Nancy Grace Roman

Dr. Nancy Grace Roman, NASA's first chief of astronomy, is shown at NASA's Goddard Space Flight Center in Greenbelt, Maryland, in approximately 1972. Figure reproduced from NASA with permission.

Annual Review of Astronomy and Astrophysics

Nancy Grace Roman and the Dawn of Space Astronomy

Nancy Grace Roman

Chevy Chase, Maryland 20815, USA

Annu. Rev. Astron. Astrophys. 2019. 57:1–34

The *Annual Review of Astronomy and Astrophysics* is online at astro.annualreviews.org

https://doi.org/10.1146/annurev-astro-091918-104446

Keywords

autobiography, space astronomy, NASA, *Hubble Space Telescope*, Milky Way, Galactic structure, stellar spectra

Abstract

Dear readers: We are sad to report that, soon after submitting her draft manuscript for this prefatory chapter, Nancy Grace Roman passed away on December 25, 2018. This final version of her memoir has been lightly edited but remains very true to the original. However, an Abstract was missing. Rather than trying to synthesize one in Nancy Grace's inimitable style, we take this opportunity to comment briefly on her life and its significance.

Nancy Grace Roman was born in 1925 and came of age scientifically in the United States during the 1940s and 1950s. Together with the equally fascinating prefatory by Vera Rubin (*ARAA*, Vol. 49), which we also recommend to you, these two memoirs give us intimate insight into the obstacles faced by women astronomers trying to rise in the field during those years. Roman's memoir is bitingly candid, recounting numerous snubs by teachers, insultingly small salaries, and attempts by her thesis advisor to simultaneously exploit her scientific findings and smother her role in them. Discouragement at every turn from doing forefront research is what drove Roman into government service, where she found a niche and blossomed as one of the visionary founders of the US civilian space program. We do not know what impact Roman might have had as a researcher with access to the world's largest telescopes, but we do know that her influence as an enabler of other people's science was vast. Her sobriquet as the "Mother of *Hubble*," bestowed by admirer Ed Weiler, is well deserved.

Nancy Grace granted an audio interview to Joss Bland-Hawthorn on August 4, 2018, just a few months before her passing. It captures her persona more vividly than mere words on paper, and we recommend the online recording to you at **https://www.annualreviews.org/r/nancy-grace-roman-interview**.

Contents

EARLY LIFE

I was born in Nashville, Tennessee, on May 16, 1925, where my father was on the faculty of Vanderbilt University. About that time, an oil company approached him to offer him a position in what was then a new field, geophysical prospecting. He took a one-year leave of absence from the university but stayed in geophysics for the rest of his life. The oil company moved him even more frequently than the military would have. As a result, I lived in four states (and one twice) before I was three. Of course, I do not remember these years, but they are one reason I remained an only child. My father wanted to know where his child would be born.

My mother had been a teacher with a specialty in music. She was an excellent pianist and had contributed to her family's income while she was in high school by giving piano lessons. In her generation, women were not expected to work outside the home, and most schools would not hire married women. Although my father had said that, if he could not support a wife, he should not marry, she did work for one year because they needed the money badly. They never told any of their families. I believe that my home life would have been happier if she had continued to teach. She was a born teacher, loved the field, and hated housework.

My father lost his job during the Depression, which led to more moves. As a result, I went to eight schools before I started high school, one twice. The curriculum in each was somewhat different. Thus, I had to study at home to catch up with my class or repeat material that I had already learned. Fortunately, I learned quickly. I also discovered that penmanship as well as vocabulary varied from region to region. However, the primary problem was that I found it difficult to make close friends. Although I probably would have been an introverted bookworm anyway, the impermanence did not help.

I went to kindergarten in New Jersey. One of my memories is of being frequently reprimanded for talking in class. I did not think that I talked any more than the others, but my voice carried—this was an attribute that came in handy much later when I became involved in public speaking. After kindergarten, we moved in the fall to Houghton, Michigan, a small city in the middle of the finger that juts into Lake Superior from the northern peninsula of Michigan. My most vivid memories of that region are of the winters. On my sixth birthday, in mid-May, I was surprised to have it snow. Although, of course, I was shorter in those years, I remember walking to school between snow banks higher than my head. For many years, I had a small scar on my face as a result of frostbite I had acquired walking to a party with my face wrapped in a scarf. The roads were never plowed to bare pavement so that there would be cover for the sleighs that farmers used to get to town. I thought getting to school by sleigh would have been fun although I now doubt it.

"YOU WANT TO BE AN ASTRONOMER!"

By the time I was in seventh grade, I had made up my mind that astronomy was what I would do with my life, if possible. I realized that it would mean many years of education but reasoned that if I could not accomplish that, I could, at least, teach mathematics and physics. However, there is evidence of my interest much before that. A letter my mother wrote when I was four mentioned that my favorite subject to draw was the Moon. That may be more interesting than relevant, but I think I received an astronomy book from my grandmother for my tenth birthday, which indicates that I was interested before age 12.

Between fifth and sixth grades, I organized my friends into an astronomy club. At that time, it was possible to buy a small hardcover book for 10¢. We each bought a book titled *Seeing Stars* that we used in our once-a-week meetings to find and learn about the constellations. I also have a memory from that time of being impressed by a bright meteor that crossed the sky nearly from horizon to horizon at twilight.

How I became interested in astronomy is vague. My mother may have been a major influence. The northern Michigan town had a dark sky. There, my mother showed me the constellations and the Northern Lights that were fairly bright in those years. She was surprised when I mentioned it to her late in her life. As she responded, she had also tried to teach me about the trees, flowers, and birds. I remember walks in the woods in my kindergarten days, but it was the sky that caught my interest, although I have also always enjoyed other aspects of nature.

As a girl, I was strongly discouraged from a career in science. Although it was not the first time I was informed of the foolishness of a career in science for a woman, I remember vividly the reaction of my high school guidance counselor when I asked for permission to take a second year of algebra instead of a fifth year of Latin. She looked down her nose at me and sneered, "What lady would take mathematics instead of Latin?" Thanks to Pearl Harbor, the issue became moot. I substituted a summer of chemistry for my senior year and started college a year before I had planned.

My parents were supportive, although my mother gave me subtle hints that she was skeptical about my choice of career. I told her this a few years before she died. She was surprised and denied it, saying that she and my father wanted me to do what would make me happy. I honestly believe that she did not realize she was showing her concern. Later, when I joined the National Aeronautics and Space Administration (NASA), the "new kid on the block" was much in the news. There were still "Woman's Pages" at that time. Their editors were delighted to have an opportunity to publicize that there was a woman with NASA. As a result, I received a great deal of publicity. A question I received frequently from my interviewers was, "How did you get interested in astronomy?" My answer is I really do not know, but I never seriously considered anything else.

When I was 10, we moved to Reno, Nevada, and then two years later to Baltimore, Maryland, where I went to two junior high schools and high school. In the second junior high school, the policy was against segregating children according to ability. I realize that this is common today, but I found it a problem. Those of us who were quick learners were bored much of the time. I did my homework in class and still often had time to write poems or develop crossword puzzles for the school paper. At the same time, the slow learners were unable to keep up and were completely lost.

HIGH SCHOOL

In my junior high years, I read every astronomy book I could find in the Baltimore library but, much to the consternation of my friends in later years, I never had a telescope. My usual explanation was that inexpensive telescopes were not common in my childhood, and I did not consider making one. However, I believe that the real reason is that I have always been more interested in the science than in looking at astronomical objects. Although I have used a variety of professional telescopes ranging in mirror or lens diameter from 5 inches to 82 inches, I have never become adept with amateur telescopes.

Because I began to get more nearsighted early in high school, the ophthalmologist forbade me to do any reading except for schoolwork. I had been an avid reader, so this was a real punishment, although not meant as such. I not only became a slow reader but also missed many of the books I would have read in those years, such as *Little Women* and *Alice in Wonderland*. I have become an avid reader again but am still slow, in spite of a course in speed reading. I suspect that the fact that a significant portion of my reading is of technical material is partly to blame.

To occupy my time, I took up weaving. I saved enough to purchase a 20-inch, four-heddle table loom with which I made many things, from rugs to scarves, to fingertip towels, and experimented with various types of weaves. I continued to enjoy many types of handwork, including knitting, crocheting, and tatting. Until I had to stop because of arthritis, I made dozens of pairs of mittens for poor children. For many years, I designed and sewed my own clothes.

When I got to college, I learned that my public-school education was comparable to that of graduates from renowned prep schools; however, one course was discouragingly poor—physics. Our teacher normally taught business courses and knew no physics beyond what she could read in our textbook. We were able to read the book, but our questions beyond that left her lost. I seem to have had poor luck with physics teachers. In college, the usual professor for the second-year physics course left for Los Alamos at the beginning of the semester. His replacement turned out to be senile. Although the head of the physics department apologized to us, expressing anger that the university from which the substitute came recommended him without mentioning his problem, the seven of us in the class were left to learn the material on our own, working cooperatively. The professor copied tests from preceding ones without noticing that they included material that had not been covered. On the final exam, I wrote an essay on a subject of which I knew nothing except what I could guess from the title of the subject. The only comment on the returned paper was a spelling correction! The primary problem is that I lost the introduction to quantum mechanics, a serious loss from which I never recovered in spite of efforts to learn it on my own.

I also had problems with my geometry teacher in high school. She was an older woman who believed in rote learning. In our exercises and proofs, we had to quote the number of the theorems we used, not their content. I had no trouble with the proofs and exercises, but I have never had the type of memory that let me memorize the numbers of the theorems rather than their content. After warning me, she finally failed me on a test.

I had my first teaching experience in high school. In my freshman class, one of the girls was failing algebra. I worked with her and felt proud that she finished the course with a B. Obviously, she was willing to put her effort into learning the subject. My second experience and my first paid job (at 25¢/h) was tutoring French, of all subjects. I certainly could not speak French, but the student needed help with grammar, which I could handle. Unlike the girl learning algebra, this one had no interest in learning. I was also quite active in extracurricular activities. They included several clubs, athletics (in which I was very poor but diligent), and plays. Although community service did not have the emphasis that it has today, students with an A average were expected to provide some service to the school. I sold streetcar tokens twice a week for most of my three years.

I was a first-semester high school junior when Pearl Harbor occurred, plunging the United States fully into World War II. In our accelerated program, our small class would have enough credits to graduate at the end of our junior year. Many of the girls wanted to get out early to do war work or study for their professions sooner. The school administration agreed that we could leave on two conditions: that we all did the same thing and that we would go to summer school to study a year of chemistry (in 10 weeks with first and second semester chemistry simultaneously).

SWARTHMORE

Because of the change in plans, I started to choose a college much earlier than I had expected and was late in applying. Johns Hopkins at that time would take women only in night school and the University of Maryland did not have a good reputation, and travel in general was difficult. This led to my choice of Swarthmore—a college with a good reputation, with a well-known astronomy department under the direction of Peter van de Kamp, that was not too far from Baltimore, and, of course, that was coed. At Swarthmore, the Dean of Women interviewed each freshman girl. If she failed to convince her not to major in science or engineering, the Dean had nothing more to do with her for the next four years. She sent me to Dr. van de Kamp. He reminded me that he was using data his predecessors had collected fifty years earlier, and he was obtaining data that his successors would use fifty years hence. I was too naïve to realize that he was also trying to dissuade me from science. My first "encouragement" came in my junior year, when the head of the Physics Department approached me in the lab and said, "I usually try to discourage girls from going into physics, but I think *maybe* you might make it."

In my first year at Swarthmore, I took algebra and trigonometry, astronomy, scientific German, and history. This selection of courses convinced me that my choice of science was correct. Because I needed little time to study astronomy and mathematics, I could spend my time on German and history. My slow reading made the latter particularly challenging. Although I think that languages are important and have made attempts to learn at least the rudiments of French, Spanish, and Russian in addition to German over the years, I have no facility in languages and they do not stick with me.

Swarthmore had two arrangements for the junior and senior years. A student had the choice of continuing to take four courses each semester with exams at the end of each that counted toward graduation, or taking two seminars each semester with written and oral exams at the end of the senior year. These final examinations were set by professors from other institutions. The seminars were not only more concentrated but also required more independent research and study. There was another astronomy major in my year, but she took the course route while I took seminars. Consequently, in astronomy, I was the only student at times. After staying up all night writing a paper for the next day, I fell asleep in class one afternoon. The professor kept right on talking although I am sure he noticed that I was asleep. For my final examinations, one of the examiners was the chairman of the astronomy department at Columbia University, who had been expecting me to do my graduate work there. When I decided to attend the University of Chicago instead, he was angry. As a result, I missed High Honors, although I was told that I should have received them. Several years later I spoke with Elizabeth Urey at an alumni day event. She knew the Columbia professor, as her father was also at Columbia and they lived near him. She told me that he had the reputation for that kind of behavior. There were few long-term consequences, but it would have been nice to have had the High Honors.

Although I certainly was not athletic, I did participate in a variety of sports in both high school and college. In college, I was also a member of a demonstration English dance group, but I was more enthusiastic about square dancing—an enjoyment that continued as long as I had the stamina for it. One of my PE classes, on recreational leadership, led to some interesting jobs while I was in graduate school. I also taught square dancing during that period. A requirement for graduation was that we be able to stay afloat in deep water for fifteen minutes. As a result, I learned to swim. While I have never been a strong swimmer, until recently I could swim slowly for a long time, an ability I have enjoyed in Washington, where I have lived in apartments with swimming pools.

I was particularly active in the Outing Club. Among other activities, we occasionally spent weekends at a cabin on the comptroller's farm about 40 miles south of the campus. During the war, traffic was minimal and we were able to bicycle safely on US Highway 1 most of the way. I have several memories of these weekends. On one trip, my brakes failed on the top of a hill with a major cross highway at the bottom. Not daring to cross that road without the ability to stop, I jumped off my bike with no consequences but a scraped knee. On another trip on a very hot day, we stopped at a house to ask for water. The woman who opened the door was highly suspicious of three rather scruffy young people. Finally, she said that we could leave our bottles on the porch and leave the porch. She would then fill them for us. During one fall weekend, the comptroller presented us with several pumpkins. We improvised many ways to cook them including a pie and a pudding with eggs that we bought from a nearby farm but were sated with pumpkins by the time we left. For some weekends, we were joined by medical students from Baltimore. One of these habitually carried a heavy textbook that I never saw her open.

The Outing Club made possible a wonderful week hiking and backpacking in the Adirondacks. This week included the most frightening experience in my life. Somehow, I became separated from the rest of the group on the summit of Mount Marcy. It became so foggy that I could not see from one cairn to another. As there was no other sign of the path, I became completely lost. After wandering for some time (it seemed like many hours), I finally found the path and made my way to a lean-to. It was occupied by several young men I did not know, but they made room for a drowned rat, and I was thankful to have a comparatively warm, quiet night. As the next day was the last day of our trip, I made my way down to the train station without further problems. This bedraggled young woman with a bulky backpack (sleeping bags were much less compact in those days) elicited many stares on the New York–to–Baltimore train, largely filled with businessmen.

My heavy schedule of technical courses left little room for the humanities and social sciences. Nevertheless, it was impossible not to get a liberal education at Swarthmore, between extracurricular events and informal discussions with other students. The four years there left me eager for more. Since then, I have read far more broadly and participated in groups and lectures with many interests. At Swarthmore, I was one of the organizers in 1945 of the first Folk Festival.

As my career shows, perhaps the greatest gift I had from Swarthmore was the ability and eagerness to learn new things, not only professionally but also in different areas. The College gave me a good background in the fundamentals of my field that permitted me to understand problems, techniques, and instruments well outside my research experience. At the same time, it stimulated my interest in a variety of social problems as well as in the humanities. I am the only person I know who did not take English in college, but writing seminar papers was a great learning experience. I think my biggest asset in my NASA job was the ability to speak and write easily and well. Swarthmore deserves a great deal of credit for the interesting life I have had.

A SMALL WISCONSIN TOWN

My decision to attend the University of Chicago proved correct. Although the decision was based on the fact that its graduate astronomy department was the first to get back to near-normal at the end of the war, Yerkes Observatory, where the University of Chicago graduate department was located, proved to be the center of world astronomy in the latter part of the 1940s, with a continuing stream of distinguished visitors who stayed for a few days to many months. To the surprise of most people to whom I have mentioned it, but not the president of Swarthmore, I found graduate school at the University of Chicago easy compared to Swarthmore.

The Yerkes Observatory was in Williams Bay, a town of about 1,000 residents in southeastern Wisconsin, about halfway between Chicago and Madison. I received my degree from the University and had been on the staff as a research associate for a year before I saw the campus for the first time, when I visited the wife of a professor in the hospital on Labor Day weekend.

Another student, Anne Underhill, arrived from Canada the day after I did. We became close friends in the two years that she was there and kept up with each other for the rest of her life. As it happened, Anne spent fifteen years at the NASA center in Greenbelt, just outside of Washington, so we visited each other frequently during those years.

Living conditions in Williams Bay were far from ideal. For the first month or two, Anne and I had rooms with an elderly, somewhat prickly woman who normally housed summer visitors. One morning, Anne and I were delighted to find hot water. We both took baths only to be berated for using her washing water. Another problem was that she had a son who did not want to let his mother know that he smoked. To indulge this habit, he spent hours in the only bathroom. As summer approached, I was delighted to be able to arrange a room with the friendly family of the chief maintenance man at Yerkes, who lived in an Observatory house. I lived there for six or seven years until they built a house of their own. A professor and his wife then moved into the house and were willing to have me stay. The University put up three small prefabricated houses. I was promised one but when I returned from vacation, having made plans to bring some furniture, I was informed that the person in charge had decided to give the house to a man. I was able to obtain a small furnished apartment in town. By that time, I had a car, so the commute (a little over a mile) was not difficult. It was just as well, as I would have had to move the furniture again two years later when I left Williams Bay.

In the first year or two, I had breakfast within my room, but I and the other unmarried students had to go into town for our other meals. In good weather, this was not a problem, but on some winter days it was quite uncomfortable. None of us had cars. Furthermore, the two dining rooms in the town were not particularly inviting. Anne and I, who shared an office, began making our suppers at the Observatory. In the winter, we could keep milk and other perishables in an unheated room just outside the office or on the floor, although it was sometimes a challenge to keep mice from drowning in the bottles. One evening, Chandrasekhar (known as Chandra) stormed into our office quite angry. We had smelled up the Observatory by boiling rice! (His office was as far from ours as possible.) On one New Year's Day when the restaurants in town were closed, Anne and I prepared dinner for the other students, cooking on a one-burner hot plate and an electric heater. I do not recall the remainder of the menu but do remember that we made ice cream in the snow.

After two years, an emeritus professor and his wife built an extension to their house for the male students who had been living in a large unheated room in the attic of the Observatory. The room was as uncomfortably hot in the summer as it was cold in the winter. They also started to serve meals. This was a major improvement over the meals in town, in both quality and convenience. Although I continued to live where I had, I enjoyed the meals at the boarding house. A bonus was that the family, from Belgium, spoke French during the meals. Although I did not participate in that language, hearing it regularly plus a weekly French class from the professor's sister was enough to make me fluent by the time I left Wisconsin.

As Williams Bay was a very small town, we were left mainly to our own devices for amusement. I organized a square dance group that continued in good health for several years. I also started a life-long association with the American Association of University Women (AAUW) to have connections with educated women. I have filled various offices in the AAUW in both Wisconsin and Washington. After I completed my degree, I joined a Great Books group in a neighboring town.

In my first encounter with the professor who would become my thesis advisor, he asked me to go to his house to change his bed because his wife was sick. Naïvely, I did it. I am sure he would not have asked a male student to do it. I did not run into many problems in graduate school, although the professors made it plain that they did not like educating women. "They will just go off and get married!"

When I started graduate school, I wanted to be an observational astronomer but was not sure in what area. To help with the choice, I asked three professors for short projects that I could work on over the summer. George van Biesbrock gave me a double-star orbit to solve; Otto Struve gave me a couple of spectra of an interesting star to analyze; and William W. Morgan suggested that I use one of the smaller telescopes to get spectra of the stars in the Big Dipper. Except for the stars at the two ends, the stars in the Dipper are close together in space. I was to determine the distance of the cluster of stars by comparing their spectra with those of stars whose absolute brightnesses were known. I decided easily that computing double-star orbits did not appeal to me. I tended to postpone working on the spectra from Struve. On a beautiful Saturday afternoon in June, I was measuring the plates when Chandrasekhar came by and asked, "Would you really rather do that than theory?" I replied, "Yes," but if I had ever been tempted to say "No" it would have been then. The project from Morgan expanded into my thesis. The stars in the Big Dipper are the bright remnants of a cluster of several thousand stars that has evaporated in time so that they are now scattered all over the sky. For example, the bright star Sirius is one of the stars originally in the cluster, although it is almost opposite the Dipper now. I collected the motions of many stars and found more than 200 of them that had left the region of the Dipper at about the same time. By geometry, I could also get their current distances and, hence, their intrinsic brightnesses. In this way, I could improve the accuracy with which the intrinsic brightness of a number of types of stars could be determined from their spectra.

My first scientific paper was a joint paper with Morgan and Olin Eggen on a type of star somewhat hotter than the Sun with weak calcium lines but comparatively strong lines of other elements heavier than helium. Because astronomers call all elements heavier than helium "metals," these are called metallic-line stars. We showed that the metal lines in these stars were a better indication of their brightness and temperature than the calcium lines.

Russia had a long history of important astronomical research and was rapidly increasing its engagement with modern astronomical problems. Therefore, we students urged Struve to teach a course on Russian. He finally agreed. In the first meeting, he taught us the alphabet. In the second, he gave us a smattering of grammar. For the third, he brought a slide of a page in the Russian astronomical journal and said, "Now read it!" That was the end of the course.

During my third year when I was working actively on my thesis, I was surprised to have the department chairman tell me of a position elsewhere and encourage me to take it. I had no idea of leaving before I finished my degree. As usual, I was stubborn, particularly when I guessed what the problem was. Morgan, my thesis advisor, went for six months without speaking to me, even to acknowledge a "Hello" when I met him in the hall. When I mentioned this to one of the male students, the reply was, "He is moody." When he was asked in a faculty meeting how I was doing, he had no idea. The remainder of the faculty assumed I was not doing anything. In fact, Bengt Stromgren and, particularly, Adriaan Blaauw, who were visiting Yerkes for several months at the time, gave me helpful advice that I should have received from my advisor, and my work was actually proceeding quite nicely. I finished my thesis on schedule with my final exams three years and three months after starting graduate school. I received a question about it 40 years after its publication.

Morgan liked to keep late hours, seldom coming to the Observatory in the morning. On the night before my final oral exam, he insisted on meeting with me at midnight. (I normally go to bed early.) To make matters worse, he decided to use it as an occasion for petting. I moved his hand several times, trying to go on with our conversation. Fortunately, this was the only time I had that problem with him.

I had passed my general oral with no difficulty and was told that the professors were surprised that I had done so well. I was not worried about defending my thesis. After all, I was thoroughly familiar with the work. However, Struve had just written a paper, not yet published, to which some of my results were pertinent. The faculty members had read the paper but I was unaware of it. As a result, much of the questioning in my exam dealt with the relation of my work to the new theory, which made it rough going for me.

A NEW SCIENTIST

After graduation, I stayed on at Yerkes for another six years as an instructor and assistant professor. As the junior faculty member, I was frequently tasked with meeting with unimportant visitors. One reporter when told I was an astronomer replied, "You can't be. You don't have a beard." Another reporter, from a small paper in a neighboring town, was noticeably uncomfortable talking with a woman. He looked longingly at a student who entered the library where we were sitting, obviously preferring to talk to him. When at the end of our conversation I told him that I was an instructor in the department, he was embarrassed and muttered, "You seemed to know what you were talking about."

Because I had access to the Yerkes 40-inch refractor continuously throughout the year, I was able to schedule observations at times I desired. This allowed me to observe double stars when the bright star was at least partially hidden by the fainter member of the pair; that is, the brighter star was eclipsed. I could then estimate characteristics of the faint star that were difficult to observe in the presence of so much light from the brighter star. I had not completed this program by the time I left Yerkes but published the data I had.

My work at Yerkes included teaching and research, both of which I enjoyed. I was too junior to have any PhD students, but I helped several of them. Years later they told me that they had learned more from me than from their formal advisors. I also learned a great deal from other faculty and students as well as from the constant stream of visitors.

Getting a decent salary after graduation was another matter. My salary, as was common for women, was well under two-thirds of that of men at the comparable level. When I complained enough to be promised a raise, it was for $200/year. When I complained again that, with a PhD, I was earning less than the data techs (then called computers) with only a high school education, I was told, "Don't look around." At another time, the chairman of the department, Chandra, said, "We don't discriminate against women—we can just get them for less." The amazing thing about this comment is that Chandra, being dark, had encountered a great deal of discrimination both in England and in the United States, but he did not recognize it against women.

I was finally made an assistant professor but realized that I had no chance of tenure. There was a woman in the University of Chicago Physics Department who received tenure two years before she won the Nobel Prize.

The first summer after I received my PhD, I spent two months at Case Western Reserve's Warner and Swasey Observatory in Cleveland, Ohio. Morgan thought that it would be helpful in the future for me to become familiar with classifying objective prism spectra. The spectra that I had been studying were taken one star at a time. However, it was also possible to image a field of stars through a prism, thus obtaining many spectra at the same time. To avoid overlap, these spectra are more condensed than those taken individually. Thus, classifying them is somewhat different although the basic process is the same.

When I arrived at Warner and Swasey, I was introduced to the faculty. The sequence went Dr. This, Dr. That, etc., Miss Roman. I rarely used the title except at NASA, but being within weeks of receiving the degree, I was hurt by its omission in these introductions. When I started at NASA, I had to use the title to get by the secretaries who were protecting their bosses. It stuck for the rest of my stay there. At my first faculty lunch at Yerkes, I was nearly the last to arrive. When I walked in, the rest of the faculty (all men) stood. I appreciated this gentlemanly act but was embarrassed and scurried for a seat as quickly as I could.

After I returned to Yerkes from the summer at Warner and Swasey, I continued with research on several clusters, using the brightness of various types of stars as I had determined them in my thesis to estimate the distances of the clusters and, from that, the brightness of hotter stars in younger clusters and newly forming associations. About this time, I stated that I expected to live to see a man on the Moon. The general reaction was that I was crazy. My grandmother missed it by a year!

Branching out from observations of clusters, I observed all available naked-eye stars somewhat like the Sun. When I studied them carefully, I noticed that for the same strength of the hydrogen lines, the strength of the lines of heavier elements, which astronomers call "metals," varied from star to star. I could divide the stars into two groups according to the strength of the metal lines. Interestingly, the stars with stronger metal lines moved in nearly circular orbits around the Galactic center and stayed close to the plane of the Milky Way. The stars with weaker metal lines moved in slightly more elliptical orbits and strayed farther from the Galactic plane. Although we had known for some time that stars in very inclined and elliptical orbits had weak lines of metals, this was the first time that anyone noticed that common stars also showed similar, although less marked differences. Stars make any element heavier than helium either in their cores during most of their lifetimes or in the extreme conditions during an explosion of very massive stars. Thus, the stars with fewer metals were older, indicating that the star formation moved closer to circular orbits near the Galactic plane as newer stars formed. My work showed that the common stars that one can see without a telescope had various ages. Because the orbits around the center of the Milky Way varied with their ages, this provided the first clue to the formation of our Galaxy and formed the basis for much later work. The paper in which I published this work (Roman 1950) was selected as one of the 100 most important papers in 100 years of the *Astrophysical Journal*, the major astronomical publication in the world. After sorting the stars according to the appearance of their spectra, I measured the colors of the stars in the violet and in the red. The colors showed the same differences because there are more metal lines in the violet and UV. The stars with weaker metals were therefore slightly bluer in the UV than their colors in the red would indicate. This effect, dubbed the "UV excess," proved to be an easier way to distinguish them than studying their spectra (Roman 1955) and has since been widely used.

These twin discoveries were the most important of my research career. Although he had nothing to do with this work, Morgan tried to take credit for it. Each year, the Vatican sponsored a prestigious invitation-only conference in astronomy. In 1955, it was on stellar populations. Walter Baade coined this term in the 1940s when he discovered that stars near the center of the Andromeda Galaxy were old and red, whereas those in the spiral arms were young and blue. Jan Oort had noticed that stars that circle the center of our Galaxy in very elliptical orbits had fewer heavy elements than most of the stars near the Sun. As my work had greatly expanded the concept of stellar populations, I should have been invited but was not. About 2010, I asked one of the organizers of the meeting why I was not invited. His response confirmed my suspicion: "Morgan felt that he could represent you."

In 1951, my parents bought a new car and gave me their 1939 Packard. This gave me the enjoyable possibility of exploring southern Wisconsin and northern Illinois. I tended to choose back roads, as I did later in the DC area. One day I headed out of an unfamiliar town on a reasonable looking road. As I progressed, the road became narrower and then lost its paving. As I passed a farm, I heard a young boy standing in the yard shout, "Mother, a car!"

In 1953, I finally decided to dispense with the foibles of a prewar car—particularly, getting my legs wet whenever it rained—and bought a new Chevy sedan, the only car I ever bought new. For the first six months, it gave me a series of headaches, but when these were finally repaired, the car gave me thirteen years of good service. I took advantage of having a new car to drive to California in 1954, taking with me three of the computers (data techs) from Yerkes. None of them had been west of the Mississippi before, so I enjoyed not only their company but also their excitement in seeing a very different part of the country.

The techs returned home by train. I spent a month at the Mount Wilson Observatory office in Pasadena and then drove north to San Jose and the Lick Observatory on Mount Hamilton. During the night I spent there, it snowed. The road to the Lick Observatory was built for horses in about 1880. While a modern road has changes in grade from time to time so that the downward momentum of a car can be checked, the Lick road has a steady three-degree grade. I was used to driving on snow by that time but was dreading the trip down the mountain. I would have stayed another night if I could have, but I had to get to McDonald Observatory for an observing run. I was surprised to pass several cars coming up the mountain to enjoy the snow. I also passed several accidents.

OTHER OBSERVATORIES

My trip out West ended with an observing run at McDonald Observatory in west Texas. When I drove long distances, I usually stopped for the night about 5:00 PM when it was still easy to get accommodations. I compensated for the early evenings by starting early in the morning. On the way from California, I left Tucson about 4:00 AM, expecting to buy breakfast on the way. I found no restaurants. Finally, at 10:00 AM, I found a small grocery and bought some food. This drive was the most uncomfortable I remember. The road from Tucson to El Paso ran due east and west. I was driving straight into the rising sun in late March in the clear desert air. I had planned to spend the night in El Paso, but after lunch there, I decided that 1:00 PM was too early to stop, so I drove on to McDonald, driving more than 600 miles alone.

When Texas banker William Johnson McDonald gave the 82-inch reflecting telescope to the University of Texas, the University had no astronomy program. Therefore, it was arranged for the University of Chicago to manage the McDonald Observatory until the University of Texas developed one. As a result, the Yerkes astronomers had full use of the 82-inch and several smaller telescopes. For several years, I spent four months there observing on every clear night. Although this seems like an incredible luxury to today's observer, who competes for several nights at most, observing at that time was less efficient. Photographic plates were much less sensitive than electronic detectors, and telescopes were smaller. I spent four hours on the best nights to obtain narrow spectra of the brightest stars in globular clusters.

Discovery of the differences in common stars led to my interest in the structure of the Milky Way and in the spatial distribution of different types of stars. Therefore, I embarked on an extensive program of obtaining photometry and spectra of a large sample of stars distributed at high latitudes above the Galactic plane and a comparison compilation of stars near the Galactic plane. Most of the stars I selected had proper motions and many had radial velocities. In this way, I could determine not only the distribution of metallicity and velocity with Galactic latitude but also the differences in the kinds of stars in different regions.

I wanted to measure radial velocities of some interesting stars that lacked them, but because the Yerkes astronomers were uninterested in radial velocities, neither Yerkes nor McDonald had a suitable spectrograph. Hence, I wrote to the director of the David Dunlap Observatory north of Toronto to ask if I could use their 72-inch telescope and spectrograph. The director replied that I would be welcome, but could I bring my own photographic plates? This was no problem as I only needed a few dozen and Yerkes had a large supply. The director later told me that photographic plates were a major item in his budget.

Although the faculty and staff welcomed me at the Dunlap Observatory, they later admitted that they were somewhat leery. Another woman astronomer, just a few years older than I, had spent some time with them a year or two earlier. Although I did not learn the details, it was clear that her visit was not a happy one. They seemed pleased with my stay and, when I left, presented me with a handmade silver pin designed by one of the faculty with the provincial flower, the lily of the valley.

In the course of my work on stars at different Galactic latitudes, I observed a star that, according to the catalog, I expected to look like the Sun. It did not look at all like the Sun! At first, I thought that I had observed the wrong star. Stars look pretty much alike even through a telescope. I was very careful the next night to be sure I had the correct star. It looked the same as the night before. I observed it with another spectrograph and, when I returned to Yerkes from McDonald, I published a two-page note about the star, known as AG Draconis (AG Dra), but otherwise went on with my normal program. Little did I realize that that observation would change my life.

LOVE

During my time at Yerkes, a postdoc arrived from Scotland for two years. We started to attend a weekly square dance at the grange hall in a nearby town. From this, we started to do other things together, including British crossword puzzles, which I was not good at, and table tennis. I fell sufficiently deeply in love that I seriously considered giving up astronomy to marry him. (At that time, it would have been difficult for both of us to have a career in astronomy although several couples succeeded.) However, this did not work out. I do not know if he was as deeply in love as I, but he was not in a position to marry. His fellowship required that he return to the British Commonwealth for two years. It was six months before he finally found a position in Australia. We wrote regularly for some time but the frequency gradually diminished, and I lost track of him eventually.

A year or two later, my best friend announced her engagement. I was happy for her. She was marrying a fine man. Nevertheless, it made me wonder what would happen to me. A month or so later, I was surprised by a proposal from a man with whom I had not spent much time alone. I liked the man and took the offer seriously but after a month concluded that he was not someone with whom I wanted to live closely for the rest of my life. We remained friends until he died, but I have never been sorry I did not marry him.

Morgan wanted me to stay at Yerkes. Though not willing to pay me a reasonable salary, he also saw to it that I received no information on other positions that might have been of interest. I realized that I had no chance of tenure at Yerkes and hence could not stay there more than a few more years. I had had trouble with my thesis advisor throughout our relationship. It was clear that he respected my ability, but he tried to use it to further his own reputation. He was pleasant to me when he thought it would be to his advantage but treated me like dirt at other times. His mood during my graduate school years made life difficult.

When another of the professors, Kuiper, told me of an opening in radio astronomy at the US Naval Research Laboratory (NRL), I was happy to take it and left Wisconsin for Washington, DC, where I have lived ever since. I believed that radio astronomy, which was then new in this country, had possibilities for advancing the understanding of Galactic structure, in which I was interested. I was correct, but I was too early. The state of the art was then inadequate. Also at that time, you were expected to design and build your own instrumentation, and I did not want to start over as an electronic engineer. Today, most radio astronomers use equipment built by specialists; one was surprised when I mentioned that I had been expected to build my own.

By the time I started at NRL, the person who hired me had moved to another branch to work on Vanguard, the first civilian satellite. I assumed that since they hired me, they had work they wanted me to do, but no one gave me any direction despite my questions. I had brought work with me and I was happy to have some opportunity to become more familiar with a new field. Eventually, I worked myself into the activities of the branch and believe that I became a useful member of the team. Years later, one of the men who had been in the branch at that time explained my reception. They had had another woman whom they felt had been useless. The last thing they wanted was another woman! (The other woman went on to have a successful career in radio astronomy, but she had not finished her education before coming to the NRL.)

I found the government appreciably more tolerant of women than the university community and ran into little obvious discrimination in my government career. Instead, after about three months, the branch head told me that I was at the wrong level and arranged a raise for me. I realized later, when I was more familiar with Civil Service, that my salary prior to Civil Service had been so low that it was not recognized as professional experience. Although I had six years of research and teaching experience after my PhD and an international reputation, I had been hired as a new PhD. Even at that level, my new salary was only half again what I had been receiving upon hire.

BEING EARLY

I have managed to be early much of my life. Of course, the first instance was, as a girl, wanting to be a scientist. I was also too early to be really effective in radio astronomy. I was also early in less major ways. In 1953, I heard of a new type of machine that had been developed at Argonne Laboratories for measuring positions on photographic plates. The traditional machines required the measurer to bisect an image with a cross hair while looking through a microscope. The Argonne machine showed on a cathode ray screen a trace across the image and a reverse of this trace. The measurer simply superimposed the two scans. I visited Argonne several times to measure a variety of kinds of images. I was convinced that this was both easier and more accurate than the earlier method, but I could not convince anyone else at Yerkes to get or make such a machine. Within a couple of years, others discovered the method and it became standard for measuring astronomical plates until photographic plates were replaced by digital detectors.

By 1954, computers were beginning to enter the science establishment. I suggested to Chandra that I would like to use a computer to reduce photometric measurements. He replied firmly, "That is not what computers are for." Of course, today, no one would dream of reducing photometric data any other way. A year after joining the NRL, I programmed the NAREC and then an IBM 610 for the job.

NEW KID ON THE BLOCK

NASA was born on October 1, 1958, with employees and structure transferred from the National Advisory Committee on Aeronautics (NACA). The NACA was a 31-year-old respected engineering organization that had been responsible for many of the achievements of the US aviation industry. Although NASA was and is still essentially an engineering organization, room was needed in the civilian agency for scientific space exploration, which had up to that time been supported primarily by the military. To meet this need, a large portion of the Rocket Branch at the NRL was transferred to NASA to form a new science center, the Goddard Space Flight Center in suburban Washington, DC. The Vanguard program, the first US effort to develop a civilian satellite, was also transferred.

Here is where that old observation of AG Dra turned out to be very important. Three years after joining the NRL, I was one of three Americans invited to the dedication of an observatory in Armenia. It turned out that I had been invited because the director of the new observatory was intrigued by my note about AG Dra. I was a replacement for another invitee, so I had only four weeks to get permission from the naval hierarchy to go on the trip. As I carried my papers from office to office, many people ended up hearing that I was going. After my return, NRL leaders asked me to give a talk about my trip and later to give a series of astronomy lectures. As a result, I became widely known.

When NASA was formed two years later, most of the science section came from NRL. The leaders of this group, who knew me because of my trip, asked whether I knew anyone who wanted to set up a program in space astronomy, which I interpreted as an invitation to apply. Although I did not want to stay in radio astronomy, I was not sure I wanted this NASA position. I realized that taking a management position would mean giving up research. I had already left teaching, which I enjoyed, when I left the University of Chicago and was not sure that I wanted to give up research, which I also enjoyed. However, the chance to start with a clean slate to map out a program that I thought would influence astronomy for fifty years was more than I could resist. I joined NASA in late February 1959 as Head of Observational Astronomy.

Parenthetically, we now know that the star that changed my life is in the unusual state in which I found it for only about 100 days every 10 or 15 years or so. Finding it was a stroke of luck. But equally important was that I recognized that it was interesting, and that I took advantage of the opportunities that my stroke of luck brought my way.

My three and a half years at the NRL were a valuable preparation for my NASA responsibilities. Not only did they give me an understanding of a different spectral region than I had worked in previously, and hence appreciation of other regions as well, but also I became used to working and communicating with engineers. I felt that one of my roles early in NASA was acting as an interpreter between scientists and engineers. Although often grouped interchangeably, their approaches to problems are quite different. Scientists ask "What?" and "Why?"; engineers ask "How?" In addition, they have different educations and backgrounds and a somewhat different vocabulary. This sometimes leads to difficulty in exchanging ideas.

NASA was a great place to work in its early years. Most of the professional staff in Headquarters was composed of the cream of engineers from the NACA. Everyone was gung ho. There was no bureaucracy. Furthermore, the priority of the Apollo program made money less tight. Once, I wanted to do something unusual. I no longer remember what it was, but I called someone in the grants office to find out if I could do it. The reply was memorable: "Don't ask me what you can do. Tell me what you want to do. It is up to me to find a way."

Astronomy was already underway in NASA when I joined. Gerhardt Schilling was serving as Chief of Astronomy and John O'Keefe, an astronomer from the Theoretical Branch at Goddard, was working part time to organize the program. Schilling was primarily a manager. I learned from him as I stepped into a major management position, but he was weak in astronomy. Although officially I was responsible only for optical and UV stellar astronomy, I became involved in the entire astronomy program. Only the cool Solar System components were excluded from my purview. For example, I was as involved in the Orbiting Solar Observatories (OSOs) and in the Orbiting Astronomical Observatories (OAOs). I also worked with William Kraushaar on the development of the first gamma-ray satellite. 1959 marked the beginning of a productive program in all spectral regions except the IR, in addition to geodesy and relativity. It also provided important education for my next position. In early 1960, Schilling left, and I took over as Chief of Astronomy, formally becoming responsible for the science for the entire program of studying phenomena beyond the vicinity of the Earth, except for the cold bodies in the Solar System.

In 1960, Jack Clark and I attended an excellent weeklong program in Williamsburg, Virginia, run by the Brookings Institution on government and government policy, that I think is one of the educational highlights of my life. There were 10 or 12 of us in the class, of whom I remember only Lou Branscomb, who later reached high levels in science policy. Basically, they were all people in science administration in government. There were several speakers, some from government and some from the academic area, who tried to give us an understanding of science as seen from the political system, the political system as it affects science, a historical perspective on the interaction of science and politics, and that sort of thing. I do not remember many details, but it was a very interesting session. Jack asked one of the speakers, "How long does it take a new agency to become bureaucratic?" The reply was, "It starts in 5 years and in 10 years, it is complete." I watched this happen in NASA. His prediction was excellent. In 5 years, we were beginning to require more paper work to justify our actions, and by 10 years, more formal review procedures for the Supporting Research and Technology (SR&T) funding and flight missions. The paper work required to get anything done continued to increase as did the time required to get a project approved.

I realize that gender discrimination existed in the federal government, as it did elsewhere, but I was not affected by it personally. Homer Newell, the first Associate Administrator of Science and Applications, was as fair a man as I have known. I do not believe that he would have tolerated discrimination against either women or African Americans. I thought that one scientist was practicing gender discrimination but learned that he was treating the men the same way. I may have run into a glass ceiling, but I am not sure I had the diplomatic skills for higher office. Robert Zimmerman (2008, p. 36), in his book about the *Hubble Space Telescope*, titled *The Universe in a Mirror: The Saga of the Hubble Space Telescope and the Visionaries Who Built It*, states with reference to me: "Her hard-nosed and realistic manner of approving or denying research projects had made her disliked by many in the astronomical community." However, I would not have gotten as far as I did if I had not been stubborn. I recognized early that I was not a good diplomat. I tend to state things as I see them without softening my comments. I try to treat everyone equally without attention to prestige or political power. In the words of another astronomer quoted by Zimmerman (2008, p. 36), "She would tell people some truths to their face in undiplomatic language, and she didn't do herself well by doing that." Though I try to listen to conflicting opinions and lead decisions in meetings to an agreed consensus, I keep the discussions on the topic. As a result, people generally liked serving on committees I chaired because they did not feel that there was a lot of wasted time.

Looking back, I realize that I had incredible freedom in handling my program. A requirement of formal peer review only began 10 or 15 years later. I decided on the acceptance or rejection of proposals largely on the basis of my own knowledge of the field and of the capabilities of the members of the still small community, what I understood of the priorities in that community, and finally, of course, the quality of the proposal and the availability of funds.

I organized a committee to help guide the program that soon morphed into the Astronomy Subcommittee of the Space Science Steering Committee. The latter was an in-house committee established later that reviewed the various proposed science missions. There were parallel subcommittees in other science disciplines as well. In the beginning, my committee was composed of NASA and Jet Propulsion Laboratory employees, but I soon brought in astronomers from the academic community.

Although the nominal funding authorizer was several levels above me, my decisions to fund rocket programs or ground-based activities to support the space observations were rarely questioned and never overturned. I was questioned on a proposal I rejected from a Nobel Prize winner. I explained that it was clear from the proposal that he was not going to be involved in the work and that the proposal did not indicate that the postdoc who would do the work had the necessary background. My explanation was readily accepted.

Most graduate education is directed toward research and academic positions with no background in management, and so I came into the NASA job with no management training. The one item I remember from Schilling's instruction was the importance of keeping a notebook, but I am sure his guidance was helpful in other ways as well. Much of my experience was "learning to swim by jumping into deep water." When you learn to swim this way, you either drown or start to dog paddle without really thinking about it. The same was true of my management experience. It was primarily a case of trial and error. If one approach did not work, I tried something else. I had two advantages: The people I was working with very much wanted to succeed, and once the projects were well underway, the responsibility for details became the purview of a NASA center.

One management area that gave me a problem was strategic planning. In 1959, before NASA was nine months old, I had to develop a 10-year plan. Although this is a standard and important management tool, I found it difficult. Obviously, I could not base it on experience. Furthermore, neither I nor others had any understanding of the technical problems we were to run into as we learned how to do astronomy from space. I do not remember the details of the plan, but I am sure it was overly optimistic. For example, at that time, we planned to launch the first OAO in 1962. Instead the launch slipped to 1966 and without half of the planned telescopes, which could not be ready in time for even the late launch.

Unlike research, management involved many meetings, both one-on-one and in groups. I had many visits from various industry representatives wanting to become involved in the NASA programs in addition to meetings more directly related to my program. As a slow writer, I found that in such meetings I could either listen or take notes. At that time, there was a course in Gregg shorthand on television. I decided that that could be the solution to my problem. My shorthand was far from secretarial quality in either speed or accuracy and my writing became a mixture of shorthand and script, but shorthand helped greatly.

The science portion of NASA Headquarters had a parallel structure of scientists and engineers. Scientists were responsible for the program planning and content within the available budget. Once a satellite mission was accepted for planning, an engineer was assigned to the project. The scientist and the engineer worked together closely as a team. The engineer was responsible for the general design and cost estimates. When a flight mission reached the stage of detailed planning, it was assigned to a field center, but the Headquarters team retained oversight. The scientist continued to work with the principal investigators (PIs). These PIs were the scientists who had proposed the mission and who had the responsibility for the scientific instrumentation and for obtaining and analyzing the data. The science supervisor also maintained a general overview of the project, with emphasis on the likelihood that the engineering would meet the scientific needs. The engineer had the responsibility for overseeing the engineering development and for schedule and cost control. The same system prevailed at higher levels also. If the director of the office of Astronomy and Geophysics was a scientist, as Jack Clark was, the deputy was an engineer and vice versa. There was no set pattern as to whether an engineer or a scientist would be the director. Goddard had a similar arrangement. For the OAOs, my counterpart was the engineer Dixon Ashworth.

The program scientist's responsibilities could be divided into two primary areas: the satellite and rocket program and SR&T. The latter covered a broad range of responsibilities, from theoretical research to flight mission components and balloon flights. In 1959, the only missions for which I had responsibility were the OAOs, a series of satellites designed to observe astronomical objects in the UV. Much of my activity involved SR&T. Part of my responsibilities were reviewing and selecting for funding proposals received from the scientific and engineering communities, including the NASA centers. The rest was investigating and supporting the development of techniques that were needed for space astronomy research. Much of the contracting for technological developments was handled by the centers, but I also participated in encouraging needed research.

I had been granted classified clearance at the NRL and retained it during my time at NASA, but I had almost no contact with classified information except for launch dates. If you contract to have a house built, you are not surprised if it is not finished by the predicted date even though house building is an established art. NASA was trying to learn a completely new process with unexpected problems arising all along. Thus, it is hardly surprising that there were unpredicted delays. Yet, if NASA did not launch on the announced date, the media crucified the agency. To remedy this problem, NASA classified launch dates for several years. If you read launch announcements carefully now, you will almost always find the phrase "not before" or "on or after."

OBSERVATORIES IN SPACE

The first satellite launch in my program was the OSO. John Lindsey, who had studied in the rocket program at the University of Colorado, Boulder, designed an ingenious satellite for obtaining spectra of the Sun in the UV. The fact that the Sun is bright made it easy to find, and once it was detected it was unnecessary to point to any other object. The spacecraft Lindsey designed was a flat rotating wheel that behaved like a gyroscope. Three weight-bearing arms were added to increase the inertia of the wheel. Two spectrographs were driven against this rotation to remain pointed to the Sun. There were six pie-shaped compartments in the wheel. Three of these were used for spacecraft functions such as power supplies and communication links; the others carried experiments that observed the Solar intensity in the gamma- and X-ray regions as well as the UV. These compartments could also be used for sky surveys. The first flight of the OSOs in May 1962 was followed by a quite successful series that continued with steady improvements for nine missions. An improved version of the Explorer 11 instrument flown on OSO III provided the first successful cosmic gamma-ray observations of the sky.

My second big space program was the OAO, which had its roots in the period from July 1957 to December 1958, when scientists organized the International Geophysical Year. During the Year, they emphasized projects in geophysics, many of which required international cooperation for their success. In the United States, the National Academy of Sciences (NAS) served as the lead coordinator. As part of this, the United States developed the Vanguard satellite. Although the military had developed satellite launchers, Eisenhower thought that there should be a civilian-version agency that could lead to the general development of space.

As an outgrowth of the NASA activity in the International Geophysical Year, the growing possibility of experiments in space led to the formation of the Space Science Board in early 1958. In the spring, the chairman of the Board, Lloyd Berkner, broadcast a wide appeal for proposals for spacecraft, with responses required within one week. These were to be in the range of 100 pounds or less and sufficiently developed that they could be prepared for final environmental test by mid-1959. There were 30 responses from astronomers for Solar and stellar UV studies. Four of these responses formed the basis for the planning of the OAO missions, only one of which might have met the tight schedule.

My role in the OAOs dates back to my first major meeting at NASA. I met with the proposers for each experiment, including one PI and, usually, several co-investigators (Co-Is), each with a responsibility for part of the development and investigation. This meeting involved Fred Whipple (Smithsonian Institution), Lyman Spitzer (Princeton University), Arthur Code (University of Wisconsin), Leo Goldberg (Harvard University), James Kupperian (Goddard), and others. I believe James Kupperian led the meeting.

The experiments planned for the OAOs included a map of the sky in several UV colors, a photometer to measure the brightness of stars and galaxies in the UV, a UV stellar spectrograph, and a Solar UV camera. A medium-resolution spectrometer proposed by Albert Boggess from Goddard also became part of the planning shortly thereafter. The development of a standard spacecraft that could point anywhere in the sky and hold the pointing accurately for many minutes was more challenging than the development of its Solar sibling.

NASA decided, before I was involved, that the instruments should fly on a standard spacecraft as a series of OAOs. Two of the experiments used a moderately large telescope; the others could benefit by flying several smaller telescopes together, although the original proposals were for single instruments. It was clear that the proposers of these experiments would each have preferred to fly on a satellite designed to meet their needs and under their complete direction. Because of his experience in the military, Code was a competent instrumentalist. The instrument he proposed was a straightforward extension of ground-based instruments, although I suspect that even he might have found designing a satellite with extraordinary pointing requirements beyond his skill. As illustrated by the later problems with Celescope (the SAO experiment) and Stratoscope (a Princeton balloon telescope), neither Whipple nor Spitzer understood the difficulty of producing a satellite that could satisfy their scientific needs.

This illustrates a difference between the National Science Foundation (NSF) and NASA. For the NSF, experimenters did not have to manage complex programs involving many interfaces or to meet launch dates. Thus, the NSF provided funds and left the astronomers to handle their projects independently. For the national observatories, which had more in common with NASA flight projects, the NSF funded independent management organizations. The early NASA PIs were not happy with the degree of NASA management, and the astronomical community generally continued to be unhappy with NASA's authority in the management of the astronomy program. As part of this management, I visited many of the organizations I funded to review the progress on the grants as well as to discuss ongoing plans.

It soon became obvious that the thermal problems for the Solar satellite were so different from those for the stellar instruments that it did not make sense to use the same spacecraft design for the Sun and for the stars. Because Goldberg's instrument was too large for an OSO, the Solar mission was postponed to an Advanced OSO, a satellite comparable with the OAOs in size but designed for the Solar thermal environment. This satellite was never developed. Although the successful Skylab program conducted most of the proposed science and much more, the original proposer, Leo Goldberg, was not involved. This left three missions that had been proposed to NASA, plus Boggess's UV stellar spectrograph from Goddard.

The desirability of a series of similar spacecraft versus individual satellites more strictly tailored to specific missions was debated throughout my time at NASA. In theory, and to some extent in practice, the use of a standard spacecraft avoids "reinventing the wheel," but in practice each mission requires greater or lesser modifications. Also, the results may not be ideal for the instrument design and use. Although series such as the OSOs were nominally successful, each mission was somewhat modified from the preceding ones.

I have already referred to the challenge of getting astronomical spacecraft to point accurately. A few successful observations resulted from spinning rockets, but other than a crude search to find out if there is something there, I don't think it was possible to do much in non-Solar astronomy until you had three-axis pointing controls. This is not difficult on the ground with the Earth as a stable reference platform but is not easy to achieve in orbit. It was necessary to use significant angular momentum to turn from one direction to another and then quiet the spacecraft completely. It was also necessary to ensure that the spacecraft was pointing accurately in the desired direction when it stopped. The British Skylark rocket had a three-axis stabilization system by 1964, and the United States probably developed one about the same time. I do not think I funded any specific research on pointing controls, but I worked with engineers in the NASA centers to learn what they knew and to encourage them to meet the astronomical needs. The Ames Research Center, under the direction of Harry Goett, who a little later became the first director of Goddard, became deeply involved in this problem. The portion of the Army Ballistic Missile Agency that was later transferred to NASA as the George C. Marshall Space Flight Center was also interested in a pointable stabilized platform.

Although I talked to engineers at both Ames and the Army Ballistic Missile Agency about possible solutions to this problem for the OAOs and about ways of testing the solutions on the ground, detailed design was the responsibility of the engineers at Goddard, who were generally responsible for the spacecraft. My role was primarily to inform the engineers of the astronomical requirements. Stabilizing the spacecraft and pointing to a specific direction turned out to be a multistep process. As the satellite rotated after launch, the satellite spotted the Sun, and the spin was reduced to only rotating around this direction. Next, the satellite detected one or more bright stars that told it its orientation. The spin was stopped and, on the basis of the known orientation, the satellite moved from the Sun to the direction of the first star to be observed. The observed star field was then matched to the expected field to locate the target, and a fine tracker kept the pointing steady in the proper direction as long as the Earth did not get in the way.

Another technical problem for space astronomy was the need for sensitive high-resolution detectors. The lack of a satisfactory imaging detector continued to plague space astronomy until the Wide Field/Planetary Camera was developed for *Hubble*. On the ground, astronomers relied on photography, as they had for the entire twentieth century, but it was not practical to get conventional photographs from space. The military had caught film dropped from orbit, but this technique was too expensive for astronomers. The Russians, on a lunar flight, developed the film on board the spacecraft and then digitized the image, but the results were not particularly satisfactory.

Photomultipliers worked well in space but had no spatial resolution. A photomultiplier measures the intensity of a photon stream from a small area of sky. The photons release a stream of electrons from a photosensitive surface. Each electron is then accelerated in an electric field to create several electrons at a detector. By preceding the photomultiplier by filters, it is possible to measure the intensity of a source in various spectral regions. For imaging, cathode ray tubes, particularly a version of the vidicon, appeared to provide a solution. These are the type of detectors that were used for television and, thus, had had substantial commercial development. However, commercial uses did not include the detection of faint signals or the demands of UV astronomy. Developing vidicons that met the astronomical requirements proved much more difficult than we originally expected. These image tubes also had other problems including variable image distortion and calibration difficulties. However, they were successfully used for the *International Ultraviolet Explorer* (IUE) in the late 1970s and for the Faint Object Camera on the *Hubble*.

Because of the need for a satisfactory imaging detector, the astronomy program started to fund various approaches to detectors in 1959, continuing the search until the adoption of a charge coupled device (CCD) for the *Hubble* led to their general use both in space and on the ground. The CCD is a solid-state detector made of a matrix of tiny sections of a solid-state detector (pixels), each of which acts as a separate photomultiplier. The measure of the intensity in each pixel is transferred down each column. The timing of each section of the readout provides the position in the column from which each intensity measurement comes.

By the end of my first year at NASA, the OSO program was going smoothly, and the small rocket program was proceeding largely on momentum, but the OAO program was another matter. It was difficult to get the various astronomical groups to settle on the requirements for the spacecraft without squabbling over detailed designs. There was the question of whether two experiments might share a single spacecraft, looking out in opposite directions. Much of the community was skeptical of the capabilities of the Goddard astronomers, although they had more rocket flight experience than the others. By the fall of 1960, Abe Silverstein, director of the Office of Space Flight Programs, was quite angry with me for not getting the OAO program successfully organized. He demanded that I develop a schedule for the program and that Goddard certify that the schedule was doable. I do not remember how I responded or what help I had from others, but the program did go ahead. A major problem was the order of the missions. Each investigator wanted to fly first to get the "scoop" on new discoveries. Obviously, that was impossible. The final schedule was based on the pointing requirements, with the mission with the least demanding pointing flying first.

The first OAO was finally launched on Good Friday, 1966. Because of a problem in the power supply, it died on Easter. After working on the mission for seven years, I was so dejected that I visited a friend in the hospital to cheer myself up. While there were no scientific results from the flight, the engineers learned a great deal, and the satellite was extensively redesigned for the next flight, which occurred in 1968. The satellite worked well for more than four years. The University of Wisconsin experiment observed over 1,200 objects in the UV for the first time—objects including planets, comets, a variety of stars, star clusters, and galaxies. Among the University of Wisconsin results from OAO-2 are the discovery that comets are surrounded by huge hydrogen halos; evidence that at least some novae increase their UV brightness while their visible light is fading rapidly; and the realization that galaxies are systematically brighter in the UV than expected from the visual colors of their stars. OAO data were used to investigate the physical properties of interstellar dust and to map the distribution of hydrogen near the Sun; OAO data combined with measurements of angular diameters of stars enabled the first empirical determinations of the temperatures of the hotter stars.

The Smithsonian OAO experiment met its goal of producing maps of the sky in four UV colors. An extensive catalog of stars was produced from these data. Nevertheless, the imperfections of the vidicon impacted the quality of the catalog. A few years later, I received a proposal from Katherine

Haramundanis requesting money to massage the data further. It was accompanied by a letter from Cecilia Gaposchkin, her mother, supporting the proposal. I did not think that the quality of the data merited further work and rejected the proposal. Somewhat later, I met Cecilia, who told me, “You were right to reject that proposal.” “But you supported it.” “She’s my daughter,” she said. “What could I do?”

The Goddard OAO experiment, a medium-resolution spectrograph, was launched separately in 1970. Unfortunately, the rocket shield failed to come off because a technician tightened a bolt too much, and the satellite failed to achieve orbit, thanks to the excess weight. This satellite would have used a two-dimensional pulse-counting detector. Although there was a back-up for the instrument, funds were too tight for another flight.

The most successful OAO was probably OAO-3, the Princeton experiment named “Copernicus” after its launch in 1972 on the 500th anniversary of the birth of Copernicus. This mission obtained high-resolution spectra of many stars in the UV and provided information at the shortest wavelengths reached for many years. To avoid detector problems, the experiment used two photomultipliers to scan the spectrum. Although inefficient, these gave high-quality data. The satellite remained productive for 8.5 years.

In 1964, the planned schedule included 10 OAO launches and a manned orbiting telescope, a larger version of the OAO flown in connection with the manned program. This was to be an intermediate step toward the Large Space Telescope (LST).

Although much of the emphasis in the astronomy program in 1959 was on observations in the UV, which could not be observed from the ground, there was a start on observations at even higher energies. Although nominally I was only responsible for the UV stellar observations, I became involved in a much larger range of activities. We began the development of the first astronomical satellite, Explorer 11, a survey of the sky in gamma rays. This was a small experiment that had been proposed earlier to both the NSF and the Space Science Board and was an extension of observations of cosmic rays and the attempted observations of gamma rays from balloons. Gamma rays were received from several sources that later proved to be gamma-ray emitters, but, as only a few photons were observed from each, the detections were far from definitive. The theoretical prediction of the intensities of celestial gamma-ray sources was overoptimistic by a factor of 100!

GESTATION OF THE *HUBBLE*[1]

Looking through the atmosphere is like looking through a piece of old stained glass. The glass has defects that distort the image. The atmosphere also has defects that distort the image, but the defects in the atmosphere move, thus blurring the image as well. The glass is colored so only some colors get through. Until the mid-twentieth century, that did not appear to be a major problem. Stars primarily radiated like black bodies and their temperatures were such that their radiation can come through the atmosphere and our eyes are adapted to seeing it. The development of radio astronomy as a result of the technology stimulated by World War II proved that the Universe was far more complex and far more interesting than the staid view in the visible band. This made astronomers eager to detect colors that do not come through atmosphere. Finally, the glass is dusty. The dust scatters light, making the background brighter and harder to see through. The molecules in the atmosphere also scatter light. This is why we cannot see stars in the daytime. It also keeps us from seeing the faintest stars at night. Finally, unlike the glass, the atmosphere shines faintly, also making the faintest objects invisible from the ground.

[1]Note added by the Editors: Much of the material appearing in this section appeared previously in Launius & Devorkin (2014), reused with permission.

For these reasons, astronomers had been anxious for decades to put telescopes above the atmosphere, and they jumped at the opportunity provided by the opening of the Space Age. The first NASA astronomy missions were to hunt for high-energy radiation in gamma-ray and X-ray regions of the spectrum. These searches relied on techniques that had been developed over decades for the measurement of cosmic rays and for studying high-energy phenomena in laboratories.

We knew from rocket observations that the Sun displayed interesting effects in the UV that changed continuously. This was an impetus behind the OSOs. Stellar astronomers were also interested in the UV, as young, massive stars emit most of their energy in that region. In addition, the strongest and simplest atomic transitions of common light elements are in the UV. Without observations of these lines, it was impossible to measure the compositions of stars. These arguments led to the development of the OAOs with their emphasis on the UV of stars. We were less interested in the IR at that time, and detector technology was too primitive to make this region easily accessible.

These instruments provided an exciting introduction to space astronomy, but astronomical objects are very distant. That makes them appear faint and tiny. A large mirror is required to collect enough light to analyze any but the brightest stars. The fineness of the detail that is discernible is also a direct function of the size of the mirror. Thus, taking advantage of the dark sky and steady images above the atmosphere requires a large mirror. For decades, astronomers had longed for a large space telescope. In 1946, Lyman Spitzer wrote a short paper for the Rand Corporation describing the science that could be learned with a 4-m telescope in space. This article is generally considered the impetus for such a telescope in the United States.

From time to time, NASA asks the National Academy of Sciences (NAS) for advice on its science programs. In the summer of 1962, the Academy assembled a group of scientists at the University of Iowa, dividing the group into various committees representing different areas of science, including one for astronomy. One astronomer, Aden Meinel, had determined that the Saturn rocket could carry a 3-m telescope. The entire astronomy committee jumped on the idea. That is what they really wanted. I thought that it was too early to start work on such a project. I knew how much trouble we were having trying to develop a satellite and instrumentation for a 20-cm telescope, and this telescope was not to be successful until 1968. Thus, I essentially ignored the idea.

At that time, the Langley Research Center was responsible for NASA's human space program. Some of the engineers there jumped on the idea of developing a large manned orbiting telescope. The NAS conducted another study in the summer of 1965. By this time, the astronomers only argued about whether the telescope should be in orbit or on the Moon. The latter would provide a stable base, making the telescope less sensitive to the motion of parts, and also provide a reference system for the pointing controls. Connected to a manned base, it could be used much as ground-based telescopes are used. However, there were also disadvantages to the Moon. Perhaps the most serious was that it was unclear how soon such an installation would be feasible, as the Moon appeared to be undesirably dusty. Furthermore, its motion is complex, making the guidance difficult before modern computers were well developed. Nevertheless, the issue remained alive until the early 1970s.

Several aerospace companies were intrigued by the Langley idea and presented designs for a manned large space telescope. This was the last thing astronomers wanted! Aside from the fact that research had not been done by a person actually *looking* through a telescope for almost a century (with one small exception), a man needed an atmosphere and that was what we were trying to get away from. In addition, a man would wiggle during long exposures and that would cause the telescope floating in orbit to wiggle in the opposite direction, blurring the image. I still thought it was too early to design a satellite for a 3-m telescope but decided that if companies were going to spend money designing such a satellite system, they might as well design a usable one.

A major problem at this stage was winning the support of the general astronomical community, many of whom as yet had no interest in observations from space. One facet of attacking this problem was to set up a working group under the auspices of the NAS under the direction of Lyman Spitzer on the uses of an LST. The committee held an early meeting in Pasadena to discuss the use of such a telescope for studies of galaxies, cosmology, and interstellar matter. Numerous West Coast astronomers attended the meeting, increasing their understanding of the possibilities and, hence, somewhat decreasing their antipathy. Although the members of the working group were supporters, the cachet of the NAS gave their report, which was published in 1969, special importance. I met with many astronomers to discuss the promise of a 3-m telescope above the atmosphere. In addition, I gave many illustrated public talks on the questions that we expected such a telescope to answer, although I also emphasized that the most important results would be those we could not predict.

The Astronomy Working Group that had been established to advise me on the entire astronomy program also started to discuss what was really needed for a successful LST and the engineering problems that required solution. By 1971, I assembled a Science Steering Group to work only on the LST. For this, I invited some NASA engineers and a group of astronomers from all over the country representing various interests that could be served by a large space telescope to sit down and outline a design that would meet the needs and would be doable. Purposely, I included several who were not really enthusiastic about the project but whose science could benefit from the program. Together, we sketched the system that would become the basis for the *Hubble*.

After about two years, a more detailed design was needed. The Marshall Space Flight Center was assigned the responsibility for turning our sketch into a design. I maintained a general overview of the continued developments as Program Scientist, but Robert O'Dell was hired in September 1972 as the Project Scientist with the detailed responsibility for keeping the scientific requirements at the center of the planning.

At one point, there was a strong push to decrease the diameter of the mirror, probably to make use of facilities that existed for other purposes. We were asked to consider mirror sizes of 2.4 m and 1.8 m. A primary objective of the telescope was to determine the brightness of Cepheid variables in the Virgo cluster of galaxies. Edwin Hubble had shown that the velocity of recession of distant galaxies was proportional to their distance. However, the proportionality constant was still uncertain by a factor of two. Galaxies have random motions. For distant galaxies these random velocities are small compared to the velocity caused by expansion, but for nearby galaxies, these random motions overwhelm the general expansion. Furthermore, the nearby galaxies are in a group in which they interact gravitationally. To determine the proportionality constant, it was necessary to determine the distance of a cluster of galaxies not interacting with nearby galaxies and distant enough that the random velocities are not significant. The nearest suitable cluster is the Virgo Cluster of galaxies at a distance of about 54 million light years. Henrietta Leavitt had shown that the brightness of a particular class of variable stars, called Cepheids, was an accurate function of their period of variation. We could calibrate this relation for Cepheids in the Milky Way. Then, if we could observe these variables in Virgo, we could determine its distance. Measuring the expansion velocity of Virgo was easy. I and, independently, several others determined that with the available detectors, we could reach the Cepheid variables in Virgo with a 2.4-m mirror but that we could not do so with a 1.8-m mirror. Dropping the mirror diameter to 2.4 m also made the design of a satellite that would fit the Space Shuttle easier.

As the early design developed, it was necessary to make a place for the project in the NASA plans. It was relatively easy to convince my superiors in NASA that such a telescope would be worth the cost. Convincing the political community, who had little understanding of science, was more difficult. James Webb, the administrator of NASA at that time, gave a series of dinners for men with political power. After each dinner, three of us presented a "dog and pony show." Jesse Mitchell discussed the engineering and its feasibility, Dick Halpern presented the management plans, and I described the scientific research we expected to do with the telescope. I never testified in Congress, but I did write congressional testimony over 10 years to justify the LST. I also pitched the case for the telescope to representatives of the Bureau of the Budget (now the Office of Management and Budget), the agency that prepares the budget that the president sends to Congress. At some point, for political reasons, the word "large" was dropped from the name with the satellite simply becoming the "Space Telescope" (ST) until launch.

In spite of these efforts, Congress continuously postponed approval for construction. Even after construction was started, Congress cut the budget below an optimum level. Of course, this increased the final cost of the mission. In the early to mid-1970s, astronomers organized a major lobbying effort, which finally led to the approval of the project. At one point, Senator William Proxmire, noted for ridiculing government funding that he considered frivolous, asked NASA why the American taxpayer should support an expensive telescope. I did a back-of-envelope calculation and determined that for the cost of one night at the movies, every American would have fifteen years of exciting discoveries. I was probably off by a factor of four or five, depending on how launch and servicing costs are allocated, but we have had 28 years of discoveries. Even at a cost of a night at the movies once a year, which would more than cover costs by any accounting, I believe that most Americans believe that the expenditure has been worth it.

At the time the *Hubble* was being designed, NASA was pitching the Space Shuttle as a cheap way to launch spacecraft. To lower the costs, a busy launch schedule was required. Therefore, all satellites were designed to be launched by the Shuttle, and several were also designed to be serviceable. The *Hubble* was scheduled to be launched by the next flight after the *Challenger* accident. That catastrophe cancelled all Shuttle launches for three years, during which the satellite was kept in storage and a knowledgeable group of engineers was kept on the payroll until the 1990 launch. These three wasted years also added significantly to the cost of the mission. The *Challenger* experience caused NASA to rethink its use of the Shuttle for most space missions, and most payloads had to be redesigned for robotic launches. Fortunately, the *Hubble* was too far along to be changed. The ability to service it with the Shuttle not only saved the basic mission after the mirror problem was discovered but also provided the possibility of replacing instruments from time to time with more modern versions, thus greatly increasing the capability of the telescope.

As mentioned earlier, I started funding development of detectors early in the ST program. A major portion of the funding for UV detectors went to Princeton, which subcontracted to Westinghouse for the development of an intensified vidicon for the telescope camera. The Steering Group and later the Working Group assumed that this detector was already chosen. As the time approached for the selection of the scientific instruments for the telescope, I was dissatisfied with the progress on the intensified vidicon. At a Steering Group meeting shortly before the selection of the instruments, I arranged a presentation of various types of detectors. CCDs had clear advantages in resolution, sensitivity, and stability, and commercial establishments were strongly interested in supporting their development. (They are the basis of the modern digital camera and are also used for TV cameras.) A problem was that a bare CCD is not sensitive in the UV. Nevertheless, as a result of this presentation, the Working Group decided to open the choice of detector for the camera. When a proposal from Jim Westphal solved the UV sensitivity problem by coating the CCD surface with an organic substance that fluoresced in the visible when hit with UV light, the vidicon lost the competition.

Many in the astronomical community were unhappy with NASA management of the ST. They wanted it in the hands of astronomers with a management contractor in the same way that the national optical and radio observatories were handled. This overlooked the fact that the scope of the ST construction and operation was far larger than that of the ground-based observatories. Nevertheless, there was one area in which the community insistence on operation by scientists was nonnegotiable—the scientific management of the operation. However, Goddard badly wanted the scientific operation of the telescope. After considering this, I decided that it was much too big a job for the small astronomy group at Goddard, even if the astronomical community would have stood still for such an arrangement, and I supported the views of the astronomical community.

As explained further below, this nearly cost me my job and pushed me toward early retirement in 1979. However, I was able to stay on for nine months longer as the ST Program Scientist in order to participate on the Source Selection Board for the Space Telescope Science Institute (STScI), which would manage the scientific operations of the ST. I found this an interesting experience. There were five proposals, four of which sited the Institute at Princeton University. The proposals from Associated Universities Incorporated, which managed the National Radio Astronomy Observatories, and from Associated Universities for Research in Astronomy, which managed the National Optical Astronomy Observatories, were highly competitive, and the decision between them was difficult. The latter was based at the Institute at Johns Hopkins University in Baltimore. Many people believed that it was selected because Baltimore is closer to Goddard. That has helped over time but did not enter our deliberations.

I left the project before substantial management problems arose, leaving their solution to my successor, Ed Weiler. He also had to handle the discovery of the mirror problem. It was clear from his actions in these major fiascos that I had left the project in good hands.

GEODESY AND OTHER PROJECTS

Homer Newell defined astronomy as "the study of where you are not," in order to exclude planetary probes, but he included geodesy, the study of the Earth, in astronomy. The Vanguard payload was a small sphere that did little but signal its position, but it provided important science. By monitoring its orbit carefully, John O'Keefe was able to determine the gravitational figure of the Earth. In particular, he determined that the Southern Pole is nearer the equator than the Northern Pole, i.e., that the Earth is not exactly spherical but slightly pear-shaped. An understanding of the deviation of the Earth's shape from that of a perfect sphere is important as a clue to the internal structure of the planet. The intensity of gravity as a function of position is of interest for the light it sheds on features closer to the surface. NASA's activity in this field created problems with the Department of Defense (DoD). Because of the importance of knowledge of the gravity field for targeting missiles, the military was anxious to keep geodetic data classified. They already had a series of geodetic satellites and were anxious for NASA to use these. This would give them control over the quality of the NASA results as well as limit NASA's program. Finally, the military tried to put a stop to NASA activities in geodesy altogether.

The extensive discussions with the DoD continued for several years. In 1965, NASA was ready to launch the first of its geodetic satellites. As a NASA program, the data would be unclassified and freely available to the scientific community. DoD did not want geodetic data to be openly available. The issue reached the White House. Dr. Donald Hornig, the president's science advisor, was not convinced of the need for classification and determined that there should be an unclassified program and that NASA should be responsible for a program to meet all the national needs. NASA went ahead with the launch of its planned satellite with the DoD planning to use it. In 1966, the DoD insisted that any data they took with the NASA satellite must be classified. This was clearly against NASA policy and would complicate our tracking arrangements with other countries. David Williamson, Associate Administrator for Special Projects, tried to settle the controversy. As I had given up responsibility for geodesy by that time, I am not sure of the details of the final resolution. The engineer with whom I had been working, Jerry Rosenberg, was transferred to another division, and coordination had become too difficult. The general availability of the Global Positioning System (GPS) has probably made the argument moot by now. NASA's Gravity Recovery and Climate Experiment (GRACE) satellites are doing a much more detailed job of geodetic mapping than anything we visualized in the early 1960s.

On the surface, the interface between the military and NASA was clear: NASA had responsibility for the use of space for civilian purposes, although as the discussion of geodesy indicates, as well as the later development of the ST, this was not always a smooth separation. To define the interface between NASA and the NSF, Keith Glennan, the first NASA Administrator, and Alan Waterman, NSF Director, signed an agreement stating that NASA has the responsibility for space astronomy and that NSF has the responsibility for ground-based astronomy. In spite of the agreement, Waterman funded the Association of Universities for Research in Astronomy, the organization that managed the Kitt Peak National Observatory, to plan a large space telescope. In practice, it was impossible to keep the interface clean. The Office of Naval Research (ONR) had a small program of grants for astronomy. My SR&T program obviously overlapped with the domains of both NSF and ONR. My counterparts at NSF and ONR and I communicated frequently, occasionally suggesting the exchange of proposals or even joint funding. We also attended each other's meetings with the astronomical community. I had occasional contact with the Air Force Cambridge Research Laboratory in connection with their rocket surveys of the sky in the IR but otherwise my only contact with the military, other than with ONR, was in connection with geodesy.

Another area in which I became involved was more physics than astronomy: relativity. Einstein had predicted that a gravitational field slows processes such as the time measured by clocks. A good way to test this prediction appeared to be to compare a clock at very high altitude with a similar clock on the ground. Because the expected change in rate was tiny, this would require a very accurate, stable clock. Hence, we started several preliminary investigations into the development of a suitable clock. Finally, in July 1976, Robert Vessot of the Smithsonian Observatory flew a hydrogen maser in a Scout probe to an altitude of 10,000 km. The observed change in time matched predictions to an accuracy of about 70 ppm.

AIRPLANES AND BALLOONS

NASA was overseeing the development of the X-15, an airplane that could reach unusually high altitudes. Arthur Code, from the University of Wisconsin, proposed using it to observe stars in the UV. He designed a spectrometer that could be mounted on the plane with which one of his students obtained good observations of several stars. The success of these observations led to the design of a stabilized platform for the plane. Unfortunately, before the platform could be used, the plane crashed. When it was repaired, it was redesigned for speed rather than altitude and no longer flew high enough for UV astronomy.

Normal commercial jet planes could reach altitudes sufficiently above substantial water vapor to make possible the observation of celestial objects in the IR. Frank Low designed an IR photometer that could be mounted in the window of a Learjet. This started an extensive series of observations from this plane and the use of a CV 990 with an airlock in which instruments could be mounted. When this plane crashed, it was replaced by a similarly modified C141 with a 36-inch telescope mounted in the airlock. This plane, called the Kuiper Airborne Observatory in recognition of Gerard Kuiper's role in supporting IR observations, was launched in May 1975 and started an ambitious journey that would take it on more than 1,400 flights through the Earth's upper atmosphere during more than 30 years. Following the success of the Kuiper Observatory, it has been replaced by SOFIA (Stratospheric Observatory for Infrared Astronomy), a 747 modified to carry a 2.5-m telescope. This has been a joint development between NASA and the German Aerospace Center (Deutsches Zentrum für Luft- und Raumfahrt or DRL).

Another stratospheric endeavor was the Stratoscope project at Princeton University under the direction of Martin Schwarzschild to fly a 12-inch telescope in a balloon to observe the Sun free of atmospheric blurring. The Office of Naval Research was running this project but needed more funds and technical support, which it requested from NASA; the latter was provided by Ames Research Center. After several unsuccessful launch attempts, one flight was very successful. This encouraged Schwarzschild to plan a flight with a 36-inch telescope to observe star fields. This proved beyond ONR's small program, both financially and technically. NSF provided some money, but the technical help came from NASA. Finally, NASA took over the project both financially and technically. It proved more difficult than anyone expected to hold the pointing for long periods. There were good results, but it is likely that they were not worth the effort. There was also one successful IR flight.

INTERNATIONAL ENGAGEMENT

NASA was anxious in its early days to involve other friendly countries in its space program, both to encourage good international relations and as a way of moving those countries into space activities. The first successful astronomical observations were long-wave radio observations from *Alouette*, a Canadian satellite designed to study the ionosphere. In the process, the Canadians made the first observations of radio emission at wavelengths too long to penetrate the ionosphere. In 1960, the British contacted me about a rocket program in X-rays and, with lesser priority, the UV. This began an extensive cooperation that included the launch of Ariel 5 (UK-5) to study cosmic X-rays and, later, the IUE and other satellites. The British may have had a successful stabilized rocket for astronomy before the United States. They were also eager to fly a satellite similar to the OAO. The cooperation also went the other way with the use of launch facilities in Australia by Al Boggess at Goddard to observe objects in the UV that were not accessible from the Northern Hemisphere. We also discussed an astronomy rocket program with the Italians in 1960, but I do not remember that much came of the discussion. In July 1960, there was a major conference on UV astronomy held in Liege, Belgium, at the Institut d'Astrophysique. Extensive participation from many countries showed wide interest in the field.

SALESMANSHIP

I had never thought of myself as a salesman, but I began to realize that much of my activity in NASA was salesmanship. This included selling the space program to astronomers as well as to decision makers in government. In 1959, the astronomical community was divided into the haves and the have-nots. The astronomers, primarily in the west, who had ready accesses to large telescopes in good climates, saw the space program as competition. As a result, they were strongly opposed. Those without easy access to good telescopes and good weather, primarily easterners and midwesterners, were more eager to become involved in the space astronomy program. I felt that it was important to convince the "haves" that they could also benefit from a healthy space astronomy program. Several who were originally opposed to it later became strong supporters.

When I started at NASA, the public was excited about the new agency. As a woman in a position where a woman was unexpected, I was of particular interest to the public. As a result, there were frequent stories about me. At first, someone from the Public Affairs Office accompanied reporters but they soon gave up this practice. One syndicated column was the source of much laughter among my friends. Noticing that I had been born in Nashville, the writer started by commenting on my Southern accent. It was obvious that she had not spoken with me! Having left Nashville well before I learned to talk and having left anywhere that could be called "the South" at the age of three, I had no trace of a Southern accent. My accent was sufficiently polyglot at that time that it puzzled people interested in accents but was probably still primarily midwestern.

The publicity led to numerous invitations to speak to a wide variety of lay groups. I often accepted, as I felt that it was important to sell the program to the taxpayers. I usually described the current knowledge in one or more fields of astronomy with many pictures and, as I did so, discussed how observing through the atmosphere limited our ability to extend our understanding of astronomical systems and suggested how space observations could contribute to the solution of major problems in astronomy. These talks were normally well received.

MEDIA INTERVIEWS

I have given so many talks and interviews that only a few are memorable for various reasons. One was a discussion of the OAO on the *Today Show*. Unfortunately, it was broadcast while I was on an airplane returning from the URSI (Union Radio-Scientifique Internatinale, i.e., the International Scientific Radio Union) meeting in Toronto, so I did not hear it.

In 1964, I participated on a panel at a luncheon sponsored by the New York branch of the AAUW and held in the Waldorf Astoria ballroom. The room was packed with tables on the ground floor, and additional observers crammed the balcony. It was certainly the largest audience I ever spoke to in person. I estimate that there were more than 1,000 attendees. Equally memorable was my luncheon partner, John Hope Franklin. Franklin was an American historian of the Civil War and slavery and was awarded the Presidential Medal of Freedom. His book, *From Slavery to Freedom*, has sold more than three million copies. I do not remember what he spoke about on the panel, but I was certainly impressed by his informal conversation.

I was asked to speak about detectors to the British Society of Television Engineers. I tried to decline, saying that I was not the appropriate person to speak on that subject, but they were adamant; they wanted me. I do not remember the talk, but the trip had other aspects that I remember well. Wives of the engineers were included on a tour of facilities in Manchester. As one of the few, if not the only, professional woman in the group, I spoke a great deal with the wives. One asked me where I was from. I replied, "Washington, DC." "No, I mean, what country?" "The United States." "You can't be. I can understand you." A more embarrassing incident occurred at a formal luncheon in the Guild Hall when I was seated at the head table. I used to have a problem with hiccups when I drank or ate very hot liquid. The hot soup stimulated an impressive bout.

MISTAKES

The start of the Apollo program in 1961 had a major impact on NASA. The science program was small in comparison to the manned program, which gave us leeway to take risks. This made substantial progress in space astronomy possible but also allowed me to make mistakes.

I funded two SR&T projects longer than I should have. One was a proposal to coat a spinning liquid with a material that would harden and form a mirror. Theoretically, this was possible. The surface of a spinning liquid takes a parabolic shape, the shape needed for a telescope mirror. Some Canadian astronomers made use of this to produce an inexpensive but useful instrument, and some large mirrors are now shaped roughly by rotating the molten glass. However, it became clear that the proposer was unable to produce the necessary solid surface. When I finally rejected a continuation of the project, he was funded by the Air Force.

A second mistake was funding Willem Luyten too long before recognizing that he was not capable of monitoring his contract with Control Data. In addition to the change in position of stars on the celestial sphere because of the precession of the Earth's rotation axis, stars have their own intrinsic motions both along and perpendicular to the line of sight. The transverse motions are measured by comparing the positions of the stars at different times. Until recently, this was done by photographing a star field a second time after a significant interval and optically superposing the plates to detect the motions. Luyten at the University of Minnesota wanted to develop a machine to do this comparison automatically. NASA needed instantaneous positions for the guide stars it would use both for the OAOs and for the ST. Unfortunately, Luyten did not have enough understanding of computers to supervise the expert at Control Data Corporation who was building the machine. I am unclear whether this expert was just a perfectionist or if he was stringing the contract along. An outside review of the project made several suggestions for improvement, but there was little progress. Finally, NASA agreed to transfer the project to the NSF with a different, younger investigator. She was able to guide it to a useful working machine that was used more broadly than just for proper motions. Understandably, Luyten was upset by this change and evoked the Freedom of Information Act to obtain all NASA documentation about the project. I felt sorry about this. There was nothing shady about the process. I liked Luyten and was sorry to have him waste his money that way. Barry Lasker at the STScI ultimately developed a more modern measuring machine to obtain positions for the *Hubble* guide stars.

ASSISTANCE

I needed additional personnel at NASA to help run the program, but astronomers were hard to find at the outset of the space age. Most wanted to stay in universities rather than join the government. Finally, I hired an engineer, Ernest Ott, to help on the more technical portions. I also hired two female astronomers, Jocelyn Gill and Nancy Boggess. Jocelyn, who had slowly developing multiple sclerosis, could no longer stand for lecturing at Gilford College and was happy to have a less physically demanding position. After a year or so with me, she moved to the manned flight program to work with the astronauts. Nancy, the wife of Albert Boggess, who was the head of the optical astronomy laboratory at Goddard, had young children and was happy to work part time so that she could be at home when they returned from school. As the children became older, she gradually increased her working hours, eventually becoming full time. She stayed with me until I retired, after which she transferred to Goddard. There she played a major role in the development of the *Cosmic Background Explorer*. Gordon Augason also spent a year with me on detail from Ames.

WOMEN'S MOVEMENT

By 1964, the women's movement was gaining strength. Mary Hilton of the Women's Bureau in the Department of Labor invited me to a class on management for women at Pennsylvania State College. The idea appealed to me as I felt that, at least at that time, women faced problems not faced by men. However, I found the course disappointing. The presenters gave standard talks that did not recognize that they were talking to women. I also felt that most of us had been in management long enough to already learn the hard way the things that were being presented. I did enjoy the company of other interesting women. An IBM executive said that IBM does not hire women managers because they might have to work nights. I was tempted to ask him when he thought that astronomers work. (I realize that working in a remote observatory is different from working in a city, but I also worked nights in Washington.) I was urged to attend another course for women in management several years later and to critique the course. It was as disappointing as the first one.

One of the things I am surprised by and somewhat ashamed of as I review my history is that I did not complain about the NASA policy of restricting the astronaut core to men. I am quoted as saying in a talk at Marymount College in 1963, "There is a great deal of discussion of whether the United States should have women astronauts, and I am frequently asked for my own opinion on this subject. Frankly, it makes little difference to me. I believe that there will be women astronauts some time, just as there are women airplane pilots, but there are so many other ways that a woman can contribute importantly to the space program that the fact that there are no women astronauts yet should not worry us." In 1966, when NASA announced that it would open the astronaut core to scientists who were not pilots, only four women applied out of a thousand applicants. I have also always been sorry that I did not endorse a stamp for Maria Mitchell. I did not think her comet discovery was that great, but I overlooked the major impact she had on science education for women as well as her importance as a role model. I do not know what I was thinking that day.

RETIREMENT

About 1976, I began to become tired of my job at NASA Headquarters and started to look casually for other positions. I did not feel that I could leave Civil Service without retiring, as I had no other pension, but astronomy positions near my level were rare. Goddard might have had a possible position, but, as I explained above, the Goddard astronomers were mad at me because I would not support putting the STScI at Goddard. Aside from the fact that the astronomical community would have been up in arms and gone to the top levels of NASA if I had tried, I believed that the Institute would have overwhelmed the small group of astronomers at Goddard.

Somewhat later, Noel Hinners, the Associate Administrator of Science at NASA, said that I had been in the job for too long and suggested my changing to another position that I did not think I wanted or would have been particularly good at. I declined and stayed where I was. Later I understood what had happened. The director of Science at Goddard (George Piper) and the head of the astronomy group (Jack Brandt) had gone to Hinners to try to get me out because of their unhappiness with my decision on the Institute. When I met Hinners after both he and I had left NASA, he asked me why I had left. I did not remind him that he had told me that I had been in the job too long.

Civil Service had not yet started the practice of giving bonuses to people who retired when they were trying to decrease staff sizes, but they did give people an opportunity to retire early if there was a major reorganization in an administration or department. Such a reorganization took place in NASA in 1979. I began to consider retirement because I was angry with the games that Congress was playing with our salaries. Civil Service salaries are capped below the level of Congresspersons, with salaries decreased in steps below the top level. They kept their own salaries low, making up the difference with perks, in turn limiting our salaries. I looked at the rate of increase of my father's pension and realized that it was substantially greater than the rate of increase of my salary. This turned out not to be a good reason to retire, as Congress raised the salary cap by more than 30% in 1980.

I debated about retiring early for most of the year. Mother was complaining that I was getting too tired. The last week in which I could have retired I realized, from the way I reacted to two personnel problems, neither of which was new, that Mother was correct. Trying to take care of her and handle a demanding job at the same time was more than I could manage. Hence, the day before the early-out period ended, I went to my boss, Jesse Mitchell, and told him I had to either change jobs or retire. He replied that he was flying to California that afternoon but to call him in the morning with my decision. When I did, I learned that he had arranged a job for me at Goddard. I no longer remember what it was, but I decided not to take it. This was certainly the correct decision. Normally, I would have had to work another 6 years to receive my pension and I could not have handled both Mother's decreasing health and a full-time position by that time. Yet, I really wanted the experience of serving on a Source Selection Board. Jesse arranged for me to continue as a Consultant for a year, so I could serve on the Board for the selection of the STScI. I felt I was leaving the program in good hands with Ed Weiler and resigned the consultancy as soon as the manager for the STScI was decided.

I retired at the end of September 1979 and was happy for a rest and time to get ready for Christmas. Nevertheless, I felt that at 54 I was too young to quit altogether. I realized that if I was to go back to astronomical research, I needed to become comfortable with two technologies that had changed while I had been in management: modern computer programming and electronic imagers. I had learned computer programing in 1956 when it was done in hexadecimal machine language, but it had changed greatly in 23 years. In early 1980, I asked the professor at Montgomery College for permission to audit a course in Fortran, the higher-level language that was used commonly in science at that time. Despite not having had the prerequisites, not only did I have no trouble with the course but also the younger students did not understand how I could do so much better than they did.

By the end of the course, I realized that I could no longer do forefront astronomical research and decided to look for a half-time job that I thought I could handle. I was offered a position as an adjunct professor at the University of Maryland. Aside from the fact that I was not enthusiastic about teaching huge classes of elementary astronomy students, most of whom had no interest in science, I could not accept this position because the University of Maryland was one of the proposers for the STScI. At a meeting on the ST, I happened to meet the director of Old Republic International Corporation (ORI), a company with a major contract supporting Goddard, primarily but far from exclusively on the ST. I decided to approach him for a job and was welcomed as a Consultant. I found my work with ORI interesting and varied. I chaired a committee of astronomers trying to judge the cost of supporting observers with the ST other than the major participants and wrote several brochures on astronomy and other subjects. The most interesting part of the job was two studies of the use of space observations in geodesy. Scientists at the Jet Propulsion Laboratory wanted to set up a network of large radio telescopes to measure plate tectonic motions, and I was asked to look into the desirability. This required becoming familiar with not only various possible techniques for measuring such motions but also plate tectonics and the major plates. I decided that the GPS system that was then under development by the military was the best way to approach the problem, as it has proven to be.

I did not get as much work at ORI as I wanted, so I went to Goddard and said I know astronomical catalogs. If you teach me computers, I'd like to work for you in the Astronomical Data Center. I got the job and continued over the years to increase my time until I was working half-time as the director of the Center. After ORI lost the contract, I transferred to McDonald Douglas, where I worked primarily on Earth observations. Instruments for looking down are similar to those for looking up.

In 1997, the Goddard contract monitor said she wanted me out. Although this was strictly illegal, the contract was up for renewal, and I did not want to fight it. I then looked for voluntary work. For three years, I worked with a 5th grade class for seven weeks, primarily doing several projects on time. Then I joined a program that sent scientists and engineers to underserved regions of the country to work with K–12 classes for a week with usually four classes per day. I very much enjoyed this program, but it finally lost its funding. I also co-taught astronomy courses at Montgomery College for both teachers and high school students for several years. In addition, I started to read, primarily astronomy books, for the Blind and Dyslexic, which I continued to do for at least 10 years into my mid-eighties.

Figure 1

The 1962 winners of the Federal Women's Award meet with President Kennedy. Nancy Grace Roman is third from left. Photo: Abbie Rowe. White House Photographs. John F. Kennedy Presidential Library and Museum, Boston. **https://www.jfklibrary.org/asset-viewer/archives/JFKWHP/1962/Month%2002/Day%2027/JFKWHP-1962-02-27-A**.

HONORS

In the early 1960s, the Civil Service Commission realized that major honors for civil servants were restricted to men and that some way was needed to honor women. They persuaded a local department store to sponsor and organize the Federal Women's Award. Each year, starting in 1961, six women were selected for the prize. Those in the first year were outstanding. One went on to win a Nobel Prize for her research in a veteran's hospital. I was selected for the honor in 1962 (**Figure 1**). A plaque and a gold medal were presented at a formal banquet at the Washington Hilton hotel. Part of the activities included a meeting with John Kennedy in the Oval Office. The winners were an interesting group. Those in the Washington area continued to meet for lunch twice a year until 2015, although I am the only one left from the early years.

The rehearsal for this dinner occurred at the same time as John Glenn was giving a press conference in the same hotel about his historic space flight. Both affairs concluded at the same time. I was anxious to get back to NASA for his debriefing but could not get out of the hotel. If I could have gotten past security, I could have ridden back to NASA Headquarters with him. Because of the delay, I missed the beginning of the debriefing but got back in time to have detailed notes on the part that interested me most.

Glenn did not think he saw more stars than in a dark desert sky. He felt that the transmission of the window was comparable with that of the atmosphere. Stars could be seen in the daytime after

Figure 2

Lego set featuring Nancy Grace Roman and the *Hubble Space Telescope*. Photo: © LEGO; reproduced with permission.

about one minute of dark adaptation. He could see quite faint stars near the Moon. The Sun was not as blinding as he expected. It was a very clear bright light much like an electric arc. He was never dark-adapted enough to see the zodiacal light. The window polarized the light somewhat. Could that explain the colors? He saw many particles that looked like fireflies—as bright and the same color. They could only be seen in sunlight.

The Federal Women's Award is to me the most important of my honors, perhaps because it was the first, but I have been fortunate to receive many others. Highlights include four honorary Doctor of Science degrees including one in 1976 from Swarthmore. I was selected as one of 100 Most Important Young People by *Life* magazine in 1962. NASA honored me twice, with the Exceptional Scientific Achievement Award in 1969 and an Outstanding Scientific Leadership Award in 1978. I received the William Randolf Lovelace II Award from the American Astronautical Society, and Asteroid 2516 is named after me. I have also received recognitions from several women's organizations, including the Women's Education and Industrial Union, the *Ladies' Home Journal* Magazine, Women in Aerospace, the Women's History Museum, and the AAUW. But by far the most fun has been the "Women of NASA LEGO Set," which went on sale in 2017 (**Figure 2**). It has received the most publicity, and I have personally signed hundreds of boxes.

DISCLOSURE STATEMENT

The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

LITERATURE CITED

Launius RD, Devorkin DH, eds. 2014. *Hubble's Legacy: Reflections by Those Who Dreamed It, Built It, and Observed the Universe with It*. Washington, DC: Smithson. Inst. Sch. Press

Roman NG. 1950. *Ap. J.* 112:554

Roman NG. 1955. *Ap. J. Suppl.* 2:195

Zimmerman R. 2008. *The Universe in a Mirror: The Saga of the Hubble Space Telescope and the Visionaries Who Built It*. Princeton, NJ: Princeton Univ. Press