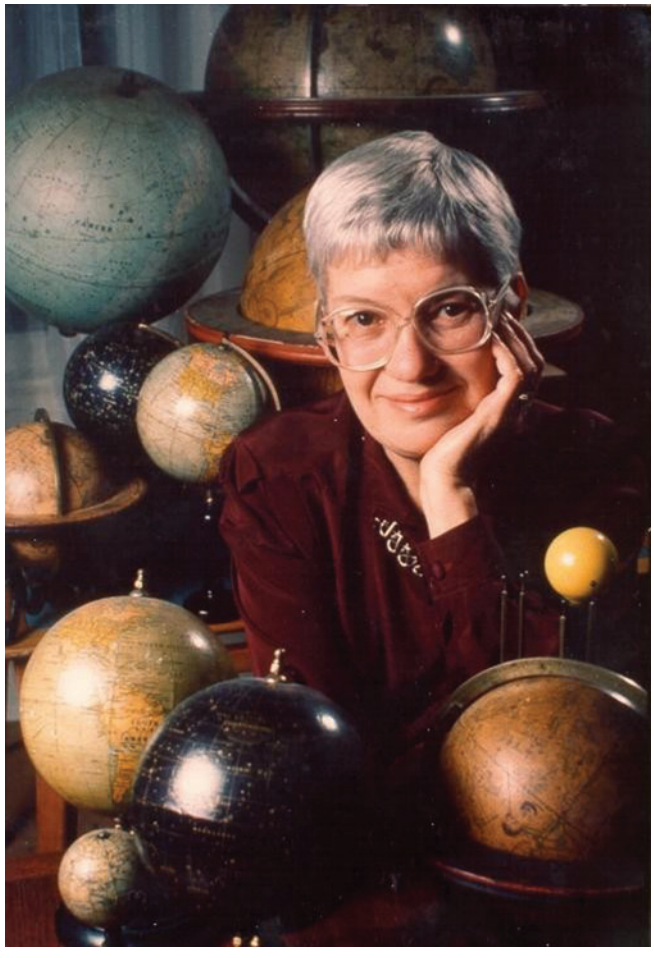

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An Interesting Voyage

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Abstract

My life has been an interesting voyage. I became an astronomer because I could not imagine living on Earth and not trying to understand how the Universe works. My scientific career has revolved around observing the motions of stars within galaxies and the motions of galaxies within the Universe. In 1965, if you were very lucky and interested in using telescopes, you could walk into a research laboratory that was building instruments that reduced exposure times by a factor of 10 and end up making remarkable discoveries. Women generally required more luck and perseverance than men did. It helped to have supportive parents and a supportive husband.

PHILADELPHIA EARLY DAYS, 1928–1938

My father, Pesach Kobchefski, was born in Vilna, Latvia, the second of four children. His father, a glove maker, immigrated to the United States in the early 1900s and joined family friends in Gloversville, New York. A year or two later, after his father had earned enough money for the family to travel comfortably, his mother and the four children followed. My father, then about seven years old and soon known to all as Pete, remembered the trip from New York City to Gloversville as a succession of bumpy separate trolley car rides. Although he initially knew no English, he recalled his youth and schooling as pleasant, especially the toboggan sledding in winter.

One lasting memory was his high school prom, when his girlfriend arrived with her father driving a car. Pete, a budding engineer who had never ridden in a car, jumped onto the front seat and left his date alone in the back seat. Following his high school graduation, Pete Cooper entered the University of Pennsylvania in Philadelphia as an engineering student. After graduation in 1920 he took a job with Bell Telephone Company.

Rose Applebaum, my mother, was born in Philadelphia. Her mother had lived on an apple orchard in Bessarabia, and at age 16 left her family and sailed steerage class alone to New York. Because her Jewish religion prevented her from eating many foods, she suffered from near starvation. A sensitive officer, who continually brought her fruit, saved her. Once in the United States, she traveled to Philadelphia to be near friends and relatives. She married a tailor who sewed men's suits for Wanamaker's Department Store. Rose was the second of their four children. She attended the South Philadelphia High School for Girls where the students were assigned seats. As Rose Applebaum, she sat alphabetically next to Marian Anderson in the auditorium. Later they recognized each other when they met in the office of Giuseppe Boghetti, the voice teacher of each. They would reconnect later in life.

After graduating from high school, Rose accepted a job with the Bell Telephone Company, where she calculated the routes and the costs of telephone lines. Rose and Pete were introduced by friends, and also met at their jobs. They had a very traditional courtship, except that they had to keep it secret from their Bell Telephone employers, who had a "no-dating" policy. Pete would arrive by trolley car to take Rose to dinner or the movies with friends. Later, his daughters loved to hear the story of how Pete arrived one day with a bouquet of flowers for Rose, dandelions he had picked in the park. Rose and Pete had a very happy marriage. Typical of the era, Rose stopped working following her marriage. Although times were not easy during the depression, their two children, Ruth and Vera, grew up amid a cheery scatter of grandparents, aunts, uncles, and cousins. The children were largely shielded from the financial difficulties.

As a very young child, I was continually puzzled by the curious workings of the world. As we drove home from Grandmother (Bubba) Cooper's house, the bushes, trees and even distant hills passed behind us, but the moon sat steadily outside my window. How could the moon know that we were going home? I must have been given an answer; probably I did not understand it. But I still remember the thrill of the question. And now, about 80 years later, the thrill I get from asking questions of nature is no less.

As a youngster, more questions followed. Why did the pictures on my bedroom wall jump back and forth on each side of my finger as I lay in bed blinking my eyes? How did water drops in a stream know on which side of a rock to pass? Could I, a lazy child, devise a street on which one sidewalk went uphill and one side downhill, so that I could always walk downhill? A little later, the questions were more conventional. How many license plates can be made with three numbers and two letters? This puzzle I solved as we drove to our new home in Washington, D.C.

GROWING UP TO ASTRONOMY, 1939–1948

In 1939, my father accepted a job with the U.S. Department of Agriculture in Washington, D.C. School here was fun after the strict Philadelphia school, which had nailed down seats and required folded hands. In our small house, I shared a bedroom with my sister. From my bed against a window, I had a clear view to the north sky. Soon it was more interesting to watch the stars than to sleep. I started reading library books, thinking that if I read enough books I would "understand it all." My mother wrote a note for me, permitting me to withdraw books from the adult (12 and over) section. I still remember the librarian looking at me sternly and saying, "I wouldn't let my little girl take out books from the adult section." My father helped me build a telescope. The small lens came from Edmund Scientific Co. for a few dollars, and the tube came free from a linoleum company. I carried home this large awkward tube on a bus from downtown D.C. The telescope was only a moderate success, but I was able to get respectable photographs of the diurnal motions of the stars. Some years later, I gave the telescope to an interested young boy.

High school too was fun. Classes were easy, and I enjoyed outside activities such as editing the school yearbook. Only physics was a problem. The high school physics teacher did not know how to include the few young girls in the class, so he chose to ignore us. The first day of class, he defined two kinds of discoveries: those that took insight and brilliance (here his examples all came from males) and discoveries that required hard work but not brilliance (his example was the discoveries of Marie Curie). I was often angry in class for his similar comments. I rarely spoke, and the teacher never learned that I was interested in astronomy. The day I received mail informing me that I had been accepted to Vassar College with a needed scholarship, I met the physics professor in the hall and told him my good news. "You should do OK as long as you stay away from science," were his words. Following graduation, I never again saw him.

I entered Vassar College in 1945. Vassar matched my interests well: excellent teachers, and students learning together. In addition to a magnificent library, a modern five-inch telescope was mine to use as I wished. Vassar's initial telescope, a 15-inch, was the third largest in the United States when Vassar opened in 1865. In 1948, it was reserved for students' studies. Three months after arriving, I had a job as assistant to the astronomy professor, Dr. Maud Makemson. I was clock winder, paper grader, and telescope helper. We learned from teachers, from visitors, books, telescopes, problem solving, and each other. After the first year, an astronomy class would generally have two to eight students. For the second year, each student was required to choose a newly discovered asteroid and to compute its orbit. This was a real learning experience, as we each learned to help the others. Highlights of my second year included the visits to Vassar of two young men, Leonard Bernstein and Richard Feynman. Feynman stayed for several days and listened as we discussed our science projects. I could not know that in two years I would be studying with Feynman. I graduated from Vassar in three years. My required senior thesis was based on an active star, Gamma Cassiopeia. My only recollection now of my paper is that Margaret Burbidge had produced many of the studies quoted for that star.

The summers of 1946, 1947, and 1948 I spent in Washington D.C. working at the Naval Research Laboratory (NRL), where exciting science was happening. Following the 1946 first successful rocket return, information about the UV spectrum of the Sun could now be obtained. This made the summer of 1947 especially exciting for me. My job was to study the optical properties of the transparent spherical bead that had served as the instrument lense. It was also the summer that my parents (and his) introduced me to Robert Rubin, a near neighbor. Bob, after one term as a college freshman at Johns Hopkins University, had enlisted in the U.S. Navy. He was sent to Cornell University to study chemistry and trained to be a Naval Officer. He became an officer as the war ended but remained at Cornell to complete his doctoral degree. My first words to Bob

were, "Do you know Richard Feynman?" Bob's answer was that he was studying with him. Three months later, after a busy summer followed by numerous trips between Cornell and Vassar, we wrote our parents that we wanted to be married after my graduation in the spring. Bob and I were married before sundown on June 25, 1948. He was 21 and working on his thesis. I, at 19, had my B.A. degree and planned to join Bob at Cornell. On our wedding day, Bob took the day off from his summer science job and called me from the the public library to tell me he was spending the morning reading. I had earlier called my office at the NRL to ask if I could start my summer job one week later. They said no, but when I explained I was getting married, they relented.

After the summer in Washington, we left for Cornell in the fall. Our delayed honeymoon was at Block Island, Rhode Island, a sight Bob had viewed during his Navy days.

GRADUATE SCHOOL AND TEACHING, 1948–1954

The Cornell physics department was spectacular. Although I had classes with Richard Feynman, Phil Morrison, and Hans Bethe, I was determined to be an astronomer. Martha Stahr, a young Ph.D. graduate from Berkeley, was a fine teacher. From her I learned classical mechanics and contemporary astronomy. The chairman of the department, the only other astronomer, had been a Navy navigator. I assisted in his classes, graded papers, ran the weekly labs, and conducted the weekly astronomy laboratory in the observatory, but I rarely discussed astronomy with him. I took his course in astrochemistry and found it surprisingly interesting. For my Masters thesis, I examined the velocity distribution of the galaxies with published velocities. At that time, the number of such galaxies was only 108. Dr. Stahr was my advisor. After I gave the Chairman a copy of my completed thesis, he called me into his office and said (as closely as I can now quote), "This is very interesting and you should give a talk at the December 1950 Haverford AAS meeting. But you will have your new baby, and you are not an AAS member, so I could give the talk but it would have to be in my name." Immediately I replied, "No. I can go." We had no car. My parents drove from Washington D.C. to Ithaca, then crossed the snowy New York hills with Bob, me, and their first grandchild, "thereby aging 20 years," my father later insisted.

The next morning, Pete drove Bob and me to the AAS Haverford meeting. I gave my memorized 10-minute talk, acceptably I thought. Then one by one many angry sounding men got up to tell me why I could not do "that." All except a little man with a squeaky voice (later identified as Martin Schwarzschild), who said what you say to a young student: "This is very interesting, and when there are more data, we will know more." Following this confusion, a coffee break was called. We left shortly afterward, for I was anxious to see how the baby was doing away from his mother for the first time. I was told that my first words when I entered the house were, "Was he happy?" The next day, the *Washington Post* front page noted, incorrectly, "Young Mother Finds Center of Creation" or something like that. Fifty years later, friends made a fake front page: "Old Grandmother Gets Medal of Science." They were certain that I would remember the original. I did.

In the summer of 1951, Bob, I, and baby David moved to suburban Washington, D.C. Bob had accepted a job at the Johns Hopkins Applied Physics Laboratory (APL), then located at the edge of northwest Washington. His office mate was Ralph Alpher. A few years earlier, Ralph had written his thesis under the direction of George Gamow at the George Washington University. The subject was the early Universe, a topic rarely treated scientifically. Ralph continued his cosmology studies with Bob Herman, a physicist located just down the hall.

After a few months of being out of school, I wanted to learn more. I would push David to the playground, sit him in the sandbox, and read the *Astrophysical Journal*. It was Bob's gentle persistence that caused me to enroll in Georgetown University, the only university in the area that offered a Ph.D. degree in astronomy. At Georgetown University, all graduate courses were

at night. This is how I (pregnant, not driving) got to Georgetown, twice a week from 1952 to 1954. After work, Bob picked up my mother and drove her to our house, she carrying dinner for my father and herself. At our house, she got out of the car and I got in, having fed the child (later children) and carrying dinner for Bob; he ate dinner in the car after I had gone into the Observatory. Outstanding scientists located in the area taught the classes. I remember a remarkable course in spectroscopy taught by Carl C. Kiess, an eminent spectroscopist working at the National Bureau of Standards. He attempted to interest me in identifying the unidentified lines in the solar spectrum for my Ph.D. thesis, but I declined, politely I hope.

GEORGE GAMOW

George Gamow often showed up at APL to talk cosmology to Alpher and Herman. Gamow had heard of my Haverford talk while visiting Schwarzschild at Princeton, so he started calling me at home and asking tough questions. He was especially interested in my Masters work. One question he asked, "Is there a scale length in the distribution of galaxies?" was especially interesting, so I decided to study it for my Ph.D. thesis. Father Francis Heyden, the chair of the Georgetown astronomy department, agreed. It was amusing working with Gamow. He was not interested in details; he cared little about what I was doing or how I was handling the mathematics or the Harvard galaxy counts. I learned much from our discussions, though little of it related to my thesis.

During the summer of 1953, the now famous Michigan Summer School took place, and Bob and I went for two of the four weeks. Grandparents babysat. It was a remarkable experience. Walter Baade, Gamow, and others including a young Allan Sandage lectured, and taught, and argued. It was a magnificent learning experience for me. I remember one afternoon that Baade, Gamow, and a relatively young Vera sat on the edge of someone's bed (not mine), for nearly five hours, asking and answering very tough questions. What came from these discussions is best described from words in the book *Evolution of Stars and Galaxies* (Baade 1963). Baade intended to write a book following his 1958 lectures at Harvard, but he ultimately had only a first draft. In 1966, Cecilia Payne-Gaposchkin edited and published his first draft, under Baade's name. At that time, there was a hint that the difference between Population I and Population II stars was due to their ages. Following the summer school, Baade wrote, "I believe that George Gamow was the first to suggest the interpretation that is accepted today. Shortly after the publication of my paper on the two stellar populations, I received a typical message from Gamow on a postcard: 'Please tell me where the lower branch of the color-magnitude diagram joins the main sequence, and I will tell you the age of your Population II stars.'" Baade answered Gamow by telling him that nothing was known, but that he (Baade) intended to determine it as soon as possible and that, in the meantime, Gamow could extrapolate as he pleased. Then Baade wrote, "I promptly received a second postcard from Gamow: 'With due respect to Schonberg and Chandrasekhar I have extrapolated the lower branch thus [there was a picture in which Gamow had marked the point at which the subgiants turn upward]. O.K., four to five billion years.'" Baade continued, "This was a guess, of course, and that Gamow hit it so well was an accident, but his remark really contained the whole story." Gamow had several such scientific accidents.

Back in Washington, fall classes were starting and I would take several while also completing my thesis in 1954. When I mailed an early draft of my thesis to Berkeley, where Gamow was spending the term, he wrote back (with his own unique spelling), "It looks quite nice. I could not force myself to plunge into the details of eider integrals, tailing, or plate-arranging, but I presume it is all right." Chandra, who was then the *Astrophysical Journal* Editor, rejected my paper. He wrote that a student of his was working on the same subject and therefore I should wait until his student's work was done. Gamow sent me a card with four enormous words: "I TOLD YOU SO."

The thesis was published in the *Proceedings of the National Academy of Sciences*. About ten years later, at a party in Texas in connection with a meeting on Relativistic Astrophysics, the current editor of the *Astrophysical Journal* asked me why I had not published my earlier paper in the *Astrophysical Journal*. When I told him that the *Astrophysical Journal* had declined it, he asked me to send him the letter. I refused, telling him to look it up in his files.

Later in the spring, Bob escorted the almost 2-year-old Judy and the almost 4-year-old Davy to my Ph.D. commencement. In 1955, Georgetown University offered me a position that I held for the next ten years. Initially, I did only research, but soon I was also teaching. During that time, Father Heyden chaired the astronomy department, Father Martin McCarthy was a visiting teacher, and George Coyne was a student. In 1997, forty-three years after receiving my Ph.D. degree, I gave the Georgetown University commencement address, and the University honored me with the degree of Doctor of Humane Letters.

LES HOUCHES, 1959

One spring day in 1959 Bob found a telegram on our front door, informing him that he had been selected to participate in the Les Houches science summer school, held in the French Alps. Without hesitation he agreed, while also notifying the organizers that he would arrive with his wife and children, aged 8, 6, and 3 (Davy and Judy had been joined by Karl). We sailed from New Jersey, with parents waving us bon voyage. Five in a small room was a little tight: two lower bunks, two upper bunks, and one crib down the center. This made getting out of a bunk a little complicated. But it was a fun trip, with activities for the children and few problems. The one evening we left the children so we could sample evening life onboard ship, we had a remarkable surprise. In our "not first class" fancy entertainment area was a recognizable astronomer, Donald Menzel, Director of the Harvard Observatory. He was entertaining with his guitar and his manner. He had come down from first class, because "this was more fun." This was quite a change from Donald at Harvard.

We landed at Le Havre, spent the night in Paris, and drove to Les Houches. We moved into a flat above the shoemaker's store, but ultimately lived high up the mountain in a newly constructed mountain chalet. It was mountain living, and unforgettable. We shared this house with a French student and his wife and two sons, who were the ages of our children. We muddled through with high school French, but the children needed no language to play together. We learned much from each other. Neither parents approved of the other children's diet. For our hikes, she carried diluted wine; I carried bottled sweet drinks. When the children needed more activity, they walked in the woods to pick some of the endless blueberries or walked higher up the mountain to bring back the fresh milk from the farmer's cows.

Bob's days were taken up with classes. Occasionally we would walk down the mountain and eat dinner with the students, thus getting to know most of them. One of the students was an active British astronomer, who dashed around in a handsome red roadster with the top down. His name was Donald Lynden-Bell, and later he and his wife would be our good friends. During the final month of classes, Evry Schatzman, a French astronomer, was teaching a class on stellar evolution. Although it was against the rules, he invited me to attend his classes. His wife would climb up to our house several afternoons a week to watch our children while I enjoyed the class. Weekends were free, and we drove to all the interesting nearby cities. We became experts in hiking up mountains and riding all of the many telepheriques in sight. We spent one lovely weekend in Geneva, with adjoining rooms overlooking the lake.

At the finish of the summer school, we left with some regret. We had all enjoyed a remarkable experience. We drove north with many side excursions, for Bob was to spend some time with

physicists in Holland. It was a lovely trip, especially Bruges with its captivating canals. I was to fly to New York with the children. The plane to New York left about three hours late, midnight instead of 9 P.M. It was a long trip. When finally we reached New York, with numerous suitcases, winter coats, and a large duffel bag, the customs officer chose to open the duffel bag. There on top, among much equipment, was a large saw. "Lady" he said, "How are you going to get out of here?" I pointed up to the visitors' row, and said "They are my parents." With that he passed us through. He never opened the sleeves of the children's winter coats. All were stuffed full with rocks. That may partially explain why two of our sons (David and Allan, the youngest) are geologists. Karl is a mathematician, and Judy is an astronomer. All are academics. Most of the rocks are now gone from our attic, but a few still decorate the front porch.

MAGIC YEARS, 1960–1970

1960 ushered in a remarkable period in our lives. Our fourth child was born in late May, two weeks after my teaching was over until the fall, and two months before Bob and I left for the Nuffic International Summer Course in Science (astronomy) at Nijenrode Castle in Breukelen, The Netherlands. We split the family: David and Karl went to the Florida grandparents, and Judy and baby Allan were cared for by my visiting parents. At Nijenrode, for three weeks, 69 students and young astronomers heard lectures from some of the world's greatest astronomers. The subject was "Present Problems Concerning the Structure and Evolution of the Galactic System." Students and faculty became colleagues; Jan Oort, Adriaan Blaauw, Hendrik Christoffel van de Hulst, Lodewijk Woltjer, and Margaret and Geoffrey Burbidge were prominent speakers. Their talks and discussions played a large role in bringing me up to date with current important events in extragalactic astronomy. Initially, Oort terrified me, but I soon had too many questions to stay silent.

We spent the next summer in Boulder, Colorado. Bob arranged to work at a branch of the Bureau of Standards there. His parents were vacationing with us. This made it possible for Bob and me to attend back-to-back international meetings in Southern California. The next morning we sat facing a Russian astronomer, he in the process of ripping his teabag apart, in order to shake the dried pieces into his hot water. As he spit out the first taste, we all knew that he had made an error. From then on, it was all science and another friendship.

In 1962 in Georgetown, I was teaching a graduate course on statistical astronomy. My class had six students; five of them had Masters Degrees and worked for the U.S. Naval Observatory, and the sixth worked for NASA. Due to their jobs, the students were experts in star catalogs, so I gave the students (plus me as a student) a research problem: Can we use cataloged stars to determine a rotation curve for stars distant from the center of our Galaxy? By the spring of 1962, we had completed a paper (Rubin et al. 1962), some of it finished by seven of us working around my large kitchen table, long into the night. We submitted our paper to the *Astronomical Journal*. The editor called me to say he accepted it, but he would not publish the names of students. When I said, "Then I withdraw the paper," he relented. The abstract stated: "For $R > 8.5$ kpc the stellar curve is flat, and does not decrease as is expected for Keplerian orbits" (Rubin et al. 1962, p. 491). Following its publication, the many comments I received were negative and some very unpleasant: it couldn't be correct, or the data were not good enough. This was my first flat rotation curve; however, the paper continues to be referenced every few years.

LA JOLLA, 1963–1964

At the AAS 1962 meeting in Phoenix, I asked Margaret and Geoff Burbidge if I could work with them during the 1963–1964 school year. Bob had been offered a 12-month leave with salary from

the Bureau of Standards, as a reward for his excellent work in statistical mechanics. When Geoff suggested we meet for lunch, I did not know that lunch would also include Allan Sandage. All the talk during that lunch was about astronomy. I passed their test, probably because, unlike many astronomers in Washington, I read the *Astrophysical Journal* carefully, sometimes cover to cover. Astronomical journals were much thinner then.

Early in 1963, I did my first observing at Kitt Peak National Observatory. At that time, its largest optical telescope was 36 inches. I planned to study the rotation properties of our Galaxy by obtaining velocities of stars far beyond our Sun. I remember my first evening, sitting in the office, waiting for dark. A man walked in and I said, "I'm Vera Rubin." He replied, "I know who you are. I was running to get to your talk at the Haverford meeting of the AAS, and I slipped on the ice and hurt my knee." This was not a good introduction to Art Hoag, a future Director of Kitt Peak. Using the Kitt Peak 36-inch telescope, I obtained radial velocities of stars in the anticenter of our Galaxy. It was an ambitious project, but I hoped to learn about motions of stars within our Galaxy, and motions in more distant galaxies.

This was also the year that we drove across the country in August—two adults, four children ages 3 to 12, and camping gear and clothes on the roof. Bob chose to take his sabbatical in a location where I would also have significant new opportunities. It suited everyone if we started driving at 4 A.M., stopped at 9 A.M. for breakfast, and stopped by 3 P.M. for the night. En route we camped and hiked and fell in love with Jackson Hole, Wyoming.

Margaret and Geoff had only recently arrived in La Jolla from Chicago. To circumvent nepotism rules, Geoff was in the Physics department, but Margaret was registered in Chemistry. She delivered her inaugural address the year we were visiting, so that was an added gift. I remember that her brilliant talk had Astro-Chemistry in the title. Within a year or so, they were both in the Physics department. Margaret and Geoff had offices on the stunning new pier extending into the ocean. My office was next to Margaret's, and Geoff's was further down the pier.

La Jolla was a lively place. Our large home looked down over the ocean. Margaret had many unmeasured spectra of galaxies, so my primary job was to measure them. Only later would I understand what an exceptional introduction these activities had on my future career. Geoff was the boss. If I measured the galaxy velocities and wrote the paper by Friday, my name was placed first on the paper. If I did not write the paper by Friday, Geoff took my completed measured velocities and wrote the paper over the weekend, and Margaret's name came first. If my memory is correct, she and I ended up about 50/50 for the publications. I also had the joy of observing with Margaret and Geoff at the McDonald Observatory. The observer stood high at the top of the dome, so I felt as if I was standing on nowhere. The assistant, usually Geoff, carried the tiny 1-cm-loaded film up the ladders along the inner edge of the dome, handed it to Margaret, and climbed down with the previously exposed image. Loading the film was a little tricky. In total darkness, I had to unfurl the large role of skinny film, cut the required size, load the cut piece into the holder, return the entire roll back to its dark place, and hope that I had placed the film with the correct side up. If not, the exposure would be wasted. After a series with no errors, Geoff came over and asked me how I had avoided errors. Easy, I said. I cut two pieces, and when the loading is done, I turn on the lights. If the "tester" has the correct side up, then I know the loaded one is correct. If it is not correct, I turn off the lights, turn over the one in the holder, and I know that it is now correct. Soon I was observing on the edge of the dome. La Jolla had interesting visitors and meaningful lectures. Geoff was always willing to sit in my office and talk about astronomy. I remember a visit from their theorist friend, Kevin Pendergraft, from New York. Geoff and Margaret were to be elsewhere, and they asked me to be his host. My recollection of the day is that our youngest child, Allan, then three years old, spent the day lying on the floor, crying and yelling most of the time. Years later, when I met Kevin, I reminded him of the day and my son. It is hard to believe, but

he insisted seriously that he remembered the day, but not that a child was present. The year in La Jolla was a grand year, and I learned much and we enjoyed it all. I recognized that it would influence my career. But Bob and I missed the trees of the east. Going home to D.C. in 1964 was leisurely and lovely, with more camping, hiking, and green trees.

PALOMAR OBSERVING FROM 1965 ON

We returned to Washington only briefly, and then flew to the1964 Hamburg meeting of the International Astronomical Union. Fortunately, my parents enjoyed being with their grandchildren, now ages 4–13. Three recollections stand out from the trip. (*a*) I had five minutes to give a talk in the Galaxy session. The overhead projector would not work, so I held up some paper images and passed them around. I have now forgotten the emphasis of the talk, but surely it was galaxies. (*b*) Unknown to us, Margaret and I had each bought in La Jolla the same dark blue suit to wear at the IAU meeting. (*c*) At the closing banquet, Allan Sandage walked up to me as Bob and I were leaving the dance floor. He asked if I was interested in using a Carnegie telescope at Palomar, still not available to women. That is a moment I have never forgotten. Of course, I said yes. It was a known secret that Margaret Burbidge had observed there when she and Geoff were postgraduate students. Margaret took advantage of Geoff's privileges to use the telescope. I first observed at Palomar one long dark December night in 1965. My assigned bedroom was on the second floor of the dormitory, and there was a velvet rope at the first floor, blocking the stairs. When an astronomer asked why the rope was there, the answer was "because Vera Rubin is upstairs." On my first night it was cloudy, so the observer on the 4-m telescope (I think it was John Beverly Oke) took me on a wonderful tour of the 200-inch telescope grounds. When we came to the toilet, the one that kept women from observing at Palomar for decades, he opened the door with flair and said, "This is the famous toilet." Of course, the sign on the door said "MEN." On my next observing run, I drew a skirted woman, and pasted her up on the door. Progress: The second floor was not restricted. When I next returned, the skirted woman was gone, but new observing procedures were in place. Astronomers now observed from a warmed room, which had its own toilet. No more observing while standing out in the cold; no more sign for "MEN." I assume the dorm is still called the Monastery.

CARNEGIE: DEPARTMENT OF TERRESTRIAL MAGNETISM, 1965–PRESENT

By 1964 I recognized that observing was very important to me, so I chose to give up teaching. In January of 1965, I walked into the Department of Terrestrial Magnetism (DTM), one of the branches of the Carnegie Institution of Washington (also known as the Carnegie Institution for Science), and asked Bernie Burke for a job. DTM, founded in 1904 by Andrew Carnegie, had never had a woman on the staff, and until about 1942, even the secretaries had been male. Bernie and I were astronomical friends, and I would periodically show up at DTM to talk science and visit the library. This day, he was clearly startled by my request, but he invited me to stay for the community lunch, this for my first time.

During lunch, the director Merle Tuve invited me to go to the blackboard and tell the staff about my work. Even more importantly, at the table I was introduced to staff member Dr. Kent Ford. Kent had just returned from the Mount Wilson Observatory, where he had tested his image tube spectrograph. At a telescope, this electro-optical device decreased observing time by a factor of up to ten compared to the conventional photographic plate, with equivalent resolution. After lunch, I was handed a photographic plate from Kent's work and asked if I could measure the stars'

velocities. I said yes, carried the tiny glass plate back to Georgetown, and determined the stellar velocities. Less than three months later I had a job at DTM. Because I wanted to be home at 3:30 when the children came back from school, my salary was diminished by one-third. My first day at work, a secretary came to ask how I spelled my name. Not a single paper had yet changed hands. And even though it was April 1st, no one played jokes.

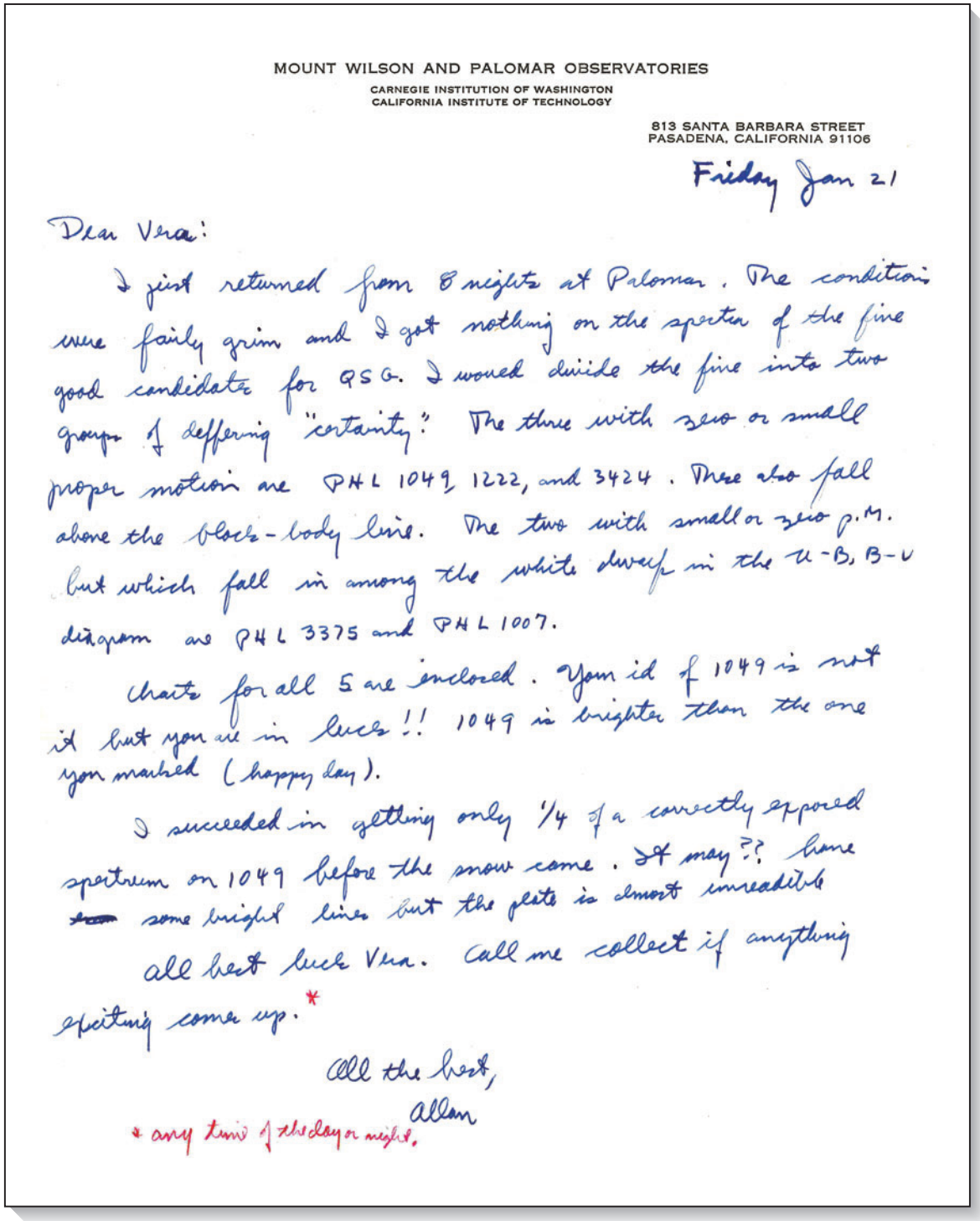
In August of 1965, I was observing at Lowell Observatory, Arizona, with Kent, using the 69-inch telescope (shortly to be replaced by a 72-inch). Kent was anxious to test the limits of the image tube, and I was anxious to obtain new information about galaxies. Kent's DTM Cassegrain spectrograph incorporated an RCA (Radio Corporation of America) cascaded image tube. A typical exposure time was 30 min for an unwidened spectrum of an object with magnitude $V = 17.5$. Knowing that the image tube permitted us to observe objects fainter than those the telescope was designed for, I arrived with finding charts with accurate positions of the neighboring bright stars we could see. At the telescope, in almost total darkness, we would measure from the visible star to the blank spot where we should observe. If we could see the object, so much the better. We obtained image-tube spectra of 3C 33, a radio galaxy; 3C 48, a quasi-stellar source; Tonanzintla 256, a blue quasi-stellar galaxy; and the red object in Cygnus discovered by Gerry Neugebauer, Dowell Martz, and Bob Leighton in 1965. We generally took several exposures on each, with times ranging from 5 to 46 min, median 15 min. Magnitudes ranged from 15.4 to >16. Because the galaxy exposures would be contaminated by the night skylight, we also took a 60-min exposure of the night sky, extending to almost 10,000 Å. After the observations, I carried the dry developed plates back to my Washington office, where I measured the wavelengths on a one-dimensional Gaertner measuring machine.

I chose to write most of the papers that Kent and I would publish. Our first paper using the image-tube, "Low-Dispersion Image Tube Spectra in the Red: 3C 33, 3C 48, TON 256, and an Infrared Star" was published on October 1, 1965 in the *Astrophysical Journal* (Ford & Rubin 1965). Not long after publication, Jim Peebles used our extended sky spectrum to discuss the spectrum near 10,000 Å. That was a delightful surprise to me and a joy that an astronomer could make unexpected use of our observations. About that time, I went up to the director's office and told him I needed a two-dimensional-measuring machine. He smiled and said, "You beat me by months. I knew you would come in and tell me you needed to measure in two dimensions, but I expected it next year." At the next AAS meeting, I described observations from both Kitt Peak and Lowell Observatories. It was a nice beginning, but only a beginning.

Our observations were not unnoticed by the astronomical community. This 1966 letter from Allan Sandage surfaced recently in my files (**Figure 1**).

ANDROMEDA

By 1968, we had obtained spectra of many galaxies, plus dozens of quasi-stellar objects, radio galaxies, faint blue objects, and planetary nebula, and published eight more papers. It was an exciting time, but I was not comfortable with the very rapid pace of the competition. Even very polite phone calls asking me which galaxies I was studying (so as not to overlap) made me uncomfortable. I realized that I wanted to do astronomy in a less competitive atmosphere. After some thought, I decided to complete our study of M31, the Andromeda galaxy, by measuring velocities of stars and HII regions in M31. The nearest large galaxy to us, M31 had been studied in the past, but with much poorer instrumentation. Andromeda offered a unique opportunity to study the dynamics of a galaxy like our own. Because of its relatively large extent on the sky, 2°, it is possible to obtain spectra of individual emission regions and, hence, to map the velocity field of a galaxy that resembles our own.



MOUNT WILSON AND PALOMAR OBSERVATORIES
CARNEGIE INSTITUTION OF WASHINGTON
CALIFORNIA INSTITUTE OF TECHNOLOGY

813 SANTA BARBARA STREET
PASADENA, CALIFORNIA 91106

Friday Jan 21

Dear Vera:

I just returned from 8 nights at Palomar. The conditions were fairly grim and I got nothing on the spectra of the five good candidates for QSG. I would divide the five into two groups of differing "certainty." The three with zero or small proper motion are PHL 1049, 1222, and 3424. These also fall above the black-body line. The two with small or zero p.m. but which fall in among the white dwarfs in the U-B, B-V diagram are PHL 3375 and PHL 1007.

Charts for all 5 are enclosed. Your id of 1049 is not it but you are in luck!! 1049 is brighter than the one you marked (happy day).

I succeeded in getting only 1/4 of a correctly exposed spectrum on 1049 before the snow came. It may?? have some bright lines but the plate is almost unreadible

all best luck Vera. Call me collect if anything exciting comes up.*

All the best,
Allan

* any time of the day or night.

Figure 1

A letter from Allan Sandage.

The first known successful spectrum of M31 was made in January 1899 by Julius Scheiner (1899). Nearly 80 years later, when I was attending an astronomical meeting in Armenia, I asked Hans Oleak, an astronomer at the Potsdam Observatory, if Scheiner's M31 photo had ever been found. "No," he said, "that was too difficult, after several wars that destroyed most instruments." I encouraged him to try, anyway. Some time later, a letter came: He had found the photo, but in poor shape! Later, he wrote a short paper concerning M31, including the 1899 image. The first large-scale study of motions in M31 was made by Horace Babcock in 1939, with the 36-inch Crossley telescope at Lick Observatory. He observed the unresolved central nucleus and a few spectra from individual emission stars or nebulosities. Exposure times ranged from 7 to 22 hours for each outer star. In the 1940s, Baade used the 100-inch Mount Wilson telescope to identify HII regions by their Hα radiation. Ultimately, he identified 688 emission regions. The publication was completed and published after his death (Baade & Arp 1964).

Knowing little about this earlier history, Kent and I planned to start by identifying emission regions in M31, regions that we would later observe. We spent one bitterly cold night at the U.S. Naval Observatory telescope near Flagstaff, Arizona, with a slide-off flat dome, in a temperature of about −20°. At the telescope, one of us identified HII regions that looked profitable for study. The other one huddled at a small heater inside the building, trying to warm up before we changed positions. By dawn we had decided that we would have to get positions of observable regions in M31 by a different method. We cleaned up, closed the telescope, and started walking away when we met astronomer Gerald Kron entering his office. We stopped to talk and told him what we were planning to study. He took us into his warm office, opened a large cabinet, and showed us copies of Baade's many plates of stars in M31! Each plate contained many emission regions in M31 that Baade had identified. For us, this remarkable gift of M31 stars and emission regions was truly coming from heaven. Gerry suggested that we contact the Carnegie astronomers, and especially the photographer, who would send us copies of the plates. After phone calls and questions back and forth, a meticulous Carnegie photographer provided copies of the spectacular images that Baade had obtained. On the images, Baade had carefully marked 688 individual emission regions that he had identified by their Hα radiation. Equally important, use of the image tube would decrease exposure times from 20 h to less than 1.5 h.

When the photographs arrived, I enthusiastically spent hours with each sheet, identifying regions where an Andromeda star or emission region was located. For each chosen M31 object, I measured a distance from three nearby stars and made forms for doing the necessary arithmetic at the telescope. These forms had identified spaces: Put star A here, B here, C here. At the telescopes, with a weak flashlight and freezing hands, we would do the final arithmetic and then move the telescope slowly, so that the invisible object would move to the exact center of our field. Our aim was to separate the Andromeda star from the many faint stars in our Galaxy. Starting from the first try, we never failed to obtain a clear, measurable image, but it was tedious and required four hands.

The first exposure was taken at Lowell. We had a guest astronomer watching us. When the exposure was complete, we all went to the developing room, and a few minutes later we had our first Andromeda spectrum. We were ecstatic but wanted to get back to the telescope to get our next exposure, so our guest volunteered to complete the developing for us. When we returned with the second plate, we found that he had accidentally washed the first plate in hot water rather than cold, completely washing off the emulsion. The glass was absolutely blank. Our guest was mortified, but I was so delighted that everything worked that I didn't care. I knew we would need hundreds of exposures.

Observing at Kitt Peak was different because we had to load all of our equipment from Lowell into the Lowell truck and make the 5-hour drive. At the loading dock, we were met by 4 to 5

Native American employees who, looking at the truck, would say "there must be a better way to make a living." I never knew if they were referring to themselves or to us. On a typical clear night we would obtain 4 to 5 spectra. The surprises came very quickly. By the end of the first night, we were puzzled by the shape of the rotation curve. In our Solar System, the orbital velocities of planets decrease with distance from the Sun, as discovered by Kepler and explained by Newton. The expectation was that galaxies behaved the same way, in that stars farthest from the massive center would be moving most slowly. In fact, the rotation curves were flat, meaning that the orbital velocities outside the center of the Galaxy did not decrease with distance. This was discovered over the course of about 4 ice cream cones that first night, as I alternated between developing the plates and eating (Kent would be starting the next observation). I remember thinking that there must be some mechanism for speeding up stars that moved too slowly or slowing down stars that moved too fast. Now, of course, the popular explanation is that dark matter is distributed throughout the Galaxy in such a way that the Galactic mass increases linearly with distance from the center, making the flat rotation curves consistent with Newtonian physics. As more and more time passes without direct observational confirmation, I wonder if the explanation is even more complex than we imagine at present.

During 1967 and 1968, Kent and I obtained spectra of 67 HII regions in M31, ranging from 3 to 24 kpc from the nucleus of M31. (By comparison, our Sun is about 8 kpc from the center of our Galaxy). Observations were made both at Lowell and at Kitt Peak Observatories, using the Carnegie DTM spectrograph that we carried back and forth. The dispersion was 135 Å mm^{-1}, and exposure times were generally 60–90 min. Radial velocities, principally from Hα, were determined with an accuracy of ± 10 km sec^{-1} for most regions. During this time, I also graduated to a two-dimensional Mann measuring machine at DTM. I presented our Andromeda results at the AAS meeting in December of 1968 in Austin, Texas. At the break that followed my talk, the astronomer Rudolph Minkowski came up to talk to me. After discussing the results at great length and being very supportive, he asked me when I was going to publish the results. I replied, "I don't know. There are many more regions we could observe." He looked at me very sternly, and said, "I think you should publish it now!" *Sky and Telescope* covered that AAS meeting in their March 1969 issue, with more than a page of plots, spectra, pictures, and words of our work.

Our paper "Rotation of the Andromeda Nebula from a Spectroscopic Survey of Emission Regions" appeared in the *Astrophysical Journal* in February of 1970 (Rubin & Ford 1970). The caption for figure 9, our first plot showing the superposed velocity curve with data, reads as follows: "Rotational velocities for OB associations in M31, as a function of distance from the center. Solid curve, adopted rotation curve based on the velocities shown in Figure 4. For $R \leq 12'$ curve is a fifth order polynomial. For $R > 12'$, curve is a fourth-order polynomial required to remain approximately flat near $R = 120'$." This was my second adventure with a flat rotation curve. This paper attracted slightly more attention. By 1975, Mort Roberts and Robert Whitehurst, using 21-cm radio observations, had extended the known M31 velocities about 50% farther than our optical velocities. The velocities remained flat.

STUDENTS AT THE DEPARTMENT OF TERRESTRIAL MAGNETISM

When I joined Carnegie's DTM, there was already in place a program to bring interesting students to work with a staff member. Within a year, we had our first request. Sandra Moore, a young student from Swarthmore College, spent the summer of 1966 (and a fraction of the next) working with Kent and me. She built instruments, worked on some of my reduced observations, and helped to write the paper. The published paper, "Faint Blue Objects in the Virgo Cluster Region," had authors Rubin and Moore; Dr. F. C. Bertiau, S. J., Vatican Observatory, Vatican City State, was the

third author of the paper (Rubin, Moore & Bertiau 1967). He has remained an active astronomer, author, and friend.

Sandy arrived again, several years later, to spend a year or two writing her Harvard Ph.D. thesis in my large outer office. By then, her name was Sandra Faber. While at DTM, Sandy decided that the University of California, Santa Cruz, with its Lick Observatory, was the only place she wanted to work. In 1972, she wrote a letter, visited Santa Cruz, gave a talk, and was offered a job. My letter of support had four short sentences, here from memory:

> Dear Dr. Kraft:
>
> I am writing in support of the application of Dr. Sandra Faber for a position at the Lick Observatory. Dr. Faber is brilliant, knowledgeable, innovative, imaginative, incisive, hard working, curious, clever, and demanding. I think there is no limit to what she may accomplish. She will be a fine addition to your staff, and she may one day be the Director of Lick Observatory.
>
> Sincerely, Vera C. Rubin.

Sandy is presently University Professor and Chair of the Astronomy and Astrophysics Department, University of California, Santa Cruz.

Another bright young astronomer, Sandro D'Odorico, was sent to DTM from Padua by his professor, Francesco Bertola. Francesco was already a friend, and Sandro had just completed his final degree. Sandro too sat in my outer office. We worked together, observed together at Lowell and Kitt Peak Observatory, and published several papers. Our first joint paper, "Observations of M82 in the Optical Infrared" (Bertola et al. 1969), took advantage of the image tube sensitivity in the red, but also took advantage of a brief visit from Francesco. Francesco had been studying M82 in Asiago and was interested in its nucleus, especially in the IR. Kent was able to get immediate observing time on the NASA 36-inch telescope, not far from Washington. Two exposures were made in February of 1969: 3h13 min and 5h39 min.

NGC 4038 AND NGC 4039, 1966–1970

Among the interesting galaxies that attracted my attention, NGC 4038/39 ranked among the highest. The Lowell plate vault had a plate of the pair taken by Lampland in 1921 with the 42-inch reflector. Shapley and Paraskevopoulos described the pair in 1940. In 1956, Zwicky suggested that during a gravitational encounter such as this, masses are ejected and later might form small galaxies. Margaret and Geoff Burbidge conducted a kinematic study of the object (Burbidge & Burbidge 1966). In 1966 and 1967, Kent and I, plus postdoc Sandro, observed NGC 4038 and 4039 at the Kitt Peak and Lowell Observatories. With exposure times from 45 to 120 min, we detected 19 separate regions with spectral lines ranging from Hβ to NII. Lines of [OI], [OII], and HeI had not previously been detected. We were able to identify the velocity gradients across the galaxies. From our efforts to understand, we concluded, "The velocity field exhibits a systematic variation of line-of-sight velocities from knot to knot, which leads us to infer a model of two rotating, interacting galaxies." This pair of galaxies had attracted my attention very early in my career, yet it now has become even more notable. More recent observations are discussed below.

FRIENDS

John Bahcall

One day in 1969, I had a phone call from John Bahcall, a physicist/astronomer I had not met. He said that he was a theorist, but he thought that he should know how observers work. A few months

later, he arrived at Lowell Observatory and joined Kent and me at the telescope. The first problem was that he did not have heavy shoes for the cold observatory. He realized that his efforts to learn to speak Hebrew to his relatively new Israeli wife Neta had resulted in her packing rain rubbers instead of warm boots. The second problem was not really a problem. In fact, it was a gift. The first time I joined Kent to observe at Lowell, I came with detailed finding charts. At the start of the first night, Kent, a very proper Virginia gentleman, said, "Please give me the finding chart." So I did, and we went to work. When we completed the first and were to start the second exposure, Kent started to repeat, "Please . . . ." I instantly said, "No, now it's my turn." From that time on, whenever we observed together, that was the recipe. But it always broke down after the first or second night, when Kent would take naps on the observing floor. I would nap relatively seldom, for I had other chores. One chore was developing the plate, if we wanted to see it immediately. So with John visiting, there were now two gentlemen on the floor. I was delighted to have more guiding time.

William T. Golden

Bill was a Carnegie Trustee, but he had broad interests. He was also very smart and charming. One day, I answered my office phone and Bill asked if he could come (from New York) to talk to me about observatories. He wanted to decide if he should accept an invitation to become an officer for a national observatory. What he really wanted to know was how an observer worked and how an astronomical organization worked. It was a long fun day. He asked tough questions one after the other, and I answered. By 4:30 P.M., when I was feeling that I might collapse of exhaustion, he asked a question I could not answer. And I still remember him saying, "Thank goodness. I'm exhausted, but you never seemed to stop." He repaid me by putting me on the astronomy board of the New York Museum of Natural History. That too was interesting, especially watching the young children light up when they learned something new. For many years, Bob and I would meet Bill at the annual meeting of the National Academy of Science, often driving him and too many others to the next activity. The many notes from him were always clever or informative, like this typical one.

> Dear Vera,
>
> Last summer you were good enough to send me a copy of your commencement address to the College of Computer, Mathematics, and Physical Sciences of the University of Maryland. As is often my wont, it took me a while to reach this on my reading pile. And I found it so wise and vivacious and moving that I have had eight Xerox copies made, and I'm distributing them to a number of friends who will surely thank me for them. And I thank you and, indirectly, the creator of the Peanuts cartoon. With warm regards and Happy New Year wishes, and admiration and affection,
>
> Sincerely, Bill.

Jan and Mieke Oort

In addition to the joy of learning about the Universe, astronomy is the route to meeting other astronomers, working with them, and becoming close friends. I met Jan and Mieke Oort at the 1960 summer school in Nyenrode, Breukelen. Later, Jan and sometimes Mieke also visited and stayed with Bob and me in Washington. Jan would walk with me to DTM. On one occasion, with the temperature in the 90s, I convinced Jan not to wear his jacket. But the warm weather made it possible to sit out on the deck with other astronomers. I remember Bernie Burke and other friends talking until 2 A.M. I also remember numerous occasions when I stayed with Jan and Mieke. Bob

had ties to Dutch scientists and would periodically visit. On one memorable occasion, I parted from Bob at the train and arrived about dinner time to stay with the Oorts. Shortly after arriving, Jan told me (he never asked) that the next day I would be one of the ten or so astronomers to question a candidate for his final degree exam. I spent much of the night reading the thesis, which was, fortunately, in English. Of course, the final exam was in Dutch, so I tried to look as if I knew what was going on. During the proceedings, I stood when the others stood and removed my hat when the others did. When it was my turn, the questions were in English and very gentle. The student passed.

On Oort's visits, or mine, most of the subjects centered around subjects he wanted to discuss. I could tell that he was not comfortable with dark matter discussions, which he avoided. One day in my office in 1986, he started quizzing me about dark matter. I then understood that it had become a legitimate scientific subject. At least for me, the discussion was memorable. Yet we never discussed his 1940 paper (Oort 1940) pointing out the absolutely flat rotation curve of NGC 3115. I had thought of questioning him about it, but I never did.

Jan handwrote many cards and letters. He wrote with tiny letters, some hard to decipher. Here are the words on a postcard, 1989, dimensions 6.3 × 7.6.

> Dear Vera, 2 Aug, 1989
>
> Thank you very much for your greeting of June 10 and your good wishes. We are very sorry we shant see you two during the summer. You will no doubt have a beautiful ocean trip, if you can avoid the strong weather we have had here during the last 4 days: beautiful clouds. I fear M will not be able to come to America as we are not very mobile this year. But I remain deeply interested in the wonderful data and the universe that you (and others) are so continually offering.
>
> Love from Mieke and me! Jan

I saw Jan last at his 90th birthday celebration in Holland. There was much science, but equally notable was a lunch at his son's home. His son, the senior financial officer in The Netherlands, had introduced a new financial system, and these were known throughout the country as Oorts. So, at least for that community, Jan had lost his superiority. We numbered about 20 for lunch, and during lunch Jan was asked which of his discoveries he was most proud of. He thought for a minute or two. Then he said it was probably the identification of the distant cloud of comets circling our Solar System, because this was so far from his usual studies. He did not call it the Oort cloud, as it was known. To the question "Would you like to travel to the moon?" he thought even longer. Then he said, "No, not to the moon, but I would go to Mars if asked." A few years later, after Jan's death, Leiden announced a program of visiting astronomers in Oort's honor. I spent a lovely few months in Leiden with Bob in 1995, lecturing, talking to students, and working. Oort was missed.

GALAXY MOTION, THE LOCAL GROUP, AND THE RUBIN-FORD EFFECT

Following the pioneering work of Hubble, observational cosmology was principally concerned with the value of the Hubble constant (H), as well as the value of the deceleration parameter (q_o). By the 1970s, instrumentation had improved to the point that astronomers could begin asking a new generation of questions. My colleagues and I posed the following: Is the Hubble expansion isotropic as observed from our Galaxy? Answering this required independent estimates of both the distance to distant galaxies and their recessional velocities. We embarked on a several-year

observing program to obtain the velocities for an all-sky sample of Sc I and Sc II galaxies, assumed to be standard candles. The galaxy sample and partial velocity results were presented by Rubin, Ford & Rubin (1973), and this was followed by an analysis of the full set of velocities by Rubin et al. (1976a,b). We found a striking anisotropy in the recessional velocities of galaxies with similar magnitudes (i.e., distances) located in different portions of the sky. Our sample of 96 galaxies was just a beginning. This was at a time when a few-hour exposure generated one or perhaps a few data points. Over the years, numerous astronomers have extended this work with larger and carefully selected samples (e.g., Lynden-Bell et al. 1987). Their conclusions were sometimes quite different. Now that it is possible to obtain 10^3 data points in a single exposure, this story will surely continue to evolve.

JAPANESE ASTRONOMERS AND FRIENDS: MY PEN PAL, 1977

Early in my career, I found myself corresponding with an astronomer who asked many interesting questions. His address indicated Kyoto. When, in 1977, I decided to accompany Bob on one of his trips to Kyoto, I told him I would be visiting. In return, my friend wrote back one sentence: "Dr. Z will be your host." I knew then that my friend was a student. When we arrived at the Kyoto hotel in the late evening, my host was sitting in the lobby, with a complete day-by-day schedule of my activities. On my first day, I met the faculty and had lunch and spoke with them. Around noon the next day, I was seated with about 20 or 25 students. Together we formed a large circle. The first student introduced himself and told me what he was working on. I proceeded to speak to him and to discuss his project with him. There were questions and discussions for about 15 minutes. This procedure continued, student by student. When finally a student said I am Ken-Ichi Wakamatsu, I had at last identified my postal friend. After about six hours, we all were finished, and I was exhausted but exhilarated. They were a very bright group.

The next day was a sightseeing day, with my student and two other students as tour guides. They told me that each student, terrified while talking with me, could not later remember what I had said, but other students remembered. So student by student they pieced my words together. I learned from them that I had been the first American astronomer to visit their campus. The rest of the visit was more relaxed but just as interesting. I was impressed with their enthusiasm to learn. On later trips, I would visit other Japanese astronomers and observatories, in cities and in mountains, and watch them observe through the nights.

LAS CAMPANAS, CHILE

One day, when I arrived at the Carnegie Observatory in Las Campanas, Chile, en route to the mountain and an observing run, I walked into François Schweizer's office. Within a few minutes, he picked up an image of a galaxy and said, "Look at this!" He showed me a photo of a polar ring galaxy, a galaxy with a ring circling its polar axis. I responded, "Oh, I know a different galaxy just like that." In that moment, we decided to study one. At that time, only a few polar ring galaxies (PRGs) were known. The best example was NGC 4650A, pointed out by Jose Luis Sersic in 1967 and studied by Sersic et al. in 1980. Francois suggested that we study NGC A0136–0801, which he had found on a prime-focus plate obtained with the 4-m telescope of Cerro Tololo Inter-American Observatory (CTIO). Our observations of A0136 were made in 1981–82 with the CTIO 4-m telescope and in 1982 with the Las Campanas 2.5-m du Pont Telescope.

A polar ring galaxy consists of a spindle-shaped S0 galaxy surrounded by a ring that runs over the Poles. The ring suggests a halo that is more spherical than flat. The system likely formed when a close encounter with a companion deposited matter into the S0 galaxy. Matter deposited

at ninety or near ninety degrees will survive longer than that deposited at a random angle. We concluded that A0136 is a stellar disc seen nearly edge on, as are the discs of the few other known polar ring galaxies. This low discovery rate suggests that these are rare events. We also suggested that the formation of polar ring galaxies around S0 galaxies is due to a second event, probably accretion. Some details from the abstract of our 1983 paper follow. We first present a detailed optical study of AO136–0801, a "spindle" galaxy girded by a ring of gas, dust, and young stars. The spindle is a normal S0 disk seen almost edge-on as shown by its photometric profile and fast rotation. The surrounding ring runs over the Poles. The ring motions suggest that a massive halo extends far beyond the S0 disk, and that this halo is more spherical than flat. We then list 22 related galaxies and derive that a few percent of all field S0's possess near-polar rings or disks. We suggest that these structures are due to a second event, most likely the transfer of mass from a companion galaxy during a close encounter. (See Schweizer, Whitmore & Rubin 1983.)

It would take several decades for polar ring galaxies to become important in helping us to understand the evolution of galaxies and clusters.

TINY GALAXIES AND SCHOOLS IN THE DESERT

Percival Lowell founded Lowell Observatory in Flagstaff, Arizona in 1894, 18 years before Arizona became a state. This observatory was a major step in his effort to learn more about the planets that orbit the Sun. In the summer of 1949, my parents invited Bob and me to drive across the United States with them. None of us had been west of Pennsylvania. Unfortunately, I had only four weeks free between classes and a summer school teaching obligation, so it required about 5,000 miles in less than five weeks. Roads had only two lanes, some all brick and some with a brick to note the lane edge. I was both delighted and depressed when we stopped to visit the Lowell Observatory. The tour was great, and the telescopes and science were impressive, but I developed a case of envy. These young men were already doing astronomy, and I was hardly beginning. Ten years later, Kent Ford was at Lowell doing early testing of his image tube that would enhance the speed of a photographic image by a factor of ten.

Deidre Hunter came to DTM in the Fall of 1985 as the first Carnegie Richard B. Roberts Postdoctoral Fellow. Upon completing her Ph.D. degree from the University of Illinois in 1982, she spent several years as a research associate at Kitt Peak National Observatory. Even then she was interested in small galaxies; most astronomers study large ones. She has worked at Lowell Observatory since leaving DTM. Many of her papers carry titles with words like small or irregular or amorphous or even normal irregular or blue irregular. From 1997–2007, she and her collaborators published 34 papers on this subject. The project name is "Little Things." Because star formation today in dwarf galaxies is similar to star formation just after the Big Bang, their studies should shed light on the birth of the first stars after the Big Bang. One recent discovery, Leo T, is the closest neighbor to our Milky Way galaxy.

Deidre and her group expect to learn how tiny galaxies form and to understand the importance of sequential triggering as opposed to random turbulence compression in their formation. Their observations extend over a wide spectrum: optical, UV, and IR. They expect that these data will help astronomers understand the processes that formed stars in the smallest galaxies. Their work may require astronomers to rethink our understanding of how stars form.

But Deidre has other serious obligations related to her science. In 1996, she adopted a class of fifth to eighth grade students on the Hopi Indian reservation located an hour or more's drive from Flagstaff. The program, which she still runs, has grown so that each year, Deidre and five or so Lowell astronomers work with a different teacher and class at a Navajo or Hopi Reservation. The astronomers come bringing bags of work for the students and teacher. On one occasion, the

students talked about stars and the sky. They made star charts of the sky in the afternoon class, picking out the brightest visible star. After dark, the class met again to view the sky, first with their eyes, next with their charts, then with binoculars, and finally with the telescope. There was little silence, some confusion, lots of questions, and some shouts of excitement. The goals of the program are to use astronomy to excite the children about science and to help the teachers to learn enough astronomy to incorporate it into their classrooms. Before the night was over, one of the very bright students announced his interest in becoming an astronomer! As part of this remarkable program, the students also visit Lowell Observatory and use telescopes after dark.

There is yet another aspect of the program. Every two years there is a several-day meeting at Lowell for the teachers connected with the program. For most of a day or two, the teachers divide into groups of four or five plus one assistant. Here, each 90 minutes, all groups carry out a new exercise that they will later present to their class. When they return to their classes, they tote four giant laundry-like baskets, each filled with several projects, plus all the materials required for 30 students to carry out each exercise. Students are given real projects that any astronomer might tackle. They examine and take notes of color photos of galaxies, classify them, identify planets, or pick out interesting objects to learn more about later. Deidre's activities reminded me of the words attributed to Adlai Stevenson in a tribute to Eleanor Roosevelt: It is better to light a candle than to curse the darkness. Astronomers, of course, like it dark.

A BRIEF CALENDAR, 1985

Jan. 5: To D.C., after celebrating New Year's in the snow of Jackson, WY.
Jan. 13–18: Tucson, AZ. VR is AAS Russell Lecturer. Dinner in Old Tucson, with former students and friends.
Jan. 26: Smithsonian talk and concert, Jerome Kerns.
Jan. 27–Feb. 3: Munich meeting, VR to report on and participate in Working Group open discussion on Galaxies and Clusters. Neta Bahcall, other friends.
Feb. 9: U.S. National Academy Class Meeting to discuss candidates.
Feb. 25: Dinner with Dr. Frank Press and ICSU Board.
Mar. 1–2: SCIENCE Magazine Editorial Board Meeting.
Mar. 5: VR informal lunch talk preceding Margaret Rossiter, author: Women in American Science 1840–1920.
Mar. 10–13: Austin, Tex. visit to participate in Symposium in honor of Bob Herman's 70th birthday.
Mar. 13: VR astronomy department talk, DTM.
Mar. 29–30: Meeting with Harriet Zuckerman, Women in Science, New York City.
Apr. 1–2: Space Telescope Science Institute, Baltimore.
Apr. 18–21: Jan Oort and wife Mieke visiting. Dinner party Apr. 20, with Jan, Burke, Kerr, Westerhout, and others; talking till 2 A.M.. Next day Oort signed U.S. National Academy of Sciences member book, years late.
May 1–4: To Lowell Observatory.
May 8–15: Observing at Kitt Peak.
May 27–31: Observing at Lowell.
Summer: First Vatican Observatory science Summer School for advanced students and post docs.
June 24–29: IAU Princeton, NJ.
Aug. 13: Vera phoned Willie Fowler and Kip Thorne. Subjects not recorded.
Aug. 28–Sept. 2: To San Francisco (science), and to see children and grandchildren.

Oct. 10–19: Observing at Las Campanas, Chile.
Nov. 7–17: Bob with colleagues and students in Japan; I accompany and visit astronomers and students.
Nov. 18–28: IAU meeting in India.
Nov. 25: Vera gives one of three invited IAU talks. Deidre Hunter travels with us and shares rooms. Lots of friends and fun.
Dec. 15–21: Observing at Palomar.
Dec. 22–29: Wyoming and Tetons, almost to New Year. New Year's Eve in D.C.

NGC 4550: A TWO-WAY GALAXY

Of all the galaxies I have observed, NGC 4550 ranks as the most amazing. It is located near the center of the Virgo cluster. In 1989, one hundred years after John Dreyer named it NGC 4550, I embarked on a program with Jeff Kenney to study the kinematics of galaxies in the Virgo cluster. I obtained the first spectra in 1989. Because observing was going so well, I decided to extend the survey to include S0 and E7 galaxies near the center of the Virgo cluster. It was the puzzle presented by NGC 4550 that forced me to enlarge my interpretation of Newton's law of gravitation. It is the most remarkable of the nearly 100 Virgo galaxies we observed and probably the strangest galaxy I have ever studied. It is a highly flattened galaxy, with an aspect ratio of one to three or four.

I took the first spectrum of NGC 4550 with the Palomar 200-inch telescope; Kent was observing with me. The instrument we were using produced two spectra of a single object—one spectrum in the red spectral region to include Hα and one in the blue to include the stellar absorption lines. The exposure was 2,000 seconds and the spectra were recorded digitally on a CCD camera. The spectra, viewed at the Palomar computer following the exposure, were then transferred to magnetic tape that I hand carried to the DTM computer. John Graham came on the following observing session, and it was he that I bothered most as we tried to interpret the spectrum.

Back at DTM, I saw immediately that this was a rare galaxy. It was known by that time that stellar velocities beyond the nuclear region have a constant velocity. The velocity of the Hα gas in NGC 4550 was equal to that of the stars, but the gas was orbiting in the opposite direction. This behavior was known for about a dozen other galaxies. But in addition, I suspected that on the blue spectrum, each of the absorption lines was double. The lines were weak, too weak to measure, but easy to worry about. I occasionally thought that I could make out a little M pattern.

In the early days of computers, those at DTM were located in a common room. One morning, by now annoyed by what I could not quite see, I promised myself not to leave the computer room until I understood what was going on in NGC 4550. After staring again and again, I thought that the secret would be found in the magnesium triplet, because there were three components, compared to only one at Hα. In a galaxy, all stars beyond the nucleus orbit with the same linear velocity. To an observer, stars on one side of that galaxy approach the observer; stars on the opposite side recede. Each star near the center rotates with a velocity that depends upon its distance from the center. Each star near the center has a velocity slightly different from its neighbor, as each approaches the central velocity.

So after more than a year of struggling, I took a piece of paper and sketched how the billions of stars in a rotating galaxy would appear to the skinny cut across the distant galaxy that my telescope sees. On the side approaching the observer, there is a constant negative velocity with respect to the galaxy zero at the center; on the receding side there is a constant positive velocity. And in the nuclear region?—velocities change smoothly from negative to positive. So I made such a plot on a piece of paper: a horizontal line, then a short diagonal, then a slightly higher line. Then I rotated

my paper, and repeated the flat, diagonal, flat, diagonal. There in the middle was an X. I had just learned that NGC 4550 had stars orbiting, some clockwise, some counterclockwise! What joy, just to finally understand this curious galaxy.

The question often asked is, Why don't the stars bump into each other? That answer is easy. There is enormous space out there in space. Relative to their diameters, stars are very very far apart. Proxima Centauri is the closest star to our Sun, yet 4.2 light years away. Thirty million suns could form a line between our Sun and Proxima Centauri. That's pretty empty. Now that I had learned the secret, we could finally publish the paper. That was fun.

There were several interesting repercussions from this result. I remembered that during the summer of 1961 in Boulder, Colorado, I had read a paper in the *Monthly Notices of the Royal Astronomical Society*, in which the author (theoretically) turned around half the stars in a sphere and then asked if the arrangement was stable. The answer was yes. The author was Donald Lynden-Bell, by then a close friend. I called him and told him I wanted to talk to him about that paper. He first told me that Allan Sandage had asked him the question. Donald also said that I was embarrassing him, for this was the first paper he had ever published and he was embarrassed ever since. Yet when he heard my story, he became my best supporter. Shortly after, I wrote Scott Tremaine, telling him of the observation and asking him the same question: Was a doubly-rotating galactic nucleus stable? He answered six months later, saying that it was more stable than a uniformly-rotating one.

Years before the discovery, Alar Toomre (1982) had discussed counter-rotating galaxies as "elegant curiosities" that would "require considerable care to notice, let alone measure." Paul Schechter later reminded me that many years earlier we had discussed the possibility that a galaxy such as NGC 4550 could exist.

FOR MARGARET BURBIDGE ON HER 80TH BIRTHDAY, WITH LOVE, 1999

Dear Margaret,

When we first met, I think it was at the University of Michigan summer school in Ann Arbor; you and Geoff were already well on your way to becoming young leaders of the astronomical community, and I was a graduate student working under George Gamow, one of the summer school lecturers. Did the words "role model" and "mentor" exist then? I think they did not. But for most of the women that followed you into astronomical careers, these were the roles you filled for us.

I remember the Rubin arrival in La Jolla in 1963, where I was to work with you and Geoff part time, while dealing with schools for three and preschool for one young Rubin, while Bob used his NSF senior postdoctoral fellowship to work in the Physics Department. You came into my office, sat down, and asked me what I would like to work on. I think we talked about galaxy rotation and especially barred galaxies. We looked through the Hubble atlas, which you must have carried in. But what I best remember was my elation because you took me seriously and were interested in what I had to say.

This visit followed an amusing lunch that you, Geoff, Bob, and I shared at a fancy, mostly male, La Jolla restaurant. The female models in the lunch-time fashion show kept arriving at our table and halting; you and I didn't know whether to ignore them and continue our discussion or pretend interest. I think Geoff finally told them to stop interrupting.

You played a different role in the early Rubin household, which I have never before admitted. When I had a distasteful task, perhaps washing the kitchen floor when it had gotten to the state that feet would stick to it, I would say in a very loud voice, "Do you think that Margaret Burbidge is washing her

floor?" Then one day I phoned you in California from D.C. to discuss something—the Equal Rights Amendment or AAS business? You answered the phone and said, "Could I call you back? Sara and some friends are here and I'm in the middle of scrambling an omelet." I hung up with great joy, knowing that you too shared the mundane duties of sisterhood.

One final story. When you became the first woman president of the American Association for the Advancement of Science, I wrote your brief biography for *Science* magazine. I called Willy Fowler to ask about the early days of B^2FH. He started by saying, "One day a Charles Laughton look-alike came into my office and said 'Why don't you work on problems of astrophysical interest?'" That remark I included. But I did not include his comment that when he visited Geoff in your small Cambridge, UK apartment, the bathtub was overflowing not with hot water but with books! As was the rest of the apartment. "How can she bathe, and look so lovely?" he wondered.

Well, it's a long time since those early days, but our mutual passion for spiral galaxies has kept us close. From you we have learned that a woman too can rise to great heights as an astronomer, and that it's all right to be charming, gracious, brilliant, and to be concerned for others as we make our way in the world of science. For these, and many more talents, we honor you today.

With love, Vera.

NGC 4435/4438

This galaxy pair had attracted my attention early in my career. Among my later 1985 scribbles, a note reads: "NGC 4435 suffers from being companion of totally distorted NGC 4438. Often mentioned, little studied." So in 1994, Jeff Kenney, I, Pere Planesas, and Judith Young returned to NGC 4435 and 4438. Jeff and I had often worked and observed together. Jeff had been a Ph.D. student of Judy Young at the University of Massachussetts, Amherst. I teased Jeff that this made him my grandson, for Judy is my daughter. Jeff and I observed together often, Judy and I not quite so much, and the three of us together was rare. Fifteen years later, with more sophisticated instrumentation, astronomers would have a better understanding of what went on with these galaxies.

AROUND THE WORLD

The White House

When in 1996 I was invited to join the National Science Board for a six-year appointment, I was happy to accept. The work of the Board is interesting and often important, sometimes writing reports for Congress, even testifying before Congressional committees. Unexpectedly, I started getting calls to come and talk to Hillary Clinton when she was preparing for events that included science. One afternoon, after a session relating to what Stephen Hawking would be talking about at a science program that evening, she offered me the opportunity to ask Hawking a question. The questions would be given to him in advance so that he could enter the question into his speaking device. Although I don't remember what my question was, I do remember that when I arrived for the festivities, I was told that the Vice President (in New England) had chosen to ask my question. In its place, I was to ask a different question live. I tried to make it a pleasant one.

There were other invitations, one to a surprise birthday party for Hillary, and sometimes events for the President. In 1993, President Clinton awarded me the Presidential Medal of Science, and I think it is true that he never forgets a name. This fun ended for me with the change of the administration.

The South Pole

I did not know that, as a Board member, I might receive another special reward. I and few others were invited to visit the U.S. installation at the South Pole. On a late November evening in 1997, Bob and I flew from Washington to Christchurch, New Zealand. There I would be indoctrinated, fitted for clothes, and, weather permitting, flown in a transport to our landing point, McMurdo Station on Ross Island. This is the main U.S. staging point for operation into the interior. Because this was the earliest flight to the base this year, most of the space in the plane was filled with necessary and science paraphernalia. Visitors and employees were squeezed in the very limited remaining space. It was difficult, but not impossible, to turn and glance out to the snow-covered spring-Sun terrain. I found landing at the base and walking on the snow were remarkable events for me. We were assigned quarters, taken to a meal (hard to tell which one), and prepared for the next day. I was asked if I would like to spend all of my time with the astronomers at the Pole. With a little embarrassment I asked if that meant that I would miss everything else, the penguins, the mountains, and all the other events. Without much difficulty, I voted for the penguins. The next morning at the Pole, the astronomer at the base told me not to spend the entire day at the telescope, as had been suggested, but to enjoy the tour. Anyway, I'm not a solar observer. In addition to the wonders at the Pole, we were treated to a Sun event—a circle around the sun with inverted arcs and other displays. Flying back to McMurdo later, I was exhausted but much too excited to sleep, so I watched the snow scenery and talked with the officers. They too were excited to have the Pole now accessible.

The remaining three days were equally exciting. Seeing some penguins involved flying near their homes and then landing. They would waddle near the plane as the visitors came out, but wander away if it was not exciting. Some smaller varieties had larger groups; some swarms looked like thousands at their home sites. Traces of history could be seen, including the remains of earlier explorers with clothes, books, and even foods that were still identifiable. McMurdo itself was interesting. Those who lived there year-round had tales to tell. Plants and vegetables were grown in greenhouses and tenderly cared for. Games were played. A normal stay was 15 months; two summers surrounding one winter. Some remain or return for a second term.

We arrived at Christchurch amid waiting families. Bob had been hiking. He and I now spent a few days visiting and hiking, some reached by trains. Then it was time to go home.

Japan 1997, Manchester 2000

During several trips to Japan, I had met and become friends with numerous astronomers. The XXIII International Astronomical Union (IAU) meeting in Japan in 1997 offered the opportunity to renew friendships and to make new ones. For three years, 1997 to 2000, I was the Chair of the U.S. IAU Commission. Apart from my professional duties, I was also responsible for producing the expected U.S. party, to which IAU officials from each country were invited, along with significant others and astronomers. Due to someone's poor arithmetic, I found myself with the honor of organizing and attending two such parties, IAU 1997 in Kyoto and three years later IAU 2000 in Manchester, England. Aside from the last-minute problems that kept several of us dashing around, each occasion was great fun for me.

ANNUAL REVIEW, 2001

In 1999, Yoshiaki Sofue, an astronomer at the Institute of Astronomy, University of Tokyo, Mitaka, Tokyo, asked if I would join him in producing a review for the *Annual Review of Astronomy*

and Astrophysics. I consented, with the condition that he be the first author. Our review, "Rotation Curves of Spiral Galaxies," appeared in volume 39, 2001. In looking toward the future, we concluded with the following words: "Perhaps we will be able to put to rest the last doubt about the applicability of Newtonian gravitational theory on a cosmic scale, or enthusiastically embrace its successor" (Sofue & Rubin 2001, p. 166). I, for one, am still waiting.

A BIRTHDAY PARTY

On June 14, 2009, at the Queen's University art gallery in Kingston, Stéphane Courteau opened "Unveiling the Mass: Extracting and Interpreting Galaxy Masses." This "Celebration of Vera Rubin's Career" was interesting, fun, full of good talks, good food, and parties and boat trips. Everyone enjoyed it all, especially Vera. Participants came from all over the world. One friend who could not leave Italy, Francesco Bertola, surprised me with a telephone call during a session, arranged by the organizers. Sandro D'Odorico, my first postdoc at DTM at the time we were studying M31, came from the Max Planck Institute, where he now designs big telescopes. It was a joy to see so many friends enjoying it as much as I did.

Stéphane and his committee did a remarkable job in satisfying the almost 150 speakers and poster people. Sandra Faber's evening public talk was beautiful and enjoyed by all. The talks and posters were great. Thank you Stéphane for so many nice events.

On the last day of the conference, I was asked by Pavel Kroupa if I would be disappointed if the flat galactic rotation curves turned out to be due not to dark matter but to some form of non-Newtonian physics. I replied that I would "be delighted, since the non-Keplerian rotation curves are an empirical observation of hitherto not understood physics, and one needs to keep an open mind in seeking solutions" (Kroupa et al. 2010, p. 20).

NGC 4406 (M86) AND 4438: A SPECTACULAR Hα COMPLEX IN VIRGO

There are many wonderful features about our current astronomy. Each year brings more surprises than the previous year. In fact, it is probably an accelerating process, which I shall now name Vera's Law. You need not agree and you need not use my law. But as I write this in 2010, astronomers understand much more about the Universe and about complex galaxies than could have been imagined forty years ago. Since then, many questions have been clarified, but many more complex questions remain to be studied and understood.

In 2008, Kenney, Tal, Crowl, Feldmeier, and Jacoby produced strong evidence that M86 and 4438 had suffered a previously unrecognized collision (Kenney et al. 2008). Moreover, they proposed that the skinny Hα features connecting M86 and NGC 4438 were caused by the passage of NGC 4438 through the Inter-Stellar Medium of M86. Their remarkable picture is reproduced here as **Figure 2**. I think that this picture is one of the great science pictures of the century. Read their paper for many more exciting details.

THE ANTENNAE GALAXIES (NGC 4038/4039) REVISED: ADVANCED CAMERA FOR SURVEYS AND NICMOS OBSERVATIONS OF A PROTOTYPICAL MERGER

A comprehensive paper published in 2010 by Whitmore, Chandar, Schweizer, Rothberg, Leitherer, M. Rieke, G. Rieke, Blair, Mengel, and Alonso-Herrero will serve as a learning and observing guide for years to come (Whitmore et al. 2010), as will other current exciting papers. In their first paragraph, Whitmore and colleagues identify the 32 published papers that are related

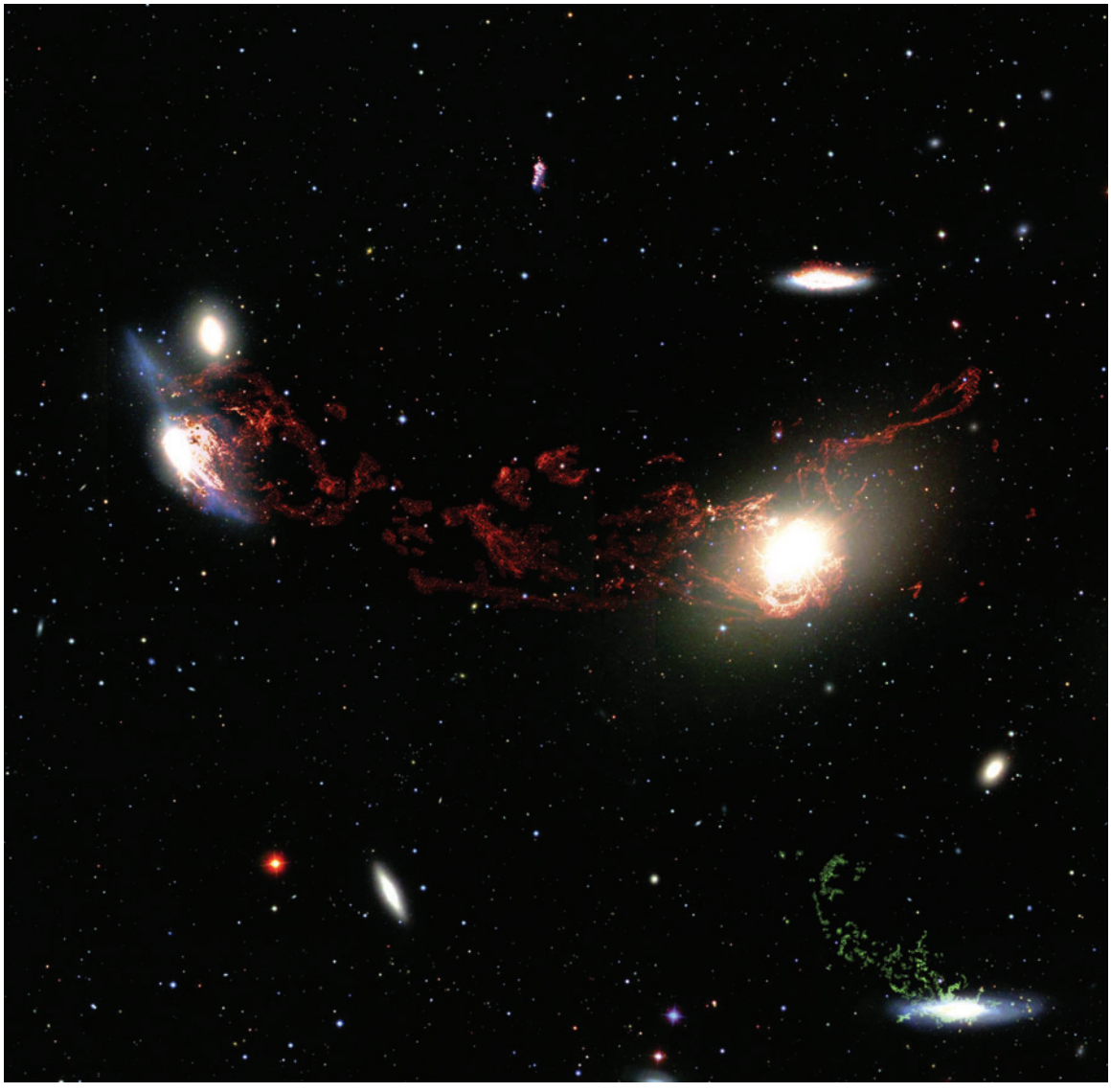


Figure 2

Hα features connecting M86 and NGC 4438, proposed to be caused by the passage of NGC 4438 through the ISM of M86. Photo courtesy of Jeffrey Kenney.

to NGC 4038/39: 31 were published in 1995, or more recently, and 1 in 1970. Forty years have brought not only 32 more papers, and many more astronomers, but also much more understanding. Using the Advanced Camera for Surveys, the Near Infrared Camera and the Multi-Object Spectrometer (NICMOS), Whitmore and colleagues extended the cluster luminosity function by almost two magnitudes, among many other new results. We are fortunate to live in an era when it is possible to learn so much about our Universe. But I envy our children, our grandchildren, and their children. They will know more than any of us do now, and they may even be able to travel there!

400 YEARS OF GALILEO, JANUARY 2010

In January 1610, in Padova, Italy, Galileo turned his newly perfected telescope to the planet Jupiter. After several evenings of observing, he had discovered four small objects orbiting the planet. He correctly identified these as moons of Jupiter. This discovery removed the Earth from its unique position, and introduced a new era in astronomy. To celebrate this event, and to close the 2009–2010 International Year of Astronomy, several hundred astronomers gathered in Padova, Italy, January 5–10, to celebrate Galileo's discovery. It was a great joy to be back in Italy with Francesco Bertola and Sandro D'Odorico, my friends for 44 years. In addition to historical talks and much food and friendships, there were discussions of current studies, especially of extrasolar planets. Evenings were filled with a climb and observing sight, a costumed play about Galileo, a view of Galileo's telescope, and a remarkable concert by students of the Galilean School of Higher Studies. These young adults excel in both science and music.

Dr. Catherine Cesarsky, chair of the International Year of Astronomy, introduced the final session. Representatives from various countries described their nation's astronomical activities. The final address by Dr. Robert Williams, current IAU President, was followed by a formal closure of the International Year of Astronomy.

This was not the last celebration. Francesco Bertola, now a distinguished professor at Padova, asked if I would meet and talk with his students the day following the meeting. Although the hotel was only a few blocks from the university, he and his wife met me with their car, and I had no idea where we were going. After an hour or so, we arrived at an old estate with gardens, trees, and flowers; the Villa Sagredo Bano. This ancient villa (before 1300) was once Galileo's home, and it was here that I was meeting the students. This ancient mansion was built and rebuilt over the centuries; Galileo had lived at the villa for some time in 1600. A guide walked us around the gardens and the house. As the 14 graduate students arrived at the Villa, they joined in the walk. At meal time we were seated at an arched table; the eight women students were seated on one side with me and the young men were seated across the table. One by one, I learned about each student's interests, their classes, plans for completing the courses and plans for the future. It was a fascinating afternoon and we all learned from each other. I learned more than the students.

SOME COMMENTS BY THE NEXT GENERATION

Dave

One evening, when I was a child about ten years old, my mother told me that she knew something about astronomy that no one else knew. To this day, I remember thinking that this was extraordinary. What I learned later was that all scientists discover things that no one else knows (because this is the point of science), that some new ideas are more interesting or more important than others, and that what my mom alone then knew was the beginning of the story of dark matter.

Judy

Being a member of the Rubin family has been a great joy and remarkable adventure. While growing up, my three brothers and I were surrounded by love at home and by a network of extended family. We saw our parents working hard and having fun being scientists, but none of us knew at the time that we would all choose to follow their lead. My decision to become an astronomer happened suddenly. During my senior year in high school, I took an astronomy course that Vera taught. At that time, chemistry was my favorite subject (and also my father's field), and I planned to major in

it in college, or so my applications said. But Vera is an excellent teacher, and the day we learned about black holes I was hooked. Here was a science that accepted the existence of that which could not be seen, and I decided to become an astronomer on the spot.

That decision led me to a Ph.D. in physics from the University of Minnesota in 1979. And almost 30 years later, in commenting on my switch from chemistry to astronomy, my father said that I had "defected." Because I changed my name when I got married during graduate school, it is not obvious to other astronomers that Vera is my mother, and there are still some who do not know that I am her daughter. Because I also study galaxies, we have collaborated and written papers together for over 38 years, and we have had the opportunity to go to meetings together in Canada, Italy, Japan, and the United States. Throughout my career, Vera has been one of my greatest champions and mentors, and I would not be an astronomer today were it not for the encouragement of both of my parents. I feel truly blessed and deeply grateful to be able to say, "Vera Rubin is my mother."

Karl

I'm not sure when I realized that growing up in a household headed by two scientists was unusual. As a young child, I just assumed that almost all adults were scientists and that astronomy was a job for women. Although I eventually came to understand otherwise, the lifestyle I was exposed to still had a major influence on my life and career path. Although there were many NSF summer programs for high school students, not many people knew about them. My parents did, and because of this I ended up spending several summers at Ohio State University, where I worked with real mathematicians for the first time. There was never ever pressure to become a scientist, but it did seem like the natural thing to do. While growing up I enjoyed solving problems, and I enjoyed traveling all over the world to both parents' conferences and sabbaticals.

These are things I still enjoy today about my academic life, and I am grateful that I found my way to such a career. I've learned that research can be frustrating as well as exciting, and that having parents who understand and encourage such a life is an advantage most of my colleagues didn't have.

Allan

I think it's no coincidence that the four children all ended up doing science. A pervasive early memory of mine is of my mother and father with their work spread out along the very long dining room table, which wasn't used for eating unless a lot of company was expected. At some point I grew old enough to realize that if what they really wanted to do after dinner was the same thing they did all day at work, then they must have pretty good jobs. I'm told that when I was 5, shortly after Vera started at DTM, I asked her if she had to pay them to let her work there.

Life at home seemed pretty normal. About as far back as I can remember, Vera would spend a week or two each year at Kitt Peak. When I asked where she was, the answer was that she was "observing." I don't think I made the connection to the standard definition; "observing" was just the word describing what she did when she was away from home for 1–2 weeks. There was a lot of discussion of astronomy at the dinner table. I remember that for third grade show-and-tell, I brought in a figure from one of her papers (Rubin & Ford 1968, p. 431). If you look closely you'll see she included a scratchy ALLAN hidden in the hand-drawn sketch of NGC 3226 (searching for all the Hirschfeld NINAs was a regular Sunday morning activity in our house). I have a vague recollection that my classmates were not nearly as excited as I thought they should have been.

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.

ACKNOWLEDGMENTS

The entire family deserves credit for making our careers work. But it was Bob who insisted that it could work and made certain that it did. He was a very special person, and we miss him.

It is not possible for me to identify by name all the wonderful friends who helped my career work. To the relatives, the teachers, the Georgetown University and the DTM scientists, the Lowell, Kitt Peak, Palomar, and Chile astronomers and staffs, the coworkers, the astronomers and friends around the world who helped make it educational and also made it fun, I send you each my thanks, my best wishes, and my love. To Sandy Keiser, who patiently answered all my questions and who solved many problems by walking to my computer and doing it herself, I send a special thank you. She made my days pleasant.

Vera Rubin's portrait was taken by Mark Godfrey.

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