE ROMAN
INTERVIEWED BY REBECCA WRIGHT
CHEVY CHASE, MARYLAND – 15 SEPTEMBER 2000

WRIGHT: This oral history is being conducted with Dr. Nancy Roman in her home in Chevy Chase, Maryland. Today is September 15, the year 2000. The interview is being conducted for the NASA Headquarters History Office. The interviewer is Rebecca Wright, assisted by Sandra Johnson.

We thank you again for visiting with us today. You've had such a distinguished career with NASA. We want to hear about all those times and experiences that you've had, but we'd like to start today by you providing us some of your background and how you got started.

ROMAN: Okay. To start with, I'm trying to use my double name again, Nancy Grace. I was born in Nashville, Tennessee, so I was a Southern baby, and in the South they used double names. I always used it in my family and I used it throughout college, but I went to graduate school in Wisconsin, and I found Northerners just could not cope with it, so I dropped it. Then, oh, maybe six years or eight years ago now—time flies—we had a summer student working with us at the Astronomical Data Center at Goddard [Space Flight Center, Greenbelt, Maryland], and she used her double name. I decided, well, if she used it, why can't I? So I'm trying to use it again. I will answer either way, and I have to admit, most people don't use it, but I'm trying. Okay.

Well, as I said, I was born in Nashville. I lived there all of three months. One of the interesting things is, as you probably know from your research, after I joined NASA, NASA was very new, and so it was getting a lot of publicity, and the women's pages were desperate for some female outlooks. So I got lots and lots of press coverage. One of the articles which was syndicated, actually, coast to coast, discussed my Southern accent, and all of the people in Washington were just very amused by the whole thing because I'd lived away from the South so long that I didn't really have much of a trace of a Southern accent. It was pretty clear that the columnist had not actually talked to me.

But, okay. I did live in the South for, oh, I guess, three years, not only in Nashville, but also Oklahoma City, Houston, then back to Oklahoma City. No, sorry, not Oklahoma City—Tulsa. I have to admit, though, that I was too young to really remember any of that part of the world.

We then moved to West Orange, New Jersey, just outside of New York, and I lived there until I was six, started kindergarten there. I did go to kindergarten there. Then we moved to Houghton, Michigan, which is on the tip, not the tip, but about half way up the finger on the northern peninsula of Michigan. Yes, quite a contrast from Houston. We were there four years, then went out to Reno [Nevada].

I'm not sure when or how I got interested in astronomy. I blamed my mother, which she was a little taken aback at, because her field was music. In fact, that was her piano [Roman gestures]. She really had no particular interest in science. Well, she was interested in just about everything, but she didn't have any background in science at all. She let me know, although she later denied feeling that way, but she let me know fairly subtly but definitely that she didn't think science was quite the field for a woman. As I say, I told her this when she was living with me and she said, no, that she certainly never tried to discourage me, and, as I say, I suspect she didn't realize that she was discouraging me.

But, anyway, we did live in Reno, and the reason I went into this, is, oh, yes, I started out by saying that my first experience was that Mother used to take me out and teach me the constellations. Of course, living in Northern Michigan, we had the Northern Lights, and she'd take me out and show me those. So she really got me interested in looking at the sky.

Then we lived in Reno for two years, and the second year we lived on the very edge of the town. We were the last house on the street. There were no houses across from us, an empty lot behind us, a ranch on the other side of us, an empty lot on all three sides of us, I guess, plus the ranch. So we had a really clear dark sky. I wouldn't be surprised if that had a major influence on my being interested in astronomy. I don't know. As I said, I don't really know when I started.

My usual answer is that most kids, or at least many kids, are interested in astronomy at the age of, oh, eleven or twelve, ten to twelve, and I just never outgrew it. It was as simple as that. And interestingly enough, a fair number of people who go on to major in astronomy have decided on it certainly by the time they leave junior high, if not during junior high. I think it's somewhat unusual that way. I think most children pick their field quite a bit later, but astronomy seems to catch early, and if it does, it sticks. Okay.

Then we moved to Baltimore [Maryland] and I went to high school there, and, as you know, I went to Swarthmore College [Pennsylvania]. I picked Swarthmore for various reasons. I had gone to a girls' high school. The better public high schools in Baltimore were, and still are, sexually segregated, which I think is rather interesting in this day and age. So I went to a girls' high school, and I'm an only child, so I didn't have any brothers. I was relatively new in Baltimore, as you can tell, and so I didn't have a lot of male friends. In fact, I had almost none. So I very much wanted to go to a coed college. So that was my first consideration.

My second consideration was that I wanted one that wasn't too far from home. This was, well, it was 1942 when I went to college. So it was the middle of the war, and transportation wasn't trivial.

The third and perhaps the deciding feature was that I wanted a good astronomy department, and I knew that Swarthmore had a good astronomy department. And unlike today when kids apply to a dozen colleges, I didn't apply anywhere but Swarthmore. It never occurred to me that I might not get in, even though I happened to be late in applying.

As I say, the war had come along. I was scheduled to graduate from high school in 1943, but I was in a course that was supposed to give us four years of high school plus a year of college in our four years. So the end of my junior year, I would have had enough credits to graduate from high school. Well, our class was fairly small. I think there were only twelve of us in it by that time, our home room class. Obviously, the whole graduating class was a lot larger. Some of the girls had asked the administration if we could graduate a year early. You know, with the war they wanted to get into nursing or teaching or get to college and get out faster. They felt that that extra year in college didn't mean that much to them.

So the administration said yes, we could do it under two conditions, that everybody in the class had to do it, they wouldn't split us, which was understandable; and we had to go to summer school and take chemistry. So we went to summer school and took a year of chemistry in ten weeks, which was sort of fun, because we started the second semester at the same time we started first semester, which meant that we had to cope with second-semester chemistry without any background. But we managed. We all got through it.

Oh, excuse me, I'd better get the phone. [Tape recorder turned off.]

Now, let's see where I was.

WRIGHT: Never too much detail.

ROMAN: Oh, yes. So, I, anyway, we had chemistry four periods a day, two periods of lectures and two periods of lab. I don't know. There were times when I dreamed test tubes that summer. [Laughter] But I got through chemistry. As it say, we all did, and, in fact, I passed my freshman college, first-semester freshman college chemistry on the basis of it several years later.

So then I went up and went to Swarthmore. At Swarthmore, the Dean of Women was very opposed to women going into science or engineering, so opposed that if she couldn't talk a girl out of it, she just never had anything more to do with her for the four years she was there.

So she sent me over to the Astronomy Department and I met with Van De Camp [phonetic], who was the primary, the head of the department and the primary Astronomy Department professor. I talked to him and told him I wanted major in astronomy. Well, he didn't overtly talk me out of it, but what he said was, he said, "You know, I'm using material that was collected by my predecessors fifty years ago, and I'm collecting material which will be used by my successors fifty years in the future." I realized many years later that he was trying to discourage me. But, anyway, I stayed with it.

Mother's reaction was that—well, both of my parents were supportive. I mean, they felt I should make my own decision. I'll come back a bit to high school again. But Mother said, "You know, you don't really know much about other subjects. You should take other subjects, too, before you commit yourself." There was no reason to commit myself as a freshman. So I took history. I had to take German because of scientific German, and astronomy and math. Well, I felt that the only way I got through my freshman-year history and German was that I was taking math and astronomy, and I never had to study the math and astronomy. So that pretty well convinced me that I was in the right field.

Well, I talked about the discouragement at college. High school was much more blatant. When I thought I was going to have a senior year in high school, I asked to take a second year of algebra instead of a fifth year of Latin. I'd had Latin from the middle of seventh grade on, and I felt that I wasn't that interested in specializing in Latin. The guidance counselor looked at me. My memory of, visions of her, I'm sure is exaggerated, but she seemed about ten feet tall, looked down her nose at me. "What lady would take mathematics instead of Latin?" which was about as obvious as you could get. [Laughter] I said I wanted to, but, as I say, it became a moot question when I didn't stay for my senior year.

Well, at the end of college, it was just at the end of the war. I graduated in February in 1946, and the question was, what graduate school was staffed again? Most of the professors had been off in war work, and so there was a question of where I might go. Well, Van De Camp had suggested I go to Columbia [University, New York City] because it had a fairly full staff by then, and he had talked to Shift [phonetic], who was the chairman of the department there, and I think Shift was counting on me coming.

Well, in the meantime, Yerkes [Observatory, University of Chicago, Williams Bay, Wisconsin] had gotten back to a reasonable status, and I felt, and Van De Camp didn't disagree, that it was a better department than Colombia. So that's how I happened to pick Chicago. Well, it had its consequences, however, because Swarthmore has a scheme of outside examiners, and Shift was my astronomer examiner. I did not get high honors, and I'm pretty sure, from what I was told, that I should have. I talked later to, I guess it was Elizabeth Uri, Uri's daughter—I happened to meet her on an Alumni Day or something, something that I was campus for. She was several years younger than I was, but she had known the Shifts because they'd lived near them, and she said, "Shift is that way. He has that reputation." Well, it didn't really hurt me that much, except it was a little discouraging, especially when others, Van De Camp and others, implied to me that they thought I should have.

Well, anyway, I went to Yerkes, and that was certainly an excellent choice. Yerkes was really the hub of the astronomical world at that time. There were people from all the world who'd come. They might come for a few days. They might stay several months. A couple stayed six or eight months or longer, maybe as long as a year. As you said in your e-mail, the GIs were beginning to come back, so it was a lively place from that standpoint.

There were always at least two women. I think it was when I started, there were ten students, of whom two were women. There was a Canadian woman who actually arrived either the day before or the day after I did. We shared an office for quite a while. Well, she was only there for two years because she came with a master's degree. We shared an office. She was at Goddard for fifteen years, much, much later. I had kept up with her over the years, as a matter of fact, and we'll still very good friends.

Then after she left, there was another woman who came, who did not stay. In fact, there were a couple of others that came from time to time that did not stay. But I didn't really have any problems as long as I was a student. I felt that I was treated pretty much the same way as all the other students. Well, I guess that's all I can say. I was treated quite normally and I didn't feel any problems with discrimination.

After I graduated, I stayed on there. Astronomy at that time probably, to some extent still, but certainly very strongly at that time, depended on your thesis advisor to get a job. My thesis advisor did not want me to leave, so the only jobs he told me of were jobs that he was fairly sure I wouldn't want. That had a number of problems, but that's neither here nor there. They're in the distant past. I also was paid about two-thirds what the men at the comparable level were paid. At one point I commented that I was getting less as a Ph.D. than what they now call data clerks, that they then called computers, who had a high school education and nothing beyond were getting, and I was told, "Don't look around."

At another time the chairman of the department, who was [Subrahmanyan] Chandrasekhar, who obviously had faced discrimination himself as a dark-skinned person, told me, "We don't discriminate against women. We can just get them for less." I think he honestly didn't recognize that that was discrimination. Well, there was an obvious problem. I was quite sure that I had no chance of tenure as a woman at that time. There were no—well, I'm not sure. There was a woman at Michigan who may have gotten tenure just about that time, but she and I were the first women on the faculties, even, of a research, major research astronomy institution. So it was not exactly trivial.

Cecilia [Payne-]Gaposchkin, who probably was the leading astronomer of her era and certainly an outstanding one, was on the Radcliffe [College, Harvard, Cambridge, Massachusetts] faculty for many years, but she did not get a Harvard appointment until 1957. So, as I say, the chance of getting tenure was pretty slim.

Well, fortunately, one of the other professors at Yerkes obviously recognized the problem, and he told me of a job at the Naval Research Laboratory [NRL] here in Washington [D.C.], in radio astronomy. Radio astronomy was new at that time. It was quite new in this country. I decided that it would have a lot of potential for the field of galactic structure, which was what I was interested in. It did, and I enjoyed my stay there, but at that time you were expected to build your own equipment, and I didn't want to start over as a electronic engineer, because it really would have meant essentially starting over. So I won't say I was actively looking for another job, but I was keeping my eyes open.

But in the meantime, I enjoyed working there. I did a number of interesting things. I made a map of the galaxy at sixty-seven centimeters, and as a result of comparing my results with those of another person who was working there at the time, working in much shorter wavelengths, I was able to show that the center of the galaxy was not a single source, but was a mixture both thermal and nonthermal sources. Unfortunately, though I did publish that briefly, I was not able to do much toward substantiating it because the other person would not let me publish his data.

So in 1958, when they set the center of the new galactic coordinate system, I said we should wait three years—the International Astronomical Union meets every three years—that we should wait three years, and I explained why, and they asked me to prove it, and I couldn't. So it was set on the basis that the galactic center was a single source. Since then—in fact, it was a couple of years later one of the astronomers from what by that time was the

National Radio Astronomy Observatory came to me and he said, "You know, you were right." I said, "Yes, I know." [Laughter]

Another interesting thing that I did is I participated in the first radar measurements of the distance to the Moon, and with that we were able to improve the lunar distance quite substantially, not nearly as well as it's known now, thanks to Apollo and the lunar lasers and so forth, but a lot better than it was known then. So I enjoyed it.

I went there in 1955. In 1956, I received an invitation to a dedication of an observatory in the Soviet Union, in Soviet Armenia, as a guest of the Soviet Academy of Sciences. Well, I had only four weeks, between the time I got the invitation and the time I had to leave. I found out later that the reason for the short notice was that they had invited a much more senior astronomer who had been born in Russia, and he had been with the White Russians and had escaped, and he was afraid if he went back to the Soviet Union, that they'd never let him out. So he declined.

I guess I'd better back up. So I got this invitation, and I was sort of curious as to why. It turns out that while I was still at Yerkes, I was studying what are called high-velocity stars. All of the stars rotate, have orbits around the center of the galaxy, and most of them go around the center of the galaxy in nearly circular orbits. They vary a little bit from circular, but they're predominantly circular. The sun has a nearly circular orbit. So that of the stars near the sun, almost all of them are moving pretty much in the same way as the sun. So the relative speed of the sun and all these other stars is fairly low.

But there are a few stars that have more elliptical orbits, and even a few stars that move in the opposite direction from the way most of the stars near the sun move. These, of course, because their orbits are so different, have very different speeds with respect to the sun. So we call them high-velocity stars, although in fact their velocities are actually slower. Anyway, I was studying these high-velocity stars partly as an extension of some other work I had done before.

One of the primary ways that astronomers study stars is to spread their light out into a rainbow, which we call a spectrum, and from that rainbow we can learn something about what the stars are composed of and how hot they are, how bright they are, and how they're moving, at least how they're moving toward or away from us. We can't find any transverse motion from that.

So I was taking spectra of lots of these high-velocity stars, all the ones that I could read. One of the stars I observed, according to the catalog, should look like the sun. Well, when I developed the plate, it didn't look at all like the sun. It was completely different. So I just assumed at first that I had gotten the wrong star. Well, then I took some other plates and, sure enough, it looked the same. I definitely had the right star. So I took various spectra, different resolutions; that is, the light was spread out somewhat more than the ones I took at the beginning.

Afterwards, when I got back to Yerkes—this was done in West Texas at McDonald Observatory [Fort Davis, Texas]—when I got back to Yerkes, I measured these plates and looked at what I described what I saw, and wrote a little two-page note, which I didn't pay a lot of attention to. It was not the primary thing I was working on. I just thought it was interesting enough to publish. Well, the director of this new observatory in Armenia had seen this note and was interested in it, and that's how I happened to get invited.

Well, as I say, I had four weeks between the time that I got the invitation and the time that I had to leave. Well, I was working for the Navy, still in the middle of the cold war. I had secret clearance, and I wanted to go to the Soviet Union. It turns out I was the first civilian to go to the Soviet Union after the beginning of the cold war. So, as you can imagine, if you've had experience in government and red tape, this was a bit of a hurdle, and it turned out that the only way I could possibly get the paperwork through was to essentially walk it through myself. I could not rely on channels to get things through in time.

As an extreme example, when I had everything else—well, I might add that the paperwork had to go all the way to the Secretary of the Navy to get approval for my going, so it was a major undertaking, and needless to say, by the time I was finished, people knew me at the Naval Research Lab.

Well, anyway, I was going to tell you a story about how I had to do my own walking, because I think it's an interesting story in itself. I had sent all my papers over and my passport over to the Navy Travel Office to get a visa, and they were supposed to get a visa for me. Well, a week passed and no visa. A few days more passed, no visa. So, finally, I said, "Can I come over and get my passport and get my visa myself?" Because I just didn't have time to wait. Well, yes.

So I came over. She gave me the passport. She said, "You won't get it."

So I said, "Okay, well, at least I'm going to try."

So I went down to the Soviet Embassy, and the first day they had me come in. I explained what I wanted and sit in the hall. I guess I was there about forty-five minutes. Then they took my passport, and they said, "We'll call you when it's ready." Well, that was all right. I expected that.

Well, the next morning or maybe even late that afternoon, I had a call. It was ready. So I went down to get it, and this time it was quite a different reception. I was ushered into the office of the scientific attaché, given tea, a cup of tea, and just generally greeted like a, almost a great VIP. You know, I was what, all of thirty-one? It turned out, among other things, that the science attaché had been a translator for the man who was going to be the director of this observatory, not that the man needed a translator. His English was excellent, but, anyway, he knew him and had a lot of respect for him, and the fact that he had invited me to this dedication really put me on a high peg with him. So I had a very interesting session and a very interesting general discussion with him.

Then I took my passport back to the Navy Travel Office, and the woman just about dropped through the floor, as you can imagine. She had been absolutely certain I couldn't get a visa and didn't even try. So, anyway, that was just one example of the sort of problems that I ran into in trying to get things through.

Well, after I got back from Russia, I was asked to give a colloquium on my experiences, which I did. Then probably as a result of that colloquium, I was asked to give a course in astronomy, so I gave a series of ten lectures in astronomy. That was one of the most fun things I've ever done, because the audience were engineers and scientists, so I could assume a complete background in science, in physics and math. I didn't have to into the fundamental problems, like Newton's laws and so forth or algebra. I could assume that they'd understand basic physics concepts and so I could emphasize the astronomy, and I very much enjoyed that series, and apparently other people did, too, because the attendance was pretty high and pretty steady. I say "pretty high," my memory is that it was two or three hundred. It wasn't trivial. Well, that was 1956.

Well, in 1958 when NASA was formed, a good bit of the science part of NASA was transferred from the Naval Research Laboratory. All of Project Vanguard went to NASA and a good bit, although by no means all, of the rocket program. Now, I had not had a lot of involvement with either of those of programs. Oh, occasionally somebody in the rocket branch would call me up and ask me an astronomical question or ask me for advice on astronomy, but no more than lots of other people in the lab asked me for advice on all kinds of things. So that I was not automatically transferred with the group.

If I'm not taking too long to back up about asking advice, after I'd been there a little while—well, there's another story I'll tell you being a woman, but I'll come to this one first. After being there a while, people began to decide that astronomers really had a rather interesting background in terms of, well, I guess the primary thing is that they had a fairly decent background in radiative transfer and optics, so they'd ask me questions on optics, although NRL had a pretty good optics branch. But radiative transfer turned out to be of interest to them because they weren't interested in radiative transfer, but they were interested in sound transfer, underwater sound, and problems were sufficiently similar that I'd get asked questions on that.

After I'd been at NASA, oh, several months, maybe longer, an industry rep came in and met with me, and he said, "How long have you been in astronomy?"

I said, "All my life."

He said, "Well, the last time I met you, it was in a conference on underwater sound." [Laughter] So, anyway, as I say, people at NASA knew me.

Well, I guess it was early 1959, I went to NASA Headquarters to hear Uri. They had a lecture from Uri, and I thought that would be interesting, a lecture on the origin of the Moon. While I was there, one of the men who had been transferred from NRL, who didn't know me well from NRL but knew me, asked me if I knew anyone who would like to come to NASA and set up a program in space astronomy.

Well, I debated about it because I knew it would mean leaving research, and I had enjoyed research. I also had had only one bit of experience in management, and that was not terribly successful. But I finally decided that the challenge of starting with a completely clean slate and mapping out a program that would influence astronomy for fifty years was just more than I could turn down, and I don't think I underestimated it, looking at the Hubble [Space Telescope, HST], which is one of the things that I was involved with. So I took it and in February '58 I joined NASA.

I was going to tell you about the problems of being a woman when I joined NRL. The person who hired me at NRL was John Hagen [phonetic], and Hagen had moved over to become the director for Vanguard by the time I arrived. It was a four- to six-month period between the times. When I arrived, I thought, well, they're hiring me. It's a government. They will have a job that they want me to do. So I sort of expected them to tell me what they'd like me to do. Nobody said anything. Well, I'd brought some work with me and I also wanted to do some reading to learn more about what was, to me, a new field. So I succeeded in keeping myself occupied.

Of course, at lunch and coffee times and so forth, we'd talk, and gradually they began to decide that maybe I would be useful and they began talking to me about their problems and I sort of worked into the group. Well, many years later one of the men in the group told me, he said, "You know, the problem was, when you arrived, we'd had another woman (who happened also to be named Nancy), and she was absolutely useless, and the last thing we wanted was another woman astronomer." [Laughter] And that's why when I got there, they didn't have any use for me, which is, I think, an interesting indication of stereotyping that would not have happened to a male in the same position.

Well, when I joined NASA, as I said, because the women's pages were so very anxious to get material, I got a great deal of publicity, much more, I think, than I deserved, but in a way, it was fun. As a result, of course, I had a lot of opportunities that I probably would not have had as a man in the same job.

Well, that sort of finishes that part of the story, and I think maybe it's up to you to ask some questions at this point.

WRIGHT: When you were asked to head up this new program, did you have the opportunity to set your own agenda, or were there expectations of what the government wanted you to do with this?

ROMAN: Well, it was pretty much a blank check. Now, during the IGY [International Geophysical Year], the National Academy of Sciences had asked scientists, asked all scientists, not just astronomers, to propose satellite experiments. There had been four proposed for astronomy, only one of which could possibly have been carried by Vanguard, and probably not even that. So when NASA was formed, they decided to use these as the basis for the astronomy program, at least in the beginning.

By the time I arrived, which was six months after the organization had been formed, they had gotten these people together at least once, I think. I think only once. They got them together very shortly after I arrived, and they pretty well decided that, as I say, that these four experiments would be used as the starting nucleus of the program, and not only that, but that they would build a common spacecraft, which became the Orbiting Astronomical Observatory, for these experiments.

They were quite different from one another in many ways, but they all required accurate pointing, or at least by standards of those days, accurate pointing, and the ability to point to any place in the sky.

The first year I was at NASA, as you can tell by my résumé, I was only responsible for optical and ultraviolet astronomy. Frankly, there wasn't much else. But the person who was supposedly head of the astronomy program was Gerhardt Schilling [phonetic]. Well, Schilling was a pretty good manager, and I learned a lot about management from him. I don't think he was a particularly good astronomer, so I think from the standpoint of doing much with the program, he really hadn't done much.

So there were two things that had been started. I think the second one had been started. One was the OAO, as I told you. The other was the Orbiting Solar Observatory, which was the brain child of John [C.] Lindsay at Goddard. He had proposed a satellite in which most of the weight would be in a wheel that would rotate relatively rapidly and form a gyroscope. Rotating against that wheel were two boxes that could remain pointed at the sun, to a reasonable degree of accuracy. Now, these boxes couldn't look anywhere else in the sky. Well, let's put it this way. They presumably could have looked anywhere in the plane of the wheel, but they couldn't look away from the plane of the wheel. So they were restricted to solar observations. But they came along, as it happened, appreciably earlier than the Orbiting Astronomical Observatory, primarily because the pointing was a so much easier problem. All they had to do was to find the brightest source in the sky and lock on it and then have the wheels start spinning, and, well, the wheel probably was spinning because of the spin of the rocket launcher. So that was reasonably under way by the time I started, but that was all. There was nothing in any region of the spectrum other than the ultraviolet.

So my job in the early days was to get things started in primarily high energy. I also started the radio astronomy program. Infrared I didn't do much with at that time—I did later—because the detectors just were not available. Clearly, we knew the infrared would be interesting, but the only thing we had in the way of detectors were barometers and they just were not sensitive enough to do much science. So it was the other wavelengths that we paid attention to.

WRIGHT: What about staff and budget?

ROMAN: The way NASA Headquarters was set up and still is, actually, as you may know, is a dichotomy between engineers and scientists. For example, in a particular division, like the Astronomy and Astrophysics Division, there would be either a scientist and an engineer as the head and the other as the deputy. Below him—it was always a "him" in my case—there were two groups. There were the scientists and there were the engineers. Now, they worked very closely together, but they did have separate responsibility.

In general, it was the engineers who worried about the project budgets, and I worried about what we called SRT. I worried about the support of the university participants—well, not only universities, but any of the scientific participants. I had full responsibility within the normal limits for their budgets and deciding which of them to fund for what, that type of thing.

As I say, I didn't have responsibility for the detailed planning of the project budgets. Actually, the detailed planning of the project budgets was done by the NASA centers, the center that had the responsibility for the mission. But the engineers at NASA Headquarters oversaw what the engineers at the centers were doing. I pretty well oversaw what the scientists were doing, whether they were at the centers or outside of the NASA community.

WRIGHT: At what time in your role were you able to bring ideas for new projects and help see those projects become reality?

ROMAN: Well, I don't know if I can think of a particular time. I'd had a lot of discussions with members of the astronomical community about what they might want to do. I did an awful lot of traveling in my early years, trying to visit all of the major astronomy departments in the country. I also visited industry but that was in a different role primarily. And talking, trying to get them interested in doing astronomy from space, whether from rockets or balloons or satellites, telling them what the possibilities and constraints were, finding out what they thought would be important. Then on the basis of that, I tried to formulate the science program that would make sense and what sorts of facilities we needed to carry out that program.

I can't say that I can give you an exact time when I would say, "Okay, now's the time to build such-and-such a satellite, and this is why we want to build it, and this is how we want to build it," but it all sort of developed along gradually, I guess is the way I should put it.

WRIGHT: I guess it's a process of evolution. It just keeps moving from one to the other.

ROMAN: Yes, of course, which is the way science works, in general.

WRIGHT: Could you give us some ideas of how that happened? As you mentioned when you first started. Maybe take an example of one of the projects and show us that phase, that evolutionary phase of how it moved from gathering information from your travels and then moved into projects that became—

ROMAN: Well, I can tell you about a fairly minor one, because, as you probably know, I'm writing a history on space astronomy, and one of the people I contacted was a person who had done the red shift, the rocket red shift experiment. He sent back some fairly extensive comments which were sufficient to refresh my memory on what had happened, which I would not have remembered otherwise.

But early in the space program, yes, I guess really quite early, Robertson, who I think was then at Stanford [University, Palo Alto, California]—unfortunately he died fairly soon and quite young—had held a conference on relativity and tests of relativity, with the primary emphasis on space tests of relativity. I had gone to that conference, so I had some idea of what sorts of things looked interesting.

Another person who had gone to that conference was Bob Visseau [phonetic] at Harvard, was Harvard and not MIT [Massachusetts Institute of Technology, Cambridge, Massachusetts]—I think it's Harvard, I'm pretty sure. He was working on clocks. He was not an astronomer. He was a physicist, and his field was atomic clocks. Because of the fact that clocks obviously have a major role to play in relativity, he had gone to this conference, and I talked to him about the red shift experiment. Actually, I talked to him about the possibility of flying a clock in a satellite to get into a lower gravity field to compare the timekeeping in the satellite to the timekeeping on the ground.

Well, nothing came of that very soon. It was an expensive proposition, and even in those days, when money was relatively flush, there were limits. We went to another meeting, which we both went to, and he said, "At that meeting you asked me to think about what I might be able to do with a high-altitude sounding rocket. You told me about the capabilities of the four-stage Scout rocket. After that, I went home and began to think about it and realized that we could do an interesting job with the sounding rocket." I can even show you. I have his e-mail if you're interested.

So I went to my supervisor and told him that I'd like to make a proposal, and he encouraged me. So I proposed to NASA to do this experiment in a Scout. Of course, it did go on. So, I don't know, does that answer your question as to how things happen? It's sort of a mixture of propositions.

WRIGHT: Yes. Well, sometimes, and many times, people will read, or I will read, the end of a result, but the progression of how it got there is somewhat of a mystery to all of us. It always helps to—

ROMAN: I think I told you about the OAOs, that they grew out of the IGY. The major decision was to create a standard spacecraft. Well, there were four experiments in the IGY, proposed for the IGY: an ultraviolet photometer; a relatively low-resolution ultraviolet spectrometer; and a high-resolution ultraviolet spectrometer, all for stars or celestial sources, which were assumed to be stars at that time; and one for the sun.

Well, as we began to look in more detail at the planning, it was pretty obvious that the solar experiment was not going to be sufficiently compatible with the others, to use the same spacecraft. The thermal problems were too different. For that matter, the pointing requirements I mentioned earlier for the sun were less than they were for the stellar experiments, so it would have been overkill.

So it was decided to save that experiment for a new series of solar satellites called AOSO [phonetic]. Well, AOSO never came to fruition. Probably the follow-on to AOSO, or the first follow-on, was the ATM [Apollo Telescope Mount] mission using the Saturn as the laboratory, and that, of course, did a magnificent job. So that particular solar experiment never got flown, but the three stellar experiments remained in the program. There was a fourth one, and that was one to do a TV survey of the sky and the ultraviolet.

Well, the TV survey and the photometer were both small compared to the others. They weren't exactly small, but they were a lot smaller than the others. So we decided that we could put one on one end and the other in the other end of the OAO. The OAO was basically a tube surrounded by a structure which contained all of the satellite equipment, all the telemetry, the power supply, the electronics generally, and so forth. So you had an open tube surrounded by all of this other stuff and you could obviously have one experiment pointing out one end of the tube and one out the other end of the tube, and that worked actually quite well.

Well, there was more to that and, again, I don't know how much detail you want to go into. Unfortunately, the TV experiment turned out to be a lot more difficult than was anticipated. TV tubes were clearly available. Cathodes were available, but melding them turned out to be very difficult. So by the time we were ready to fly the first OAO in 1965, the TV experiment was not ready to fly.

So the question was, what do we put in the other end of the tube? Well, we had been supporting some rocket work at Lockheed [Aircraft Corporation]—wasn't Lockheed Martin then—and so Phil Fisher [phonetic] said, yes, he could adapt his rocket payload to go into the telescope. Explorer 11 had been a gamma ray satellite. [William L.] Kraushaar said, yes, he could adapt that instrument. There was a prototype. He could use that to fly.

The prototype had been on exhibit at the World's Fair in New York outside. I had seen it the preceding summer, and I was shocked at its condition, but it turned out that the damage was only superficial, and it took very little work to get the experiment back in working order. So we flew that experiment in the first OAO. Well, the first OAO didn't last long enough to get any scientific results, unfortunately. If it had, we might have discovered gamma ray sources a little, well, not earlier, because OAO did not fly till the rocket experiment discovered Scorpio X-1. But that's another indication of program planning, of how you react to an emergency.

Okay, your turn.

WRIGHT: Funding. Has that ever been an issue to continue while you were in position?

ROMAN: Oh, very definitely. Yes, the early days, funding was pretty flush. Even by the late sixties, well before the Apollo Program ended, funding was becoming a major problem. Most of the missions—well, maybe not most—most of the big missions had to be de-scoped to save funds. I say, "save funds." Save funds temporarily because the HST is either a horrific or an excellent example, depending on which way you want to look at the thing. We had to cut the costs.

Well, we did various things to cut the cost. We started with a three-meter telescope, which is what the astronomers wanted. In order to cut the costs, we cut down to 2.4 meters, and that had a couple of advantages. It saved building new test chambers. It meant that the spacecraft could be redesigned into a system that would make pointing easier and maneuvering easier, and it meant various other savings.

But even after that was done, the budget was still higher than NASA felt they could ask for, that they didn't think Congress would approve. Well, then the question was, what do we do? Well, the only thing we could do—well, I guess we sent it to Congress that way. I don't think we did any other de-scoping, because we did look at the question as to whether we could go to a smaller mirror.

One of the main things that the astronomers wanted to do was to determine to so-called Hubble constant, the ratio between the velocity of recession of a galaxy and its distance. Unfortunately, we can't do that from nearby galaxies because all of the galaxies relatively near the sun are in what is called a local group and they all interact gravitationally, so their velocities are like the velocities in the solar system where they're all dependent on one another. They weren't separate, so there was no way we could use the nearby galaxies to determine the Hubble constant.

So the first real group, the closest group, that we felt we could use to determine the Hubble constant was the Virgo cluster. The way we get the distance to a galaxy, or at least the nearer galaxies, is to look for a type of variable stars called sephiads, which vary in intensity in a period which is directly related to their absolute brightness. So once you can find a sephiad, can measure its variation with time and long enough to get its period, you know exactly how far away it is, or at least pretty well how far, how exactly in terms of distances to galaxies, not exactly in terms of the nearest stars.

So the feeling was that unless the Hubble could get light curves, could find sephiads and get their light curves in galaxies in the Virgo cluster, that it was not worth doing, and that put a limit on how small we could make the mirror. We decided that 2.4 meters was usable, but anything smaller would not be. So we stuck with that and we pretty well stuck with the program as it was. But what happened was, it was too expensive for Congress, so they stretched it out to the next year, and it went a couple of years that way.

Well, this whole time we had a marching army being paid, so we didn't keep down the total cost of the program, but we kept down the annual cost. Well, then *Challenger* came along and, of course, that killed everything that used the shuttle for several years. By that time we were ready to go. So we essentially had this beast in storage for three years while we maintained all the engineers that were working on it. So a lot has been quoted about the price of the HST having grown so much and, of course, it did.

There was also a major inflation in that period, which added to the cost a great deal, that we had no control over. So the cost actually tripled over our original estimate. But of that tripling, almost all of it, or at least a very major fraction of it, was the result of, (a), stretching out the program to keep the costs down, (b), the *Challenger* disaster, and, (3), very definitely the inflation. So I don't really feel that the project should take the blame for any of those things. I won't say there were no cost overruns, but they weren't major compared to the other problems.

WRIGHT: Has that project resulted or lived up the expectation that you wanted—

ROMAN: You haven't heard about the Hubble? [Laughter]

WRIGHT: Not from you. [Laughter]

ROMAN: Yes, I think it has more than lived up to the expectations. I think we have done everything that we said we were going to do, and more.

WRIGHT: It must be very exciting to see that for you and all of your colleagues to watch that.

ROMAN: One thing I ought to tell you. I think I've mentioned this in other interviews, but I still find it interesting. In the early days when we were still trying to get approval through Congress, [William] Proxmire—you might probably remember him—asked why the average American taxpayer should want to pay for the Hubble. It was then the ST, Space Telescope. My answer was—and this was all by mail; I didn't ever testify in Congress, fortunately, I think—my answer was that for the price of a night at the movies every taxpayer would receive fifteen years of exciting scientific results.

In the first place, I don't think my prediction was wrong. In the second place, I recently figured whether that statement was still true, and it is. In spite of the increase in cost of the Hubble—other things have also gone up—it's still true that the average American taxpayer is getting fifteen years of exciting science for the price of a single night at the movies.

WRIGHT: And hopefully they will continue to learn more and more about those results as well.

ROMAN: Oh, yes, I think so.

WRIGHT: When you were discussing earlier the first days and then, of course, the days that led up to the Hubble, it made me think about the different type of spacecraft or vehicles that your telescopes have used to get where they need to go. How did that change, just that piece of technology, affect how you planned?

ROMAN: Well, of course, in the seventies when all the NASA effort was being put on the space shuttle, it was decreed that all missions would be launched on the shuttle. So all of our spacecraft were designed to use the shuttle. Now, the Space Telescope actually had been planned to use a manned launch and manned maintenance in orbit from its very beginning.

I guess the first suggestion of space telescopes goes back much further, but the first serious planning after the beginning of NASA, if you can call it serious planning, was in 1962 at a National Academy of Science Summer Science workshop. At that time there was serious discussion of a three-meter telescope which would be man-maintained in orbit or on the Moon. There were discussions of low Earth orbit, low Earth orbit with the telescope attached to a space station, high Earth orbit, and the Moon. At that point there was no particular thinking about one or the other.

I felt that before we'd launched OAO that we were a little bit too premature to go into a three-meter telescope, and I'm afraid I didn't do anything about it. But in the 1965 National Academy summer study, the push for such an instrument was so strong that in spite of the fact that I thought it was a little early, we still hadn't launched OAO, and I don't think they'd launched even the unsuccessful one. I know we hadn't launch a successful one. I felt it was still pretty early, but, okay, it was clear that it was going to happen.

So I started the planning, and, as I say, that from the beginning was planned as a mission that would be man-maintained and man-serviced in orbit and, of course, with the shuttle. When the shuttle planning came along, it was an obvious payload. However, everything was to go on the shuttle, even relatively small missions. So when *Challenger* came along, most of those were taken off. Those that could be launched by expendable launch vehicles were taken off the shuttle and, as a result, many of the missions had to be redesigned. The IRAS [Infrared Telescope in Space?], for example, and COBE [Cosmic Background Explorer] both, particularly COBE, had major redesign problems as a result of being taken off the shuttle. I think IRAS had originally been on the shuttle. I'm not sure about that. I know COBE was. So from that standpoint, the change in launch vehicles was a pretty major effect.

Other than that, I think the main growth in the capability of expendable launch vehicles was the fact that we could go to larger spacecraft. Now, that didn't mean that all of the spacecraft had to be larger. We're now launching relatively small ones. But certainly the HEAO, the High-Energy Astrophysical Observatories, would not have been possible without the more major capabilities that had grown up in the ten or fifteen years before it was launched.

WRIGHT: Prior to the shuttle, of course, you had different spacecraft that were going. I know that you mentioned the ATM earlier. Could you tell us about those early days of working with that?

ROMAN: Well, ATM wasn't exactly early. At the end of the Apollo Program, there were Saturns that had been left. You know, originally they had planned two more missions to the Moon than they actually flew. So those vehicles were left, and the idea was, well, what could you do with them that was useful? There were suggestions of filling the Saturn, the last stage, with sand for technological tests. Well, some of us, some people didn't think that made a whole lot of sense when you could use them to get things into orbit, in which you'd learn more than you'd learn from a tank of sand, and at the same time would get some engineering experience as to how you launched things. So that is the origin of ATM.

By that time, I was not handling the solar program, so I can't give you a lot of details as to what happened. But basically we looked at the things that the solar physicist or solar astronomers, whatever you want to call them, wanted to do, and clearly they wanted high-resolution spectroscopy. They wanted a coronograph, which blocks out the main light of the sun and lets you see the outer atmosphere of the sun without waiting for an eclipse. A coronograph is basically an artificial total eclipse. They wanted an X-ray telescope that would let them see where the X-ray radiation from the sun originated. What else did they want? Those were the main things they wanted, a white-light coronograph, a high-resolution ultraviolet spectrograph, an X-ray imaging telescope, and an X-ray spectrograph. With the capability of being carried on the Saturn, they could build fairly large instruments, get high resolution in the spectra and reasonable resolution in the imaging. So they put those together into the ATM payload.

Now, it was really an early space station, because the Saturn can was made into a residence for the astronauts, and I think it was a six-month mission. It was quite a long mission, much longer than any others that we've had. So it was very productive scientifically. It was maybe more expensive than sand, but we thought the return was worth it.

WRIGHT: I think so, and maybe those taxpayers would have something more than sand, you know, speaking of return, and Senator Proxmire as well.

The evolution from the idea to the reality for the project is one thing that we certainly have talked about, but the evolution of your job, was that also happening, what you started doing when you first joined NASA compared to what you were for those years?

ROMAN: Well, yes, I mentioned that in the beginning I was only responsible for the stars and I was only responsible for the ultraviolet optical. Then after the first year, I took over the sun as well and the whole electromagnetic spectrum. It's always amused me, [Homer E.] Newell defined astronomy as the study of where you aren't and then proceeded to add geodesy to it, which is, of course, the science of the Earth. [Laughter] So I was given the responsibility for the geodetic program, probably because the techniques of geodesy are much closer to those of traditional astronomy than they are to the space physics types of activities. Of course, relativity just sort of naturally fell in, primarily because nobody else wanted it. So I had really everything outside—well, I was going to say everything outside the solar system plus geodesy, and I guess relativity really is within the solar system, too.

But then as the program got larger, pieces of it were split off. First, the solar program was split off into a separate program. Then the high-energy was split off into a separate program. EUVE, the Extreme Ultraviolet Explorer, was just beginning when I left NASA, and I don't really remember if that was part of my program or not. I certainly was involved with it, but I don't remember whether I had responsibility for it. But everywhere from that through longwave radio remained my responsibility, if it was outside the solar system.

WRIGHT: Goodness, you had quite a galaxy of your own to take care of.

ROMAN: Yes, I did, and, as far as stuff, I had a secretary the whole time, or least the branch did. The first year, as I said, I worked for Schilling. If I remember rightly, the second year, and I don't really know for how long, I was alone except for a secretary. Then I had an assistant. Then after three or four years I had a full-time assistant, first an astronomer and then an engineer, actually, because I couldn't find an astronomer, and I hired Nancy [W.] Boggess, who had a Ph.D. in astronomy, but was a mother and didn't want to work full-time. So she worked, I think, half-time, but her hours were such that she could stay home till the children left in the morning and be home by the time they came home in the afternoon. So it worked out well for her and it worked out well for me. As her children got older, she began working more, first three-quarter time and then full-time, so that by that time I had two full-time technical people working with me, by the time I retired. Nancy actually handled most of the astronomy program.

I think it was the engineer first and then the astronomer, because it was Ernie Ott [phonetic] who was an engineer and then Jeff [Jeffrey D.] Rosendhal who was an astronomer, who later left and went into—I don't know whether he went to the Education Office directly or not. I think so. I hired Ed Wyler [phonetic] then to replace Jeff. Then when I retired, Ed had been there a year and I felt quite comfortable leaving things in his hands.

WRIGHT: I'd like for you to elaborate for us and explain for us a comment that you've made often or part of your conversation that you've made while we were sitting here, and it goes back to what the astronomers wanted. I find that very interesting and would like for you to talk to us about that, on how you determine or take what the astronomers wanted and where this group of astronomers came from that basically shared with you what they wanted. And if you don't mind, while you think about that, I think we're going to change the tape out.

ROMAN: Okay, fine. I wondered if you wouldn't have to some time along the road.

WRIGHT: We're going to change that out and let you think about that question for a second.

[Tape change]

ROMAN: Okay. Well, there were various ways that I found out what astronomers wanted. As I mentioned, I did in the early days make an effort to visit all the major astronomy departments in the country, talked to them about the possibilities of doing things in space, and tried to interest them in participating. The ones that showed an interest in participating, I simply tried to find out what they thought made sense and what sorts of things they'd need to do what they want to do. Now, obviously some of the things were impossible. Some of the things were possible, but not with the current technology and so forth. Many people were just not interested at all.

There was a major split, I'm tempted to call it the East Coast-West Coast split, although it's not strictly that. But the West Coast astronomers had their major ground-based telescopes, and they weren't really terribly interested in going into space. Now, they've changed. They're taking an active role in the Space Telescope, but at that time they were rather opposed to NASA's astronomy program. They felt it was likely to take money away from them, and they were a little afraid of that.

Actually, that brings up something which is probably worth mentioning, although it's also in the literature, and that is that [Alan T.] Waterman, who was the Director of the National Science Foundation [NSF] when NASA was formed, was worried about the NASA astronomy program because the NSF had been the prime funder of ground-based—of astronomy, period, because there wasn't anything but ground-based except for a few rocket programs at NRL, well, in DOD [Department of Defense] generally. So outside, for civilians, NSF was the fund source for astronomy and he didn't want to lose the program.

So he and [T. Keith] Glennan signed a memorandum of understanding that NASA would not support ground-based astronomy, that that would be an NSF responsibility. And that actually proved to be a problem at times because it was clear that there were areas of ground-based astronomy, particularly theory and laboratory work, that were needed to interpret the space observations or even to plan the space observations. Yet there was a difficulty. NSF wasn't interested in funding them because they weren't necessarily the types of things that would attract the highest enthusiasm from others in the field, and they had to be funded.

It was only a problem relatively early, but we had to argue that we had to be able to fund these areas in spite of the memorandum of understanding. I guess the primary way we got around it as a problem was that I had very close relations with the astronomy people at NSF. We'd call each other up on the telephone and say, "Look, this is something we would like to have," or, "Should we go ahead and fund it or do you want to?" and they'd call me up and say, "Well, we have this proposal that really looks like you should be funding it," that sort of thing. So there was a sufficiently close coordination and cooperation at the working level that we could get by. But it did, at first, cause a problem, as you can guess.

Well, anyway, let's come back to how I determined what was wanted. A second source of information was the Space Science Board. The Space Science Board had panels on astronomy, I guess eventually different aspects of astronomy. I don't remember for sure when they were subdivided. They'd meet and do long-range planning particularly. You may or may not be familiar with the fact that in astronomy, every decade, starting with 1960, the National Academy of Sciences has produced what they call a decade survey in which they discuss and list the projects in astronomy that they think should be done in the next years.

The first one, the 1960 one only, only concerned ground-based astronomy. But after that, they included space astronomy together with the ground-based astronomy in their decadal reports. Then, as I say, there was a Space Science Board that studied only space.

Probably the single most important source of interaction with the astronomical community was something that, I don't think originally, but fairly soon was called the Management Operations Working Group for Space Astronomy, or the Management Operations Working Group for Astronomy. NASA had something they called the Space Science Steering Committee, which was an in-house NASA committee. Under that, they had various of these management and operations working groups. They were basically subcommittees, but if you called them a committee, they had to be open to the public, and so the name was changed, even though the function wasn't.

So I had one which I chaired. Now at the present time they have something similar, but it's chaired normally by a non-NASA person. But I chaired this committee. We'd meet every few months and discuss the program. We'd look at what was going on. We'd look at the development of projects. We'd look at what areas of observations or experiments would be likely to be the most productive with various projects. And we'd look at what we needed to think about for the future.

At first that committee actually selected proposals for flight. Later, we went to a system where we selected for flight by ad hoc committees, or ad hoc groups. But the system was set up fairly early to use peer reviewers to select experiments for flight. Now, for what we called the Supporting Research and Technology Program, the SRT program, in the early days there was relatively little peer review. I say "relatively little." Some proposals I would fund or reject by myself, if I felt that I understood them well enough and had enough background to make a good judgment, and there wasn't a whole lot of competition in the field that I thought needed development. But relatively soon I would send them out to two or three people for reviews, and then make a selection on the basis of what I heard, learned, plus what I felt by myself.

Now I think the system, as I understand it, is that they have an assembled review of SRT proposals, just as we used to have for flight proposals, and the selection is pretty well made by that group, rather than by the program chief.

WRIGHT: Were you able to submit some of your ideas or your—

ROMAN: Well, I never submitted any. Well, in the first place, I think it would have been a severe conflict of interest for me to submit any, an actual proposal. But even beyond that, the job at Headquarters was sufficiently demanding that it was just about impossible to do research. When I took the job, I was told that we would have one day a week for research and every seven years we could have a sabbatical. But to get the one day a week for research, you had to work two other days, and to get the sabbatical, you had to find somebody who really wanted to come for a year, which at that time was pretty difficult. Now it doesn't seem to be so difficult, but at that time it was. So I never got either one of those.

I did a few rather routine types of work while I was at NASA. The first thing I did, I wrote a chapter on high-velocity stars for a compendium. I had promised to write that before I went to NASA, and it had gotten delayed many times. That's one of the hardest things I ever did, because settling down to really concentrate on a field, review the literature, and so forth took a different kind of concentration from the Headquarters activities in which you very rarely had time to just to concentrate for more than five or ten minutes on a particular subject.

But I did go to McDonald [Observatory] and observe for a week and took spectra of stars that Hanais [phonetic] had gotten from Gemini. So I had ground-based classification for his, the stars he'd observed in the ultraviolet spectra. For a while I spent some time at Goddard occasionally, classifying plates that I had taken before I went to NASA, and finished that job. So I did a little bit of research, but it was pretty routine. You know, it was fairly mundane. It wasn't state of the art by any stretch of the imagination.

WRIGHT: The astronomers that you have referred to, were there many and did that number change as the years moved on?

ROMAN: Yes, it changed in two major ways. In the beginning, there weren't many. As I mentioned, there were only four proposals in the IGY. Beyond that, when you went into the X-ray and gamma ray fields, you were not dealing with traditional astronomers; you were dealing with physicists. So a fair number of physicists came into the program, for two reasons. One, because they had the techniques. They knew the techniques to work in those regions, which traditionally trained astronomers did not. But the second reason is that nuclear physics was ramping down, and they were looking for new things to do.

So there was a very large influx of high-energy physicists into the astronomy program. The major influx was probably in the late sixties or somewhere in earlier. Unef [William L.?] Kraushaar at the University of Wisconsin, for example, was responsible for the first gamma ray satellite, Explorer 11. I don't remember the date. I have it. It was obviously, as you can tell from its number, pretty early. It was not a terribly successful satellite, but that was simply because of the problems with gamma rays, not with his instrumentation. In fact, he later flew a gamma ray experiment on OSO, on the OSO wheel, which contains experiments as well as spacecraft implements, and got some very interesting results.

In the meantime, well, of course, Giaconi [phonetic] and the X-ray group had come in from physics, very definitely. I mentioned that Visseau [phonetic] was a physicist, although he'd come in a very different part of physics.

So, yes, there was major expansion, and certainly the astronomical interest in the program grew. On the instrumentation side, the growth was primarily from the physicists. Now, where the astronomical participation in the program grew was with the International Ultraviolet Explorer [IUE], because that could be operated pretty much the way you operated a ground-based telescope. You applied for time in the same way as you applied for time on the ground. It was open to everybody. Good proposals were approved as long as there was time available, let's put it that way. I wouldn't say that all good proposals were approved.

So the astronomical community became very comfortable with space, working with space, as the result of the IUE. Of course, Hubble is benefiting from that experience very definitely, even though it's not nearly as simple to operate as the IUE was.

The IUE, according to one report that I've read, was used by more than half the astronomers in the world, which is an incredible thing. There are well over, I think it's 3,600 papers, refereed papers that have been published on the basis of IUE data. IUE is a satellite which, I guess, of all the spacecraft I've been involved with, I feel the proudest of. I think, although I've had people contradict me, that Hubble probably would have come along eventually, even without me, because there was enough enthusiasm for it in the astronomical, in at least parts of the astronomical community and parts that were fairly influential, that it would have come. It may not have come as soon. It would have had different problems, but I think it would have come eventually. IUE, I don't think would have. IUE was probably the last program that I could get approved in spite of opposition, and there was a lot of opposition for a couple of reasons.

The primary opposition came from the high-energy people who fell that it was competition for funds. But the other problem came from the fact that the U.S. participation was from Goddard. The astronomical community, the university community, has never really accepted the scientists in NASA centers. So putting those two things together, it was a real struggle to get that into the system. So, as I say, and particularly in view of the results, that's the program I feel most proud of.

WRIGHT: That's great.

ROMAN: I sort of worry that the present system, which puts so much responsibility into people who really do have a conflict of interest, even though it may not—you know, they may not be proposing a competing experiment for a particular mission. They do have a conflict of interest, and I guess I feel that that's dangerous.

WRIGHT: Let me take the other side of that. What was probably the most challenging part of your job? You mentioned that IUE was probably one of the proudest things that you were able to accomplish.

ROMAN: Well, I guess it was also the most challenging in many ways because I had so very little support.

WRIGHT: Yes, the difficulty of that. Did the continual change of technology, just how different that we do things in the seventies compared to doing things in the fifties, and how technology progressed, did it affect your decisions or did that have a—

ROMAN: Well, it certainly affected what we were able to do in a major way. One of the things I did because of this history project is, I re-read Smith's book, and I was interested to read something that I had forgotten. Apparently I was almost entirely responsible for the fact that we're using CCDs [charge coupled devices] for the wide-angle camera, the wide-field planetary camera on the Hubble. That was, in fact, the first use of CCDs in astronomy. Now CCDs have just completely taken over astronomy since then. But I just did not feel that the intensified Vidicon, which is what most people assumed we'd use, was going to do the job. It looked to me like the CCDs should, and they have, obviously.

WRIGHT: So much that changed, I know people have a hard time trying to decide what day they're going to buy their computer because they know the next day it's going to be better and cheaper. I was just thinking about that when you were talking about how so much change, you know, of course, you have so much, and, of course, the *Challenger* stopping things, and you had technology still progressing, how much did you have to go back and redo before you could do what you wanted to do?

ROMAN: I don't remember too much of that happening, actually, in spite of the *Challenger* redesign. As I say, the Space Telescope was pretty well set by the time, oh, it was pretty, reasonably well designed by *Challenger*, and it certainly, yes, it was, yes, I said, I guess I said it was almost finished at the time of *Challenger*, so you couldn't do much about that.

In the case of COBE, it was primarily simply a matter of a different spacecraft. I don't remember that technology had a major role in the changes there. In the case of the Compton Gamma Ray Observatory [CGRO], it was launched by the shuttle, but it was originally designed both to have some maintenance in orbit, although because of budget, a lot of the maintenance was dropped before launch, I mean, before it was built. It was just decided that it was too expensive to put in that many maintainable components. I'm told that they felt that maintaining it in orbit might have been rather dangerous, although there were things they could do. I don't know whether they could take care of the gyro or not. Probably not. But the thing that is striking is that it was designed to be recaptured by the shuttle and taken back to Earth, and that was some budgetary decision.

WRIGHT: That's interesting.

ROMAN: Yes, and sad.

WRIGHT: Yes.

ROMAN: Because it was working well and was certainly a good satellite, good set of instruments getting good data.

WRIGHT: You retired, I believe in 1979. When you retired, did you feel like the goals that you had set for yourself and for your program had been accomplished?

ROMAN: Yes, I think so. I had my mother with me by that time, and she was getting older, and it was obvious—well, there were a number of reasons for retiring, but it was obvious that it was getting pretty difficult to take care of the apartment and her and a very demanding job with a lot of travel. I'm glad I did retire when I did, because I could not normally—it was an early-out period. I normally could not have retired for another six years, and by that time I could not have handled things at all. I also felt that I was tired of the job, and I had looked for other jobs, but there are not that many jobs in the government at my field and level.

I did look at Goddard, but because I would not support having the institute at Goddard, the Goddard Science Group was against me, and there was no way that I could get a job at Goddard. Now that changed, but that's a different matter. I guess those were the two main reasons why I should have retired, but I'm glad I did.

But it was rather interesting, I went in to Morton, who was the division chief, on Thursday. The early-out period ended that Friday. I went in to see him on Thursday, and I said, "I've got to leave Headquarters. I either have to go to Goddard or I have to retire." At that point I wasn't sure which I wanted to do.

He said, "Well, I'm going to California this afternoon, but here is my number. Call me in the morning with your decision." This was, I don't know. Well, it was on Thursday. I don't remember what time of the day. So I called him on Friday and told him that I had decided to retire. He, in the meantime, had arranged for a job at Goddard for me, which I thought was rather interesting in the time. But I decided that retiring was probably the thing to do, and, as I say, I've never regretted it.

As you know, I continued to work half-time up until a little less than three years ago. I have been connected, well, I guess I worked with more, no, I guess I hadn't, didn't work more than half-time. I've been connected with Goddard that whole time. I worked first, well, actually, the first thing I did was to consult for one of the "beltway bandits" and primarily on the Space Telescope, so it was not a complete break. I stayed with them—I did various things for them. I did a little bit on what is now the Chandra. It was then AXAF [Advanced X-ray Astrophysics Facility]. I did a—well, basically managed a study on the—also, Space Telescope—the cost of supporting the observers for the Space Telescope, not just the institute but, not the institute—that was separate—but the people who were going in to use the telescope, what sort of support they would need, how many we can expect, what the astronomical community was likely to be by the time the Space Telescope was launched, that type of problem.

I did two studies of the use of geodesy, use of space techniques for geodesy, primarily to compare the relative cost-effectiveness of different techniques. I did miscellaneous other things for them. Then they dropped the Goddard contract and I went over to McDonnell-Douglas [Corporation], which had picked it up. Did a little bit on Space Telescope, but primarily there I worked on the Earth observation system. I found that the instruments for looking down are not all that different from the instruments for looking up. So I worked there on a part-time basis.

But in the meantime I hadn't gotten enough work with ORI, the beltway bandit, to satisfy me, and I was aware of the Astronomical Data Center at Goddard. So I thought, well, I'll see if I can get a job there. So I went out there and I said, "Look, I know astronomical catalogs. If you'll teach me computers, I'd like to work for you," and I got the job. So I started working one day a week and then gradually worked more as I was getting less, really getting less at McDonnell-Douglas. Up until then I probably was working pretty well for ORI. Finally after the McDonnell-Douglas ended, I started working half-time entirely for the Data Center. So that's it.

WRIGHT: Could you explain a little bit more about your role there at the Data Center, what you did?

ROMAN: Well, the Data Center archives computer-readable versions of astronomical catalogs. Actually the first years I simply would review the catalogs, check that they were consistent, reformat them if I thought that would make them more usable, and then prepare documentation which would provide enough information to let somebody else use them. But after, I don't remember how long, five or six years, I guess, the person who was in charge of the Data Center was asked to leave, and for all practical purposes I took over managing it. Now, I wasn't always the manager in title, although I was for a while, but for the most part, I effectively ran the science side of the program.

Now, there were other people who were much more skilled at computer side than I was, who played a major role, but basically I had the primary responsibility for the center. That involved not only the Astronomical Data Center at Goddard, but it involved major interaction with the Astronomical Data Center in Strasburg, France, and to a lesser extent with others around the world.

But that involved not only reviewing catalogs when they came in, but also keeping my eyes open at meeting and any of astronomical literature for catalogs, which I think, thought wold be appropriate to be archived, and things of that sort, as asking for them and talking to people about how to—I'm often answering questions about they should prepare their data to submit it to the center, that sort of thing. This, I should emphasize, did not include satellite data. The center is only ground-based observations and laboratory work.

WRIGHT: Well, you got to use quite a bit of your talents all in one spot, and possibly not have so many long hours—

ROMAN: That's true, yes, and it was even better because I could do a lot of the work from home, telecommuting. By that time, Mother couldn't be left alone for a day, and I did have help so that I could do other things, and I could get to Goddard when I needed to. I went at least one day a week because I felt that I had to interact with the other people in person, not just by e-mail, at least that often. But it helped. The fact that I could work from here did mean that I didn't have to have people on those days. She didn't need the kind of attention that would keep me away from work for a long period of time, but I had to be here. I had to prepare the meals. I had to help her out of a chair, that sort of thing.

WRIGHT: What a great combination. You could feel very relaxed doing your work in, take care of your personal—

ROMAN: Yes, it worked very nicely, and I continued after she died. I continued to telecommute quite a bit of the time, and now the administrative assistant in the group telecommutes three days a week, and the primary computer person, although they do have some others working, helping the group now, but the one was primarily working with me, has moved out to Arizona and is telecommuting. So I started something.

WRIGHT: Your trend continues. What a legacy. I'm sure they all appreciate that, too. It's nice to be able to have great people working for you, and everybody's happy in that—

ROMAN: Yes, well, I think the group was happy, although the reason I left—as it happened, I think it was probably the best thing in a lot of ways. But the civil service person, this was a contractor, of course, the civil service person, who had taken over our part of the contract, and I did not get along. I was asked to leave. I think, to some extent, she was jealous of me, but there were other—I mean, we also disagreed in many areas.

WRIGHT: Sure. When we were preparing for this time with you, I read that someone had quoted you as saying that you had categorized your career into three phases: research and teaching and management and support. Do you still feel that way?

ROMAN: Yes, yes, very definitely. In my period at Yerkes, I stayed Yerkes for six years after I got my degree, did graduate-level teaching and spent most of my time on research. Of course, NRL, the three and a half years at NRL were research entirely except for this informal course I gave. Then I had the management period, which was very definitely management. Then the support services contractor. Now I guess I might say I have a fourth area that I'm keeping pretty busy on a whole variety of volunteer activities.

WRIGHT: And enjoying those as well?

ROMAN: And enjoying those, too, and also doing other things, but, well, I'm the secretary of the National Capital Astronomers, which is primarily an amateur organization, although it does contain a number of professional. I'm now, as of a few weeks ago, assistant treasurer of our local AAUW branch, but I also manage their web pages. I read for the blind and dyslexic because they were anxious to get technical readers and they particularly wanted somebody who could read astronomy—

WRIGHT: Oh, how neat.

ROMAN: —and I do various other things. Give lectures. So maybe my fourth portion of my career is as a volunteer.

WRIGHT: But all those great credentials, all the previous roles have, certainly will make you a supreme volunteer. You'll have so many different opportunities to go for.

ROMAN: Yes, and I have done a little teaching since I—well, I actually did a tiny bit before I retired, but I've done a little at the local community college in the summer, teaching, well, I've done one course for advanced high school students and several courses for high school science teachers.

WRIGHT: Oh, what a value for them. That's terrific.

ROMAN: I've been co-teaching those and it's been fun. As I think I've indicated, I've always liked teaching, and most people seem to think I'm a good teacher. So it works out.

WRIGHT: Well, you certainly have shared so many of your ideas, which is, of course, the foundation of teaching, is that sharing of information. I was thinking back when you were talking earlier about not quite sure when your interest began to be an astronomer, but certainly it was at a time when there weren't very many astronomers. What was the perception then of astronomers compared to what maybe the perception of astronomy is now?

ROMAN: Well, it certainly was a lot less known, and I guess maybe today people get astronomy and astrology confused a little less. I'm not sure. But, yes, when I joined the International, the International Astronomical Union [IAU] supposedly contains essentially all professional astronomers in the world. When I joined it, that was the first the membership got over 1,000. Now it's pretty close to 10,000. So that's been quite a change. But the fact that the field is small has made it interesting because the fact that the field was small means that astronomers knew each other all over the world. This has made it very pleasant, and it's also helped in my job at NASA because, of course, in our international programs we dealt with astronomers in other countries. The fact that they knew me and I knew them in many cases made things a lot easier. Of course, the other thing is pretty obvious. As a woman, I was better known than someone else probably, than a man would have been with the same career. So people remember me.

WRIGHT: Out of that thousand, I'm sure there weren't too many women. Is that—?

ROMAN: No, not too many. There have always a lot of women in astronomy, but in general they've had more menial jobs, or they've taught in women's colleges. Women astronomers go back a long way. I guess the first one is generally acknowledged to be—I can't even think of her name. She was an Egyptian, about 300 B.C. I guess that was Hypatia. Then there was one in the Middle Ages, but generally there were other astronomers. For example, the Herschel's, did a lot of observing, but [Caroline Herschel] also did a lot of observing on her own. Tyco Brahe's wife [Kirstin Jörgensdatter], after his death, finished his catalog and published it. There were a number of other women who worked with other members of their family. The man got the credit but the woman did her share of the work, to put it mildly.

Then there were a lot of women at Harvard around the turn of the century. They were known as "Pickering's harem." [Edward C.] Pickering was the chairman of the department and he hired women, and he hired them to do very menial work, but they did excellent things.

For example, the Henry Draper catalog, which is a catalog of the spectral types of all stars brighter than about ninth magnitude, which is something, oh, I don't know, close to 300,000 stars, was done by a woman. It was a woman who found the relation between sephiad brightness and pariad [phonetic]. These were women at Harvard. The women did a lot of work in variable stars. A lot of progress in that field is due to the work of these women in Pickering's harem, but they just didn't get much credit.

WRIGHT: Well, maybe in the years ahead—

ROMAN: Oh, it's much, much better now. It's much better when I started, in spite of the fact that today's women think they're terribly put upon. Well, maybe they are.

WRIGHT: Did you find a lot in the years that you were at NASA, see more and more women come into this field?

ROMAN: Oh, yes, very definitely. You know, they're both in NASA and in the universities.

WRIGHT: As part of your career, you mentioned you traveled quite a bit. Did you have a chance to stop your reasons for being there and have a chance to gaze up and look up at the stars from all over the world?

ROMAN: Well, unfortunately, a good num—well, it depended on where I was. A lot of places don't have very good skies, like Washington, and I guess I always—I do look at the stars. Even there I go out every night and look at the sky, and it's pretty discouraging. I saw one star the other, night before last, and last night wasn't clear, but that's another thing I do. I support a program where the amateurs bring telescopes to a national park near here so that the general public can look at the sky. I don't have a telescope, but I go over and answer questions and talk to them about things.

But, anyway, coming back to looking at the sky, I can tell you a story about that. Once upon at time at Yerkes, I used to do a reasonable amount of babysitting, and I babysat for a family that had gone to the movies, and the Chandras had gone with him, the Chandrasekhars, and after they came home, we sat a while and probably had some coffee and ice cream or something. I don't remember that anymore, but, anyway, Chandra and I left together, and the person that we'd babysit for, who was an observational astronomer said, "You could always tell an observer from a theorist. The theorist looks down. The observer looks up," and I never noticed that but I realized at the time, yes, I always look up when I go out at night.

WRIGHT: Yes, well, there's so much to see, isn't there?

ROMAN: Yes.

WRIGHT: You spent so many years day in and day out and then something other people literally reached the stars from one way or the other. Was there a time that you thought maybe you should have take a different road, or were you always glad to be in the path that you were in?

ROMAN: I think I was glad to be in the path. As I said, the period at NRL, I did feel that it was not a field I stay in indefinitely because of the fact that I didn't have the electronic background, but I enjoyed it. I found it very helpful when I went to NASA because, of course, there I was, in NRL I was in an engineering organization, so I was dealing with engineers full-time.

One of the problems when I started with NASA was that the astronomers knew what they wanted to do and the engineers were perfectly willing to help them do it, but they couldn't communicate with each other. So I felt that much of my time in, or at least an important part of my time in the early period, was acting as an interpreter between the astronomers and the engineers.

WRIGHT: Yes, finding a common language, I guess.

ROMAN: Finding a com—well, yes. It really did need somebody who had some feeling for both sides. So my period at NRL, it was a happy period. I'm not unhappy about it and particularly in view of the fact that I didn't see, I thought it was a much better opportunity than I saw anywhere else in the university area. So I don't feel unhappy about any part of my career.

WRIGHT: Any time at all that you come on something that was possible maybe unexplainable as you were starting to put things together in your research or having maybe the proposal people wanted you to do something that would push the envelope, would maybe answer some questions that people have asked for decades and no one wanted to reach to find those answers?

ROMAN: Oh, well, sure. Certainly some of the proposals were— in fact, particularly in the early days when we were doing surveys, it was a matter of simply trying to find the answers to the unknown. As far as my research, I made some interesting discoveries, but I guess I wouldn't say that they were unknowable because I found them out.

WRIGHT: Well, they were unknown at one time.

ROMAN: They were unknown at one time, definitely yes. I think the most—I mentioned that I worked on high-velocity stars. The way I got into that was that I was studying stars, bright stars not all that different from the sun. The group at Yerkes who had been working with Morgan, who was my thesis advisor and whom I continued to work with after my degree, had pretty well divided the types of stars by temperature. Different people worked on temperature groups. Well, the last group that anybody worked on was the sort of "average" star, let's put it that way. So I took over that. Nobody else wanted it, and so this was a supposedly very uninteresting group of stars.

Well, when I took the spectra and looked at them carefully, I discovered that some of the stars had somewhat weaker lines of what astronomers called metals. Now astronomers have a very strange definition of a metal. A metal is anything heavier than helium. So to an astronomer, neon is a metal and oxygen is a metal and carbon is a metal, etc. But I found that in these stars the lines other than hydrogen and helium were a little weaker, just a little weaker than they were in other stars. I divided the stars into two groups according to whether the lines were stronger or weaker. I found two things. I found, and this was the primary thing I found, was that when you looked at the space velocities and the space distribution of the stars, the stars with stronger metals had lower velocities relative to the sun and were more concentrated to the plane of the Milky Way. Of course, the others had higher velocities and were farther from the plane of the Milky Way. Then I also found that you could measure this difference in metalicity by looking at the colors, and if you compared the color—you assumed the colors were in the blue and the yellow were the same and then looked at the ultraviolet, the ultraviolet was stronger in the stars with the weaker metals so that you could tell them apart a lot more easily than you could by the spectra. That we knew, had known for a long time, that very high-velocity stars tended to have the weak lines, but nobody had ever thought that the stars near the sun would. It was not an easy thing to find. I could see it on the photographs.

I had never been able to see—I made tracings of plates. I made tracings at all kinds of resolutions. I never could see it. I have since tried looking at it with tracings from CCDs. I cannot see it. I could see it photographically. I still can, but I cannot see it on tracings. But anyway, it was clearly an important observation from the standpoint of understanding the structure of the galaxy. So here were these uninteresting stars turning out to be very interesting indeed.

WRIGHT: I guess that's the—

ROMAN: That's the fun of science.

WRIGHT: I was saying that's fun of astronomy because it's a endless quest—

ROMAN: Oh, I think any science field is.

WRIGHT: Yes, when we look up at the stars, I guess that the one quick place that we know that you're looking only a small part and there's just so much more to see. You were the first astronomer for NASA, the first female senior executive, and your list of firsts go on and on, but looking back, did you at the time realize the ground that you were breaking for so many others? Not just for women but for other astronomers and for people who wanted to move into a field that they loved?

ROMAN: I don't know. I think I did but not something that I pay all that much attention to. I guess that's the best way to put it. As far as other astronomers, I don't know to what extent I broke the ice for them. I think the women's movement generally broke the ice a lot more for them than the few of us who did things early.

WRIGHT: Do you feel your field of astronomy is certainly one that's of value today for people to move into?

ROMAN: Yes, although fresh Ph.D.s will tell you that jobs are very hard to get. They are if the job you want is a research position in a university. But very, very few astronomers are unemployed. So I would say yes. What I tell people about astronomy as a career is go in with your eyes open but if you really want to do it and you recognize the problems, do it. I mean, after all, when I graduated there were extremely few jobs in astronomy, to put it mildly, but then, of course, there were a lot fewer astronomers. This was right after the war when we had the influx of GIs coming in and getting degrees. Even at that time the average number of astron—Ph.D.s in the country was about—per year was about twenty-five, male and female. Today, well, it's probably closer to 1,000. Maybe not that high, but it's certainly several hundred. There just weren't that many jobs either, as you can imagine.

WRIGHT: And do you feel like the taxpayers are still getting their money's worth on—

ROMAN: Well, I guess the question is, what do you feel is worth it to the taxpayer? The taxpayer is still learning about the universe. Also, depending on what field the astronomer's in, he's learning about things that affect the Earth and affect the climate of the Earth and the problem of solar flares, solar activity and its effect on electromagnetic systems and communications satellites and things like that. So my own feeling is, yes, but I don't think you can put a price or a value price on basic science. I think basic science is like poetry, or like, was it—who, Edison who said, "What value is a newborn baby?" So to say, "Is the taxpayer getting his money's worth?" it's a question of what he feels his money should be used for and what various things mean to him.

WRIGHT: So much of it's an investment to learn more and more, and it's certainly pays for itself, doesn't it?

ROMAN: I mean, when you look at the amount of money that's going into astronomy, including both the space and ground-based program, which is very large by historical standards, and you compare that with the amount of money that's going into, say, football or baseball or cigarettes or alcohol, it's trivial. The question is which do you get more from, and I don't know. It depends on who you are and what you want.

WRIGHT: What your value is, that's exactly right. When we were talking about, I was thinking about satellites, and even that word has changed, I guess, in its meaning for lots of people. I was going to ask you and I forgot earlier about your reaction or what your thoughts were when you first heard about Sputnik and, of course, and how that affected so much more. When you look up at the sky now, there's more than just stars. You have artificial or man-made objects as well.

ROMAN: Well, I guess mainly I was excited, and I was in NRL at that time and we immediately went over to the lab and started listening to it, but I don't remember much beyond that, except for the excitement.

WRIGHT: And, of course, there's been so many more other objects that are up there now, and then, of course—

ROMAN: Yes. Now they're sort of routine. You don't even pay attention to launches of anything except manned launches.

WRIGHT: Is it like what a traveler riding down the street and they have this wonderful landscape they look, but they don't look at the billboards? Is that how you do when you see this other space debris, or is that come to block your vision at all when you're starting to—

ROMAN: Well, space debris doesn't block vision, but it's a problem in satellite safety, and it will be a problem in other ways eventually. But right now it's primarily a matter of damage.

WRIGHT: I guess the other area that we were just curious about your involvement was the search for extraterrestrial intelligence. Did that cross your realm of possibility?

ROMAN: It did, but not in a very major way. When NASA first became involved with it, I was handling that part of the program, but I never got very deeply involved in it, and it gradually went over to the planetary program. I have to admit that I had and still have mixed feelings about that. I think that the SETI Program is high unlikely to succeed. I think there is life elsewhere in the universe. I can't prove it, but I'd be very surprised if there isn't, but there would be life sufficiently like us in both capability and evolutionary status to communicate with us and yet still be near enough to communicate, I think personally is highly unlikely.

On the other hand, there are people whom I respect very highly, scientists I respect very highly, who do think it's worthwhile. So, as I say, I have mixed feelings.

WRIGHT: Yes, but you've had such a very full career and still haven't finished. You have so many more things that you're doing down and sharing information. Are there other things that you can think of that you would like to add or maybe something that we didn't cover or maybe something we just talked about briefly that you would like to add some other details on? I'm just kind of going through my notes and trying to make sure that—

ROMAN: Well, I cant' think of anything at the moment, but—

WRIGHT: I have one thing that—when you were talking about your trip in 1956 to Russia, we talked about it, but you—some of the details. If you could share with us what it was like going to—was that your first trip also outside the country or—

ROMAN: No, it wasn't my first trip outside the country. My first trip outside the country was in 1955, when I went to the International Astronomical Union meeting in Dublin and before the meeting I visited, well, I spent a few, I visited observatories in and near Paris and southern France and in Germany and also visited a friend in Germany and a friend in London before I went to Dublin. I guess I, you know, the interesting thing about that trip was that I just sort of assumed it might be my only trip to Europe, because in those days people didn't travel as much as they do now. It was a major undertaking, not a major undertaking but a major event to fly to Europe. Of course, I don't how many trips to Europe I've had since, quite a few. Anyway, but, so it wasn't my first trip. It was my second.

The trip itself was fine. I was met—as I say, I was a guest of the Academy of Science, so that I was definitely a VIP. In the Soviet Union at that time scientists had a pretty high status. So I was met at the airport and taken to a good hotel. But I had a number of interesting experiences. In Moscow, at that time we weren't allowed to wander on our own. They said we'd get lost. Now two years later I was there for the IAU meeting, and I guess there were just too many Americans, and they gave us on us and we wandered all over the city by ourselves. But in '56 we didn't.

I dislike Moscow. I've been there the two times. I haven't been there since, and I have thoroughly disliked it both times. I was glad to get away from it the first trip to go down to Armenia and also to come back. I've enjoyed the rest of the Soviet Union, but not Moscow.

But, anyway, so then I went down to Armenia, and I guess I have a number of interesting bits of memory of that, let's put it that way. One of the things that I remember—well, one of the things from Russia generally, but particularly Armenia at that time was that so many obviously mangled soldiers, people with one leg or an arm missing. The war damage was apparent elsewhere in Europe, even by that time. But in the Soviet Union, it was a matter of seeing the people, the men that were damaged.

The observatory was new, obviously, and yet—and I stayed in the dormitory there, which was a new building, and the top step was slanted at about a fifteen-degree angle from the horizontal. Then later I went, the director of the observatory had a house on the observatory grounds, but he also had an apartment in Yerevan.

The observatory was outside in a town called Borzhomi [Georgia?], and I visited his apartment, which was three years old. You walk up the stairs and the plaster was coming off the walls. I mean, it just, incredible. It was fast building. You could see that. You could see buildings going up. Well, a huge building that covered most of a block could go up a floor a day in Moscow, but the quality was terrible. Another thing that I remember is that they talked about equality of men and women. They were doing the building. The men were standing around watching. The women were carrying the cement. You saw the same thing on the roads. The men would be watching. The women would be doing the work, the road work. We had supper, really a dinner, on the collective farm nearby, and—very nice dinner, local food.

Two interesting things about it, there weren't enough forks to go around, and, more striking, they, the local, the people, the farmers were, had been doing the barbecuing or grilling. They had a pit. I guess—what do you call that? I can't even think of the name. It's a perfectly standard name, standard way of cooking with a fire and you hang meat over it. Anyway, they had been doing that, and they were all standing behind the area where the fire was, watching us. But when we finished and left the table, they all came and sat down and ate what we had left. I found that very striking. There were also interesting things.

One of the more interesting experiences, I was so glad to get out of Moscow that the first morning, quite early, I went for a walk, just a, you know, just went out in the country, and met some farmers. It was fall, and they were picking grapes. So they had to give me some of their grapes. Well, I was beginning to get so many grapes, I didn't know whether it was more polite to accept them, which they obviously wanted to give to me because they were proud of them or to turn them down because it was obviously more than I could possibly eat, but it was an interesting experience just the same. Then there were sort of things that I noticed, like, for example, they "date" their bread, by putting—they have flat dough that they hang over the fence posts, fence rails, and let the sun bake it, which I thought was rather interesting.

WRIGHT: Be a different recipe, isn't it?

ROMAN: Yes, and another experience I found interesting was one of the young women there asked me about—I was not at Yerkes anymore, but she asked me about, you know, where I lived when I was at Yerkes and so forth and what did I do about getting home after observing. I said, well, I walked. And she said, "Aren't you afraid of the wolves." I said, "No, we don't have wolves in that area. We have them farther north, but not in that area." Well, while I was at the observatory, one of the dogs was killed by a wolf. So they really were worried about wolves there. Well, those are some of the experiences.

Then, another experience, after the conference we drove up through Azerbijan to Georgia and went out to western Georgia to an observatory at Abastumani, which was normally off the area where the foreigners could go, but they had special permission to take us. Well, it snowed while we were there, and so they decided, this being in a mountain, as observatories often are, that we probably should go down, just to spend the afternoon and then come back that evening. So they decided it really wasn't safe to drive down in the snow. Then there were a couple interesting things about that. I don't remember where they put various people, but they didn't really have accommodations for us because they weren't expecting us.

Well, I stayed with the librarian and there were two things that I found interesting about that. One, it was fairly cool. It was snowing, after all, and but the end of the room, the sort of a very tiny apartment had French doors, and they didn't close. So it was cold, really cold. The other thing about the apartment is there was no seat on the toilet, which was a bit of a problem, but I guess not for them because they tend to use the Asian-type of toilets where you stand over them.

The other thing was, it was some of the younger people, including there was a Russian woman whom I ran into many times during my career, who, I guess, not at that time, because the space program hadn't started, but had a major position in the Academy of Science and later essentially was my counterpart in the astronomy part of the program. She was a lot more politically inclined than I am, but other than that, she was more or less my counterpart. She was several years older than I am, but not much. She and some of the students were playing ping pong. Well, I went out to watch and they invited me to play. She challenged me, I guess, that was it. She had challenged me to play. Well, I accepted. I figured it was a way to get warm, if nothing else. Much to the amazement of everybody, I beat her soundly. It was such fun to sort of watch these students standing around counting, keeping score and getting more and more amazed. So I felt that I'd upheld the pride of the U.S. Clearly, they didn't think any American could beat her. She was very highly respected by them because both her age and her position, and they thought she could do everything, and here was this foreigner coming in.

The other thing, and this may be more than you wanted to hear about this experience. The other thing was, the next day, we drove down to Tbilisi, and this woman who was sort of our, as I say, she really was our hostess, insisted that we each drive. There were in the group, at this symposium there were four of us from outside, well, five of us from outside the Iron Curtain, two who were clearly communists, a Mexican and a Frenchman, or at least communist leanings. I don't know if they were party members, but they certainly had communist leanings, and three Americans who, as far as I know, had no communist leanings. So she insisted, she had obviously heard that all Americans could drive and didn't believe it. So she insisted that we each drive.

Well, the two men drove first, and then she insisted that I drive, and I said, "Well, I don't have an international driver's license." "Well, that's all right." Well, she insisted. So I took the wheel, and it turns out that at that time, women did not drive in the Soviet Union. A few of them were beginning to drive in Moscow, but that was all. Well, the chauffeur who was, of course responsible for the car was obviously very worried, but he couldn't say anything against this woman. So as I started, I could just seem him visibly relax and feeling much more comfortable. He realized that I could drive, and it felt so good to drive after sitting for so long that I just kept the wheel. I drove into the—no, let's see, no. The observatory where the dedication was was at Furokam [phonetic]. I drove into the town of Borzhomi, which is a resort town. I just drove right into town and eventually parked for lunch, but the interesting thing of that was that they were un-used to seeing a woman driver that I got cat calls and whistles, which I have never gotten, at no other time gotten for my driving.

WRIGHT: What an impression you left.

ROMAN: I surely did. So those are my impressions of my first trip to Russia.

WRIGHT: Of the observatories that you had the opportunity to visit, is there one that seems to stand out in your memory as, well, in fact, well, more than one of the others? And that might even have been influenced by the time that you've seen them. You've seen them so many times now that—

ROMAN: Yes, as I said, I was—I don't know. I don't think of any that I feel—I have to admit the 200-inch telescope is pretty impressive, its dome, and the other things. Well, you know, the prime focus cage and so forth. I guess that I would say that was the most impressive that I've seen, although, well, I have not seen the really large telescopes in Hawaii, or even the one in Texas, which is a—well, yes, I have seen the one in Texas. No, no, I haven't. I saw the 107-inch, but I haven't seen the new one that they have which is a different type of telescope but very large. So I haven't seen the really large telescopes that are coming up today. So I guess I would say that in my—things I've seen that I would count the 200-inch as the most impressive, although I have to admit that it's sort of hard to pick one as standing out.

WRIGHT: At least it's nice that they're changing so you have places to go now and more things to see and more adventures to have and things to learn.

ROMAN: Yes, oh, yes. Yes. I've been to Mauna Kea [Hawaii] a couple of times but not since the really large [Keck] telescopes are up.

WRIGHT: Well, we'll be looking forward to hearing about your latest adventures as they become quickly history. You'll be able to plan more and to do more things. We certainly have learned so much, and we certainly enjoyed speaking with you today.

ROMAN: I'm glad you have. I sort of felt that I was rambling, but I've been interviewed so many times.

WRIGHT: Not at all. No, it's been wonderful with the details and we certainly hope again as we send this to you for your review that if you find more things that you would like include, please do and just thanks again for saving the day for us.

ROMAN: You're welcome.

[End of Interview]