Annu. Rev. Astron. Astrophys. 1994. 32: 1–36

WATCHER OF THE SKIES

E. Margaret Burbidge

Center for Astrophysics and Space Sciences, University of California, San Diego, La Jolla, California 92093-0111

FAMILY BACKGROUND

It is presumptuous to borrow words from a poet for the title of this prosaic account of a lifetime in astronomy, but I do so because of my love of the poetry of Keats. My love of astronomy will, I hope, be clear from this memoir.

My lifetime in astronomy begins with my family; therefore that is how I will begin this account. My father was a chemist—a lecturer at the Manchester School of Technology (MST), and my mother was one of his students. Her determination, her will to succeed, and her interest in all the natural sciences are gifts that she passed on to me.

My mother told me that she had an excellent, supportive teacher in high school, who suggested that, with her ability in mathematics and natural sciences, she should, after leaving school, enroll as an undergraduate at the MST. But my widowed grandfather, typical in his British 19th century view of the traditional path his younger daughter's life should follow, said "no"; she should apply herself to the arts of housekeeping and, since she was very attractive, she could expect to find a suitable husband without difficulty.

Here came the crucial support from my mother's school teacher, who encouraged her to sit for a scholarship exam offered by the MST, and guided her studies in preparation. The exam was taken without my grandfather's knowledge and, not surprisingly, my mother won a scholarship.

It was time for the crucial confrontation: The school teacher made an appointment to talk with my grandfather, and pointed out that it would be civically irresponsible to deny such a promising student, so high in the scholarship exam results, the opportunity to accept the scholarship and attend the college. My grandfather was stern, but basically kind and certainly civically responsible (for example, he was a strong supporter of the well-known Hallé Orchestra of Manchester, and a generous provider of financial support to struggling young musicians). Therefore, he dutifully but reluctantly gave his consent.

0066–4146/94/0915–0001$05.00

E. Margaret Burbidge

My mother chose to major in chemistry at MST. She never revealed to me the steps that led to that decision, so I can but guess. Chemistry is fascinating, and its relation to the 19th and early 20th century optimistic view toward improving the quality of life for all, must have played a part. And I cannot rule out the immediate reaction of a sheltered, 18-year-old girl to my father, who was good-looking, a very gifted experimental chemist, and a wonderful teacher, with a strong sense of humor, seventeen years older than herself, and with an eye for a pretty girl. In his class there were only two women students taking chemistry. His twinkling blue eyes must have settled on my mother. She told me that all the students joked about his idiosyncrasies, something that happens only to teachers who have strong personalities and are either excellent or very bad teachers. He was good. He taught inorganic chemical analysis from a small textbook which he had written, although his own natural interests were in organic chemistry. My mother told me that in his classes he described various precipitates resulting from standard inorganic test procedures, in ways that produced giggles from the two women students. A precipitate was described as "lavender-violet in color"—which did he mean? He clearly knew little about colors of women's dresses. But his description of one precipitate (or solution, I do not know which) as "peach-blossom color" set his name for that class forever: Stanley John Peachey was now referred to by the class as a whole as "old Peachblossom."

His name, my maiden name, "Peachey," was reputed to come from Huguenot ancestors, presumably fishermen, who fled from Brittany to England during the persecution of the Huguenots.

The inevitable happened: My mother and father married despite my grandfather's opposition, in 1916 (a signet ring of my father's which I possess bears the inner inscription March 12, 1912, and I never extracted the significance of that date from my mother).

After their marriage, my father obtained lucrative patents for some of his research inventions in rubber chemistry, particularly one which greatly speeded up the vulcanization of rubber. He left his teaching position, they moved to London around 1921, and he set up his own industrial chemical laboratory for further research.

I was born on August 12, 1919, a date which, at age 11 or 12 when I had been given a much Bowdlerised account of the "facts of life" by my mother, struck me as a weird and wonderful coincidence: The beginning of my existence as a few-celled creature must have coincided with the World War I Armistice—November 11, 1918. My excitement in telling my mother this deduction was not greeted with enthusiasm nor with any further explanation.

EARLY YEARS; SCHOOL YEARS, FUN WITH THE STARS, MATHEMATICS, AND SCIENCES

My pathway to astronomy led through what was then a fairly normal route. At age 4, before beginning school, my first view of the beauty of stars in the summer sky during a night-time boat crossing from England to France was the earliest step toward a lifetime love of astronomy. Then I developed an early interest in arithmetic and in numbers (especially large ones with many powers of ten to write out and contemplate); this began in my first years in school. I had learnt to read before going to school, so books were a continuing delight. My parents gave me books written for children on all the natural sciences, and reading these was coupled with both my mother's and father's willingness to show me and tell me about the wonders of the seashore, of flowers, plants, and trees (both my sister and I became passionate tree climbers throughout Hampstead Heath, near which we lived). My love of flowers is lifelong, and has been inherited by my own daughter.

I did not have a telescope; viewing the stars and planets was confined to what I could see with my father's binoculars. My parents subscribed to a weekly publication, *The Children's Newspaper*, which carried a regular feature on an inner page that described currently interesting sights, such as the brightness of Venus in the evening sky, the phases of the moon, close passage of any naked-eye planets by each other or past noticeable stars, and sometimes there were descriptions of constellations. I became fascinated by the changing phase and position of the moon, and it remains an ingrained habit to glance up at the moon, particularly when it is near new or full. Obviously that habit has been deeply embedded in my mind by the observational astronomer's concern with the bright or dark half of the lunar cycle!

In the present-day climate of concern over the quality of education in the U.S., it distresses me to read of people who have never wondered what causes the changing phases of the moon, who have no idea what causes the seasons, and some who do not even know that the Earth moves yearly in orbit around the Sun, much less what causes a lunar eclipse. How many decades of abysmal apathy have led to this state of affairs? We cannot reach out and touch the stars, but it is a revelation to witness the fascination one can arouse by giving a popular talk or slide show to school classes or to groups of teachers.

My father gave me a microscope and a chemistry set. I had great fun with both, although my father was always watching rather anxiously over the latter, exhorting me to be careful and to be clean and tidy in preparing my chemistry experiments. One of the tales my mother told of her undergraduate years was of "Old Peachblossom" reiterating to his class that they should be able to carry out chemistry experiments "on top of a grand piano, wearing evening dress" (although why they should want to do so was a good question). He deplored my mother's lab overall, spotted with acid burns and other spills.

When I was 12 or 13 years old, my grandfather gave me Sir James Jeans' popular books on astronomy. Suddenly, I saw my fascination with the stars, born at age 4, linked to my other delight, large numbers. That the nearest star is 26,000,000,000,000 miles away revived those excitements of my first school years (although falling short of my then favorite contemplation, 1 followed by 36 zeros). I decided then and there that the occupation I most wanted to engage in "when I was grown up" was to determine the distances of the stars. My mother recalled telling me, as I lay on my stomach on the floor reading the wonders described by Jeans, that it was bedtime, and that I pleaded for a little more time: "Mum, it's so exciting!"

At school (Francis Holland School for Girls), the teaching of general science (not much physics, some chemistry, more botany) was not extensive, but mathematics was taught well and the science teacher, Mary Pearson Barter, was excellent; I have never forgotten the debt I owe to her. My last year in school was spent in waiting for my 17th birthday, when the Dean at University College London (UCL) would accept me as an undergraduate. During that last year, I attended only the classes I chose—more mathematics, French and Latin, beginning German, and most enticing, individual lab work in science. Miss Barter gave me permission to work alone in the lab, with some rather primitive physics equipment which was stored in boxes in the greenhouse on the roof. There were magnets, circuits, lenses, mirrors, prisms, and suchlike, all very simple; she recommended an easy first-year undergraduate textbook on experiments that one could set up oneself. All my friends in their final school year were studying for Oxford entrance (history, English literature, languages, etc), or were still studying for what are now called O and A levels. Although discipline at that school was fairly strict, there was no ban on nonconformity and the incipient divergence of our paths through life created no problems—those girls remained my friends.

1936–1947: UNIVERSITY, WORLD WAR II, GRADUATE STUDIES

For my university years, my mother (my father had died after a long illness) chose University College, London, (UCL on Gower Street). It was a wise choice as it turned out, much better than Cambridge would have been. A first year of physics, chemistry, and pure and applied mathematics, was leading toward the choice between chemistry or mathematics as a major, when I discovered that UCL offered a major in astronomy with a math minor. I had thought that my interest in astronomy would have to be pursued as an amateur, but it seemed that there might be career possibilities. So, without worrying about future jobs, I began studies with Christopher Clive Langton Gregory (C. C. L. Gregory, whose son is the well-known expert in visual perception, Richard Gregory) and Elizabeth Williamson.

Experimental work was carried out with some difficulty, considering the frequency of cloudy nights, on two small telescopes in the forecourt of UCL. If the stars were veiled by the clouds over London, we learnt the intricacies of setting up telescopes, determining the errors of alignment, collimation, circle calibration, principles of navigation, and how to measure and compute orbits of binary stars (artificial ones, created by small lights reflecting in a mercury bead!).

In my third year, astronomy students took courses in atomic and molecular physics from Professor Dingle and Dr. Pearse at Imperial College in South Kensington, and, a highlight of the year, we were taken to the University of London Observatory (ULO) in Mill Hill, which seemed at that time to be on the outskirts of London and therefore a "real" observatory. We were shown, without being allowed to use them, the 24-inch reflector, the gift of which Gregory had arranged with a donor (Mr. Wilson) in Ireland, and the twin Radcliffe refractor, an 18-inch visual and a 24-inch photographic instrument, again acquired by Gregory's efforts from Oxford (Gregory 1966), where they had been housed in the Radcliffe building. On learning that the 24-inch was beginning a program of measuring stellar parallaxes, I remembered the James Jeans books of my childhood and, for a few months at least, declared my principal goal in astronomical research to be the determination of the distances of as many stars as possible.

Those three years as an undergraduate passed all too quickly, and created, or rather fostered, an enduring fascination with the physical sciences—the many branches of physics, especially astronomy and astrophysics, and geology. These years also provided an awakening on the social side, involving new male and female friendships. My favorite workplace at the time was a certain desk in the science library, on the ground floor to the left of the imposing steps and portico of UCL. It was, sadly, later demolished by Hitler's bombs during the war; I felt that as a personal loss.

My graduation was marked by no ceremonies; in the summer of 1939 it was obvious that Britain was headed for war with Nazi Germany. What useful work was I fitted for? Gregory provided me with an introduction to a computing firm which worked on the tables for the Nautical Almanac. Today's readers of this Prefatory Chapter should realize that computing, in those far-off years, was done mainly by logarithms and hand-cranked small machines. Helped by my B.Sc. record, and the recommendations of Gregory and Williamson, I was accepted, and left early on a Monday morning for my first day at work. It did not turn out well. A lengthy private luncheon, which I had assumed would be an occasion to welcome me into the firm, with an opportunity to meet other employees, convinced me that I did not want to work there. I made a quick and, to me, important decision: At the end of one day's work, I submitted my resignation.

For a few weeks, I joined my mother who had chosen the ARP—The Air

Raid Wardens—as her war work. The post to which she was assigned was in Hampstead, not far from the flat where we lived. The work was of course unpaid, but as she was in comfortable, though by no means wealthy, circumstances, she did not want to earn wages or salary, she simply wanted to serve her country with useful work to the best of her ability. She hated Hitlerite Germany, and to the end of her life could not forgive even those Germans who had suffered under Hitler.

September 3, 1939, dawned, and England was at war. For months it was referred to as "the phony war," because the air raids which had been expected to start immediately did not happen for months. Thus my "work" in the ARP was as a "gofer"—a runner of errands, a body for first aid learners to practice their coming tasks of rescuing and giving first aid to injured people from bombed or fire-bombed buildings. My mother's tasks in the daytime were clerical, in the ARP post, and at nighttime, checking all houses and flats in the area to make sure all windows were properly blacked-out, and helping those who could not fix their curtains effectively. A census of inhabitants in the area for which her post was responsible was kept up-to-date, so that in the event of direct bomb hits the wardens and police could know how many injured or dead were to be taken care of.

When the Battle of Britain started, I had a good and, I felt, more useful job offered by C. C. L. Gregory. He had been sent to the Admiralty, for classified work concerned with protection against submarine warfare and bombing attacks. At ULO, the astronomer had gone into the Army, and the mechanic/technician had gone into the Airforce for work on repair and maintenance of airplanes. I was offered a job which combined caretaking, maintenance of equipment, seeing about repairs to buildings for shrapnel damage, and the opportunity to use the 24-inch Wilson reflector and a spectrograph for a start on research towards a Ph.D.

Mill Hill, northwest of London, suffered relatively little from the German bombing attacks; it was southeast London that bore the brunt. The 24-inch mirror, deemed replaceable, was left in place, but the lenses of the 18- and 24-inch refractors were removed and stored safely at the bottom of the concrete telescope pier. Starting in 1940, I used the Wilson telescope and its inefficient spectrograph to work at night on Be stars—my Ph.D. thesis project. This ousted parallax work as my abiding interest, and stellar spectroscopy became part of my life. There were two other war-related occupations during the next few years—growing vegetables for the Gregory household in the grounds around the Observatory, and doing subcontract work for the Ministry of Defense on two small individual projects, one assembling a consignment of optical instruments for measuring approach angle and speed of attacking aircraft, and one under a microscope filling two small holes etched in glass plates with a suitable evaporative oil and powdered black enamel, for use in stereoscopic instruments for assessing photographic surveys of allied bombing results on enemy targets.

Those nights, standing or sitting on a ladder in the dome of the Wilson reflector, guiding a star on the slit of the spectrograph, fulfilled my early dreams. I have never tired of the joy of looking through the slit in the darkened dome and watching the stars. The Wilson telescope was so antiquated that it was driven by a sector operated by a hanging weight. One had to remember the times for stopping the drive, setting the sector back, and winding up the weight for its next fall. I often think about the joys of work in an open dome, under the stars, next to the telescope, joys denied to most younger astronomers and students who must sit in a warm console room, facing a television guiding screen and many complex computer interfaces, well removed from the telescope itself. I feel lucky that for 25 years after my career in astronomy really began, my telescope work was always conducted in admittedly ever-improving and more sophisticated ways but ways which my early training had led me to love.

The purpose of my research was to try to understand the physics of the Be stars, using Gamma Cassiopeiae as the prototype. The broad H and He absorption lines indicated fast rotation; double emission lines within the absorptions would come from a rotating ring or disk. The aperiodic variations in relative intensity of the emission doublets had been studied especially by D. B. McLaughlin, Otto Struve, and R. E. Baldwin. The model envisaged involved outflow from the stellar surface, maybe due to equatorial instability, and the appearance of sharp absorptions, strong in metastable He I λ 3889, indicated the formation of an outer shell of gas that was stable for months before eventually dissipating. While sitting on the observing ladder, I used to picture the star, its surface seething with turbulent outflowing gas, driven by radiation pressure and balanced by stellar gravity versus equatorial instability from the rapid rotation. I used to picture those photons traveling through space and time, waiting for me or someone cleverer than me to make sense of the physics of the processes in the outer layers of such stars.

With the end of the European side of WWII, VE Day, the small ULO staff returned to Mill Hill. C. C. L. Gregory, back from the Admiralty, directed the replacement and adjustment of the lenses in the refracting telescopes, stellar parallax and proper motion work resumed, Roger Pring returned from the army to become First Assistant, the mechanic returned from the Air Force, and I became Second Assistant. Observations made at post-sunset and pre-dawn for parallax, and in the middle of the night for proper motion, were resumed, and the limitations of the Wilson Reflector and its spectrograph became increasingly apparent to me. The exchange of astronomical literature recovered after the hiatus caused by the war, and the publications by Otto Struve especially provided the goal and impetus toward my aim to have access to larger telescopes, better instruments, and clear skies. Having read an advertisement in, I think, *The Observatory* for Carnegie Fellowships at Mt. Wilson Observatory, I put together an application; I did not have many publications but my undergraduate record was good and my research plans in stellar spectroscopy would, I felt, give me a reasonable chance for the award of a Carnegie Fellowship.

The letter of denial opened my eyes to a new and somewhat frightening situation: new, because I had never before experienced gender-based discrimination. The turn-down letter simply pointed out that Carnegie Fellowships were available only for men, although the advertisement had not stated that fact. Apparently, women were not allowed to use the Mt. Wilson telescopes, and Carnegie Fellows were guaranteed the right to apply for observing time on those telescopes.

A guiding operational principle in my life was activated: If frustrated in one's endeavor by a stone wall or any kind of blockage, one must find a way around—another route towards one's goal. This is advice I have given to many women facing similar situations. I tell them: Try it, it works.

In the interim, before the prospect of better telescopes and better skies was realized, other important changes in my life occurred.

1947–1951: MARRIAGE, L'OBSERVATOIRE DE HAUTE PROVENCE, PLANNING TOWARD YERKES

In the autumn of 1947, following C. C. L. Gregory's advice, I enrolled in some graduate-level courses at UCL. The college building had suffered such severe bomb damage that the lectures were not conducted in the old physics area, but in Foster Court, where geography had previously been taught; damage there was slight. The courses were given by David Bates, one course straight out of *The Theory of Atomic Spectra,* by Condon and Shortley, and one on the night sky emissions in the upper atmosphere, the application of quantum physics to the excitation and de-excitation of oxygen ions. The small class contained some students who had been pre-war undergraduate contemporaries of mine, and some new graduate students enrolled with Professor H. S. W. Massey. One of these, from Bristol University, sat next to me, and we became friends. His name, Geoffrey Burbidge, is familiar to all readers of this prefatory chapter. We began to talk about many aspects of physics, but also about many other topics—tennis, opera, theatre, the Cotswold countryside, politics, and history. The not-very-long delayed culmination of our friendship was marriage on April 2, 1948.

Geoffrey (hereafter in this article, Geoff) and I rented an apartment created in the Gregorys' house in Mill Hill. Geoff's thesis project was the study of the mesonic Auger effect in cosmic rays; his supervisor was Professor H. S. W. Massey. This work required the calculation of mathematical functions which can nowadays all be found in tabular form. The calculations were done on a Brunsviga hand-cranked calculating machine which we bought, and which we still possess. It played its role later when Geoff used it extensively in the work with Fowler and Hoyle on stellar nucleosynthesis (B^2FH—see later). (Willy Fowler called this machine, which he could hear cranking in the office where we sat round the corner from his in the Kellogg Radiation Laboratory at Caltech, our "Babbage machine.")

In Mill Hill, in addition to carrying on his Ph.D. thesis work, Geoff joined in the parallax and proper motion photographic work at the University of London Observatory. The walk of a mile or so between our apartment in the Gregory house to the Observatory involved some interesting encounters with the nighttime police patrols, who, demanding to know what we were doing on the streets at such ungodly hours, found our account of our work puzzling although interesting.

After our marriage in April, 1948, we applied and were accepted as members of the International Astronomical Union, and were permitted to attend the first post-war IAU General Assembly, held in Zurich in the summer of 1948. IAU meetings in that era were so much smaller than the colossal gatherings they are now, and I was able to meet and talk to Otto Struve, then President of IAU Commission 29 on stellar spectra, whose work on stellar spectroscopy was so important to me in guiding my thesis work and my future career. Dr. Struve, hearing about the difficulty I had in getting to the *better telescopes, better instruments, clear skies,* said it should be possible for me to apply for an IAU grant to go to the U.S., and to apply for the new Fulbright travel grants in order to get there.

We did not immediately follow this advice. First, it seemed important to see what we could do in Europe. As a result of contacts made during the Zurich IAU meeting, we applied for permission to travel to L'Observatoire de Haute Provence (OHP), St. Michel, France, to use the 80-cm telescope and spectrograph to continue work on the Be stars. Dr. Ch. Febrenbach gave us some weeks in the summer of 1949 on that telescope, so we applied to the Royal Society, London, for a modest amount of travel funds to get to Haute Provence. Readers will have no trouble in guessing the response from Professor R. Redman: You should not be seeking observational opportunities outside the U.K., use the telescopes we have available here; request denied.

This again activated the principle I have already described: If you meet with a blockage, find a way around it. In this case, the way around was to scrape up all of our available funds (not much!) and pay our own way to OHP.

Once there, in the summer of 1949, our eyes were opened again to the beauty of clear, dark, summer skies, and the advantage of the OHP 80-cm telescope over the Wilson reflector in London. One of the resulting publications was called *Hydrogen and Helium Line Intensities in Some Be Stars* (Burbidge & Burbidge 1951). That summer was wonderful: In addition to the nighttime, there was the OHP dormitory and friendships created there (we became especially attached to Madame Barrachino, who ran the dormitory and diner, and with whom we maintained Christmas-time correspondence for many years). I still remember her wonderful Provencal accent: hard to follow at first, with our school-days education in French. It took about two weeks before we were able, quite suddenly in my case, to take part in the friendly colloquial conversation at the dinner table with the others—Georges Courtès, the Duflot twins, Daguillon, and others whose names I do not recall after nearly half a century.

After these magical weeks in Haute Provence, we were given time at L'Institut d'Astrophysique in Paris to use the Chalonge microphotometer to make tracings of our spectra. This hospitality extended even to providing us, free of charge, a bedroom in the Institute, and initiated a lifelong friendship with Gérard de Vaucouleurs, who taught us how to use the microphotometer. The tracings were done on photographic paper, which then had to be developed in the darkroom; while I ran the microphotometer, Geoff developed the tracings. After many intensive days and nights of this work, he began wearing rubber gloves as protection from the chemicals. It was in this guise that he crept into the back of a lecture hall where a conference on Novae was in progress. Fred Hoyle was a participant in the conference; Fred, like us, was existing on a minimum amount of money, and thus we met at lunchtime in the cheapest nearby restaurant we could find. That was the café where a never-to-be-forgotten incident occurred: Fred, after the meal, looking at the check, saw that the waitress had added the amounts incorrectly, and pointed this out to her. She launched into a spate of rapid Parisian French with which our Provence experience provided no help; Fred, in his best Yorkshire accent, said "I may not understand your lingo, miss, but I *can* do arithmetic!" and pointed to the figures again; she (fairly gracefully) accepted that she had made a mistake.

Thus began our lifelong friendship with Fred Hoyle.

It was now time to follow the suggestions given to us during the IAU meeting in Zurich. Since Otto Struve was the Director of Yerkes Observatory, that seemed the obvious place to try and gain acceptance, and this idea was reinforced during a meeting in London at the Royal Astronomical Society, where we met S. Chandrasekhar, also on the University of Chicago faculty and living at Yerkes Observatory, Williams Bay. It was clear that there were no Mt. Wilson-type restrictions on women using the 40-inch refractor at Williams Bay or the 82-inch telescope at McDonald Observatory in SW Texas. Struve had created McDonald Observatory, which for many years was maintained and operated by the University of Chicago, and he had observed there frequently with various collaborators. Nancy Grace Roman was on the staff at Yerkes, and was a frequent observer at both places.

At the same time, there were Agassiz Fellowships available at Harvard College Observatory, and as we were great admirers of Director Harlow Shapley and the HCO staff, it seemed a good idea for Geoff to apply for an Agassiz

Fellowship. So this is what we did; his application was successful, mine for an IAU grant and both of us for Fulbright funds to travel to the U.S., were also successful, and I was informed that I would be welcome at Yerkes, although there was a disappointment there: Otto Struve had accepted a position at the University of California at Berkeley, so I would not be able to work with him.

During the IAU 1948 meeting in Zurich, we had also made friends with Brad and Bede Wood, at University of Pennsylvania, and—wonderful friends that they were—they said that they would meet us on our arrival on the Queen Mary at New York, to help us through immigration. They invited us to stay with them before we set off for our destination—Geoff to Cambridge, Massachusetts, and I to Williams Bay, Wisconsin.

The last year spent at Mill Hill was devoted, in addition to scientific work, to taking part in helping the rapidly recovering UCL and ULO. Professor Massey, now in charge of astrophysics, took great interest in planning the future of astrophysics and in revitalizing ULO by the addition of new buildings. Before this planning came to fruition, however, and while the planning for our venture to the U.S. was underway, other friendships brought enrichment to our lives.

Two Chinese astronomy scholars, displaced by the 1949 revolution in China, sought refuge in England, and both came to visit ULO with the request that they be permitted to work there, unpaid, just to keep their contacts in astronomy alive. They were T. Kiang, who eventually settled in Armagh, Ireland, and S.-K. Wang, who eventually returned to China to become prominent in the revitalization of China's astronomical re-awakening and repair of the ravages attributed to "the Gang of Four," and to create the radio astronomy program at Beijing University. They were welcomed at ULO.

Two more friends arrived in London—Gérard de Vaucouleurs, who had befriended us in Paris in 1949, and his wife, Antoinette, whom we had not met previously. Gérard was in London for work with the British Broadcasting Corporation on their French broadcasts, but both he and Antoinette wanted to work as much as possible at ULO. Gérard took part in the Radcliffe refractor programs, and Antoinette measured spectra obtained in the ULO laboratory and with the Wilson reflector. At that time, I had invested in an easy-to-ride motorbike (top speed: 45 mph, with the starter on the handlebars, and, instead of the usual pedals, it had flat paddles for one's feet). It also had a pillion seat. Gérard used to sit on that pillion while I drove him to the apartment he and Antoinette rented, after a night's observing. There exists a photograph of myself on that bike, C. C. L. Gregory on his massive powerful motor bike, and Anne Carew Robinson, who was hired at ULO during this period.

In 1951, it came time to say *au revoir* to ULO, and set sail for the U.S. and the observatories of Yerkes, McDonald, and Harvard College.

YERKES, McDONALD, AND HARVARD COLLEGE OBSERVATORIES

From the moment I got on the train from New York to Chicago, it was like entering a new world—literally, a time of expanding horizons in all directions, physically, mentally, and spiritually. I cannot remember the journey from Chicago to Williams Bay, but I recollect well the welcome at Yerkes. The Van Biesbroeck family—Georges, his wife and his sister, Marguerite (the librarian at Yerkes)—had a large house on the Yerkes grounds which they ran as a boarding house for students, postdocs, and visitors. Mrs. Van B. ran the house, kitchen, and dining room; all three of them, I believe, worked in the vegetable garden across the road where, European-style, they grew many of the vegetables that we ate in the diner.

At those meals, I met many who have remained friends or of whom I have interesting memories. These included Eberhart Jensen, from Norway, Nancy Grace Roman, Harold Johnson, who was working with Bill Morgan on the famous stellar classification program, and graduate students Don Osterbrock, N. Limber, and Larry Helfer. My grant from the IAU, $1,000, covered board and lodging at the Van Biesbroecks; Geoff's Agassiz Fellowship, $1,600, covered a rented room in the household of a Harvard professor living near the Harvard College Observatory; we had some savings, and planned as soon as possible to try for observing time at McDonald. I started working with Professors W. W. Morgan and W. A. Hiltner. Bill Morgan started me on his observing program with the 40-inch refractor, with which I was privileged to spend nights taking spectra of B stars for Morgan's program of mapping the nearby spiral structure in our Galaxy. How well I remember the meticulous instructions for developing the spectra—metol sulfite developer for the *exact* time, then, after the fixing in hypo, the plates were *not* to be placed in running water for 30 minutes, the usual procedure with which I had been brought up, but should be placed in six successive dishes of water for 5 minutes each. There were other details to this procedure, and Bill Morgan and his assistant, Irene Hansen (later, Irene Osterbrock) would be likely to detect if one had deviated in any way from the instructions. The work under Morgan on spiral structure in our Galaxy initiated my interest in spiral structure in galaxies in general.

Before the cold Yerkes winter set in, Geoff and I prepared a program to submit for McDonald observing time with the Cassegrain spectrograph of Struve/Elvey fame. But the time for submission was past; since we wanted winter time when the December Milky Way was up, we were too late. Here the never-to-be-forgotten kindness of Al Hiltner came to our rescue. He had set me to work on prevention of internal reflections and scattered light in a spectrometer for calibrating coudé plates at McDonald, and he had a month (I believe) scheduled for photometry at McDonald. He said there would be many nonphotometric nights during this period, and if Geoff and I could get ourselves to McDonald with the small stipend allowed to graduate students and postdocs for travel, we could have the nonphotometric nights for spectroscopy, since the Cassegrain photometer and spectrograph were easily interchangeable.

Soon after my arrival at Yerkes, the gorgeous fall colors spread over the woods around Yerkes. Bill Morgan organized what was apparently an annual event—a walk some way along the path around Lake Geneva—a right of way since Indian times. Cameras were encouraged, and later in the fall Bill and Irene organized the annual photo exhibition in the library, for which all entrants must have taken, developed, printed, and mounted their pictures, for the competition. My photos, very much also-rans, were duly displayed; they were mostly pictures of birds, water pools, waves, and rocks taken in the Scilly Isles the summer before we left England. I do not remember who the prize-winners were, but the event is one of my many happy Yerkes memories.

December came, Geoff traveled from Cambridge, Massachusetts to Chicago (courtesy Mr. Fulbright) and we set off for Pecos, Texas by train coach plus Greyhound bus. We were welcomed by the McDonald secretary at Pecos (mistakenly labeled Pecan by me in my ignorance of US names!) and were driven 90 miles to McDonald and installed in one of the small visitors' cottages.

At McDonald, we made another friend, the Superintendent, Marlyn Krebs, son of the Yerkes Superintendent. He, with, I suppose, Al Hiltner, instructed us in the use of the spectrograph, darkrooms, photographic supplies, and the protocol about whether any particular night was suitable or not for Hiltner's photometry.

I shall never forget the excitement of using that 82-inch telescope. Compared with the Yerkes winter nights, when one only ended observing on a clear night when the temperature dropped below −5°F, the McDonald nights in the dome were usually quite comfortable on the Cassegrain floor, even when a Texas "blue norther" had blown in.

After the observing run, it was time for Geoff to return to Harvard, and this time I accompanied him. I looked forward to measuring the spectra we had obtained, but Harvard College Observatory had no measuring machine. When I asked Dr. Harlow Shapley, who was welcoming and kind as always to overseas visitors, about a measuring machine, he said he was intending to acquire one—it was much needed at HCO. Years later, Shapley teased me about my persistence on this topic, and reminded me of a conversation one day in his office, when he had just received news that King George VI of England had died and had been succeeded by Queen Elizabeth II. He had greeted me with the words "Well, you now have a new Queen in England!" He recalled my response as being some moments of surprised silence, and then: "Now, what about that measuring machine?"

Geoff was attending lectures by Bart Bok on galactic structure and stellar distribution functions; I was learning all I could from the work of Cecilia Payne

Gaposchkin on stellar element abundances and on variable stars. HCO was a wonderful place for widening horizons. Among the friends we made there were Arne and Ingrid Wyller, who were planning to drive with Ingrid's mother from Cambridge across the country and into Canada for the 1952 summer meeting of the American Astronomical Society (AAS) in Victoria, B.C. They were looking for a fourth passenger to share expenses on the three-week camping trip, and invited either Geoff or me. We tossed a coin, and I won. Our route went through Yellowstone Park, where several days of sightseeing renewed my earlier undergraduate interest in geology.

We reached Victoria—again, new science and new friends. By the good luck I was encountering everywhere, Harlan and Joan Smith, whom we had got to know at Harvard and who had driven out for the Victoria meeting, were looking for someone to share their drive (again, camping) down the coast into California to visit Lick, Mt. Wilson, and Palomar Observatories.

After this, it was time to return to Cambridge, Massachusetts, and the only affordable transportation was by Greyhound Bus. Not many of today's postdocs or graduate students have needed to make that arduous although fascinating five-day journey (I stopped one night in a YWCA in St. Louis to rest my weary bones); the experience with its encounters, both friendly and adversarial, would be a story in itself. I remember well my relief at being met by Geoff around midnight at the Cambridge bus station.

For the next year, Yerkes Observatory offered both Geoff and me postdoc positions, and we started work there again. With the Yerkes facilities, one could tackle our McDonald spectra of Be stars, measuring wavelengths and line intensities. Chandra's book on Radiative Transfer gave us theoretical background to tackle the physics of the outer atmospheres of these stars, including the effects of rotation, electron scattering, and radiative transfer in the hydrogen emission lines. We measured the Balmer decrement in the emission lines to be slower than the standard "case B" planetary nebulae decrement, and we calculated the effect of radiative transfer in the Balmer emission lines and were able to explain the Balmer decrement in the emission lines in Be stars. We calculated absorption line profiles for various rotational speeds and fitted to the observations. Several papers were published (e.g. Burbidge & Burbidge 1953 a,b).

Meanwhile, I worked for Bill Morgan on observing B star spectra at Yerkes for his work on spiral structure in our Galaxy. Geoff started working with Chandra, who was at that time engaged in his magnum opus on magnetohydrodynamics. In common with the rest of the astronomical community, we both found Chandra's lectures to set a precedent for excellence; each topic, of course, appeared as a classic monograph. Geoff started work on the magnetic stability of stars, and this got me interested in the Ap stars with strong and variable magnetic fields. We both, through Chandra, became interested in white dwarfs and in stars with apparent deficiencies in their metal/hydrogen surface abundances.

Two landmarks stand out. First, an application for observing time at McDonald with the coudé spectrograph in the winter of 1952–1953 to continue work on Be and related stars was denied because the Milky Way time was over-subscribed, but we were given time in the spring of 1953, and thus needed to plan a different observational program on stars of higher galactic latitude. With interests aroused through Geoff's contact with Chandra and Fermi on the role of magnetic fields in astronomy, we selected a list of Apm stars, the most important being α^2CVn. Again, the influence of Struve played a role; he had published an enormous wavelength and identification list of absorption lines in α^2CVn. We planned to determine atmospheric abundances, in particular of those elements abnormally strong (Eu, Si, etc).

The second important event, in the spring of 1953, was a conference on "The Origin of the Elements," hosted at Yerkes by Gérard Kuiper, and organized by Maria Mayer and Harold Urey; a short account of which we wrote up for *Observatory* (Burbidge & Burbidge 1953). We had thought only subliminally about the question of the origin of the chemical elements in the solar system (meteorites, the Sun) and in those few stars on which abundance analyses had been carried out. How did the so-called cosmic abundances come about? Why were some stars apparently different (including the Apm stars, the Ba II stars and S stars, the apparently metal-deficient stars, and white dwarfs)? At the conference, George Gamow was in great form with his ylem theory, with jokes about his leaps past the difficulties at $A = 5$ and 8, and Maria Mayer was describing the Mayer-Teller hypothesis, of a primordial polyneutron origin to explain the neutron-rich isotopes (Mayer & Teller 1949). A bell was rung in my mind—the fascinating RAS talk by Fred Hoyle (Hoyle 1946) on physical processes for building the elements in the interiors of stars at late evolutionary stages, when exhaustion of hydrogen as a fuel for nuclear reactions, followed by internal contraction and external expansion in an inhomogeneous structure explaining the red giants, would be followed by further internal collapse to very high T,ρ conditions sufficient to set up equilibrium among the nuclei and produce most of the more abundant elements. How much of Fred Hoyle's work was ahead of its time, ignored by the stolid, unimaginative, mentally constipated run-of-the-mill astronomers!

The McDonald observing run was successful; our spectra had to be microphotometered in the Yerkes basement before we left Yerkes. Our return journey to England, at the expiration of our two-year visitor visas, was by way of Ann Arbor, Michigan, for the famous 1953 summer school at which George Gamow, Walter Baade, Edwin Salpeter, and George Batchelor were lecturing. Geoff and I were housed in an apartment of a U. Michigan professor, and Allan Sandage (with whom our lasting friendship began during this summer school) was staying with other single postdocs and graduate students in a dormitory where they were educated and entertained by nightly bull sessions with Walter

Baade. The lectures by Gamow, in which he expounded his ylem theory, crystallized our own ideas that the elements were not formed in some primordial series of events at the origin of the universe, but were built up out of hydrogen in successive generations of evolving stars.

For the next year, we had two job offers: first from Professor Z. Kopal, who offered us two junior faculty positions at U. Manchester, and second, one position, for Geoff only, with Martin Ryle's radio astronomy group in Cambridge. Geoff, following his interactions with Chandra and with Fermi in Chicago, was very interested in the strong radio sources, about which there was currently much discussion as to the physical mechanism producing the radiation. I had all the McDonald spectra to analyze, and Cambridge seemed to be an ideal place in which to tackle them.

CAMBRIDGE, 1953–55: SYNTHESIS OF THE ELEMENTS IN STARS

We found an apartment in Cambridge, 8A Botolph Lane, just around the corner from Free School Lane, where the old Cavendish Laboratory housed the radio astronomy group. With permission from Prof. Redman (and payment of a "bench fee" to Cambridge University), I was able to use the measuring equipment at the Observatories in Madingley Road. Wavelength reductions on our spectra were done with our Brunsviga, and I measured equivalent widths on the yards of Yerkes microphotometer paper with a planimeter. Our apartment rental did not include permission to keep a bicycle in the narrow access passage, so the daily walk behind Trinity and Johns Colleges and along Grange and Madingley Roads to the Observatories provided time to think, plan, and contemplate.

Geoff was not having a particularly easy time at the Cavendish. As a theoretician, thought to be contaminated by ideas inspired by Fred Hoyle, and as a proponent of the synchrotron mechanism for producing radio emission from the strong extragalactic sources (the Ryle group at that time favored plasma oscillations), the Ryle group did not willingly share their observational work on counts of radio sources with him.

It was in the autumn of 1954 that we made the acquaintance of Willy Fowler and his family—he was spending a sabbatical year at the Cavendish, where he had hoped to do some experimental nuclear physics, but had found that none of the equipment he needed was available or working. At this time, element abundances were emerging from our curve-of-growth analysis of α^2CVn (Burbidge & Burbidge 1955), and the heavy element anomalies were beginning to suggest that somehow neutrons were involved—an idea whose germ had been planted by the Mayer-Teller and Gamow work, but we believed the processes must take place in stars. While we were well-educated in atomic physics, we knew much less about nuclear physics. Geoff attended a $\Delta^2 V$ lecture by Fowler, and asked him afterwards if we could talk about processes involving neutrons in stars. Fowler, a leader in the experimental low-energy nuclear physics program at Kellogg on the light elements, had recently worked with Salpeter and Hoyle while they were visiting Caltech. He was excited by the prospect of adding neutron processes towards a theory to build all the elements in their cosmic abundances through generations of stars—which through evolution, finally produce Hoyle's iron peak elements (Hoyle 1954), and end as supernovae, exploding and enriching the interstellar medium with heavy elements made from the initial ingredient, hydrogen. Fred Hoyle was in Cambridge and we four worked together during that exciting 1954–1955 year, adding together one piece after another of the puzzle. Willy's wife, Ardy Fowler, in her wonderful hospitable way, made available their rented home in Cambridge and we four divided time between there, the Cavendish, and Botolph Lane until the time came when Geoff and I had to think about jobs for next year. Knowing all too well of the ban on women for Carnegie Fellowships, Willy thought Geoff had a good chance for one, and thought he could give me a postdoc fellowship in the Kellogg Radiation Laboratory at Caltech. "This time," he enjoined us, "you must apply for regular immigration visas, not visiting two-year Fulbright visas." So we did.

One more good memory of Cambridge remains to be recalled. I reviewed, for the radioastronomy journal club, a paper by M. Schwarzschild (1954) on masses and the mass-to-light ratio in galaxies, about which little was then known. This was, to me, a new interest, related both to stellar evolution and, more strongly, to Geoff's work on radio galaxies. I thought of an observing program on measuring rotation curves of spiral galaxies to determine their masses and M/L ratios. Geoff was awarded a Carnegie Fellowship, but it was made clear that work with the 100-inch Hooker telescope on Mt. Wilson on galaxies, which had been carried on by Milton Humason in collaboration with Edwin Hubble, was no longer possible; the night sky was too bright. Humason's multi-night exposures, as he sat at the Cassegrain focus of the 100-inch, would have been impossible; all such work in the future should be carried out at Palomar. However, we set off for Pasadena while the work on B^2FH (Burbidge, Burbidge, Fowler, & Hoyle 1957) was only partly completed, with the primary goal to spend most of the next two years on that major project.

CALTECH, MT. WILSON, B^2FH

In Pasadena, we renewed the friendship begun in Ann Arbor with Allan Sandage, and he began a Chinese water-drip campaign on Dr. I. S. Bowen, Director of Mt. Wilson and Palomar Observatories, to consider Geoff's Carnegie Fellowship observing proposals as coming jointly from both Geoff and me, and to allow me to go up Mt. Wilson with Geoff. To stay in the Monastery was, of course, impossible, as was use of Observatory transport up and down Mt. Wilson. Standard reasons for not allowing women on the telescope included the fact that there were only male-oriented bathroom facilities on the mountain, and that the telescope technicians (then called night assistants) would object to operating under directions from a woman. However, Dr. Bowen relented, under pressure from Caltech as well as from Allan Sandage, and we were allowed to go together and stay in the Kapteyn Cottage (a small summer cottage) on the mountain, as long as we brought our own food and used our own transport—an old, but serviceable, Chevrolet. The night assistant "problem" was solved when Arnie Ratzlaff, at the 60-inch, asked us one night why we did not come to the galley for midnight lunch, but brought our own sandwiches. On being told that we did this on Director's orders, Arnie simply laughed and said that was nonsense. So from then on, we joined in the communal night lunch in the galley.

Down in Pasadena, Geoff and I were working hard on B^2FH, dividing time between 813 Santa Barbara Street (his scientific abode) and Kellogg (mine). The eight processes were being worked on: H-burning, He-burning, α-process, the e-process (Hoyle's process), the s-process (slow neutron capture for interiors of S-type red giants), and the r-process (rapid neutron capture in supernovae to produce the displaced peaks alongside the s-process peaks at Maria Mayer's "magic numbers" of closed nucleon shells); there were also the p-process, to account for some low-abundance heavy isotopes, and what we called the x-process, for deuterium, Li, Be, and B. Red giants of type S, we felt, had atmospheres that were too complicated to tackle for abundance determination and the α^2CVn abundance anomalies, which had seemed so promising originally, did not produce the right heavy-element overabundances. At this point, we needed good spectra of a Ba II star, where the enhanced spectral lines were of just those elements in the s-process chain. A problem arose; the brightest Ba II star, ζ Cap, was in Prof. Jesse Greenstein's observational programs, which covered a wide range of forefront problems in stellar spectroscopy (Greenstein 1984), although its analysis had not yet been undertaken. So we chose HD 46407, the next brightest accessible star from the list in Bidelman and Keenan's important paper (1951); in that paper they concluded that the strength of Ba II λ 4554 in these and in S-type and carbon stars was probably not a consequence of ionization and excitation processes.

Our Mt. Wilson observing program also included various low-metal-abundance stars, and some T Tauri stars; these were linked to the complete picture of stellar evolution which was emerging at that time. We worked on this program throughout the winter of 1955–1956, with Geoff doing the plate-cutting and other darkroom work while I worked at the telescope, at the cassegrain focus using the movable ladder in the 60-inch dome, and at the 100-inch, mostly at the coudé focus. Geoff's expertise at plate-cutting with a diamond (this was before Bill Miller's ruling machines were designed) had been learned in the workshops of his father's building business in Chipping Norton, England.

By April 1956, I was beginning to be unable to disguise pregnancy under the loose layers of warm clothing one used to wear for winter observing out in cold domes, and there came a time when climbing up the movable ladder and heaving it around the dome became more than I could manage. So we terminated our observational program, and concentrated on analyzing the spectra. Meanwhile, the work on B^2FH was proceeding marvelously.

For the light elements, the experimental work organized at Kellogg by Fowler and the Lauritsens was crucial, while some more recent nuclear data on energy levels in iron-peak elements produced an e-process curve slightly improved over the original Hoyle calculation. For the neutron-rich heavy elements, our data on HD 46407 (Burbidge & Burbidge 1957) fitted beautifully with the s-process prediction that the product of neutron-capture cross section times abundance, averaged over the s-chain isotopes, would be constant, and, within a factor ~ 2, for 11 out of 15 elements studied, this proved to be the case, even though measurements of the neutron-capture cross-sections at that time were not particularly accurate and, of course, received subsequent improvement. Geoff had noted that the light curve by Baade of the SN in IC 4182 was linear, indicating the sort of decay produced by radioactive elements. Radioactivity proved correct, but we chose the wrong radioactive element, Cf, instead of $Ni \rightarrow Co \rightarrow Fe$, as later proved correct.

Several papers were written but the principal one, always thereafter known as B^2FH, was a 100-page account of all eight processes (Burbidge et al 1957).

REACHING THE GALAXIES—YERKES AND McDONALD OBSERVATORIES AGAIN

Towards the end of Geoff's second year of the Carnegie Fellowship, and of my research fellowship at Caltech, and after the birth of our daughter, we were offered positions in the University of Chicago's astronomy department, i.e. at Yerkes Observatory. Chicago at that time had a "nepotism rule," so that husband and wife could not both be on the faculty. This applied however distinguished and senior the couple; for example, Joseph Mayer had a professorship and therefore Maria Mayer, future Nobel Laureate, could not be on the Chicago faculty and so was employed by the Argonne Laboratory—her teaching and research at the University thus was "voluntary." So Geoff was offered a faculty position, while a Shirley Farr Fellowship was offered to me. We accepted gladly.

Back at Yerkes, we once again had access to McDonald Observatory, and could set about realizing the goal that originated from our interests in Cambridge. The McDonald 82-inch had a prime focus capability; since the telescope diameter was not large enough to accommodate a cage, access to that focus was obtained from two electrically operated "pulpits," off a bridge which rose up and down within the dome slit. Each pulpit could be manually cranked up, so that with suitable manipulations one could reach in and insert plate holders or other instrumentation directly at the prime focus; telescope guiding was then done from an eyepiece and a paddle on a post which could be moved electrically around the rim of the telescope. At first, the experience up there produced a slightly dizzy sensation, as one looked down upon the primary mirror and felt the awesome responsibility of knowing that it was as much as one's life was worth to fumble and drop anything down onto the mirror.

Horace Babcock, years earlier, had designed and constructed a spectrograph that could be placed right at the prime focus—the "B Spectrograph," with small plateholders that pushed into the side. Direct photography was done by sliding the spectrograph over, leaving the place for standard 5×7 inch plateholders to be inserted.

The B spectrograph had been used extensively by Thornton Page for his classical work (Page 1975) on velocities of binary galaxies and their statistical mass determinations, but it had fallen into disuse; the only current prime focus user was Georges Van Biesbroeck, who studied comets, asteroids, and proper motion stars. Our first McDonald observing run after arriving at Yerkes renewed our friendship with Marlyn Krebs; he disinterred the B spectrograph from some dusty storage region, and the three of us cleaned it, remounted the collimator, adjusted the whole optical train, and then set to work using it on a program we had prepared on spiral galaxies. The spectrograph was designed to work with 103a-F film, in the red, using the Hα and [NII] emission lines from ionized gas in the galaxies. The spectrograph slit was long enough that one could obtain velocities along half the major axis of these galaxies, or across them at various angles, in single exposures—this proved enormously more economical of observing time than the earlier work of Babcock, and of Mayall and Aller, on velocities of individual H II regions in M31 and M33.

Essential to our operation was the help of the young telescope operator Johnny Carrasco, as he was then called; now he is the well-known and heavily acknowledged chief telescope operator Juan Carrasco, of Palomar fame. As before, Geoff was in charge of photographic operations. The dexterity he had used in plate-cutting at Mt. Wilson was now essential in cutting the half-inch squares of photographic film by a cutting punch which deposited the tiny square into a drawer, *but*—it could fall film-up or film-down, and it is much harder to tell by feel in the dark which is the emulsion side of photographic film than is the case with plates. Only the delicate touch of Geoff's fingertips could do this unerringly. It was an essential part of our operation—imagine spending three, or four, or even five hours standing, exposed to wind and cold in the pulpit of the bridge, eye glued to the eyepiece, with those precious few photons falling only on the back side of the photographic film!

We have, of course, an ample store of anecdotes about our McDonald days. The two most traumatic were the occasion when the motor that moved the post around the telescope rim jammed and trapped Geoff's arm, nearly breaking it; with great presence of mind he yelled "cut the power," and Johnny and I were able to extract his bruised arm. The second occasion was during an attempt to take a spectrum of NGC 5128 at the cassegrain focus. At that time, NGC 5128 had sometimes been hypothesized to be a planetary nebula, but as a strong radio source, Cen A, it was obviously a galaxy; at declination $-43°$, it was just accessible from McDonald. To reach it, we had the cassegrain floor raised right up; I had removed the safety chains guarding the inner edges of the two halves of the cassegrain floor, and, in my excitement over setting on this far-southerly faint object, I stepped over the edge of the floor and fell some 10 feet down to the main floor, carrying the control paddle with me. Completely winded, but with no broken bones (thanks to the padding of heavy winter clothing), I could not draw breath to tell Johnny and Geoff that I was alive and conscious; I could hear them fumbling their way down the steps in the dark, dreading what they might find at the bottom. Fortunately, bruises, a cut forehead, and a multitude of aching parts were all I earned, and a spoilt night's observing and a day in bed were all the payment exacted. Not surprisingly, we carried out our spectroscopic work on NGC 5128 at the prime focus, where one could lie on the roof of the coudé room to reach the guiding eyepiece.

Such were the hazards of observing "in the old days." Other astronomers fared worse; Joy fell off the Mt. Wilson high Cassegrain platform and suffered numerous fractured bones. The eagerness of the most active and excited astronomers can still lead to peril, as was sadly demonstrated recently at Kitt Peak; we all mourn Aaronson and know that "there, but for the grace . . . " might we have been.

Near the start of our work on galaxies, I took a picture of M51 to try out the direct imaging capability at the 82-inch prime focus. Seeing that image on the 5×7 inch photographic plate in the washing dish in the darkroom produced such euphoria that I felt it was almost sinful to be enjoying astronomy so much, now that it was my job and the source of my livelihood.

The long-slit spectra, with the inclined and curved emission lines of Hα and [NII], now so familiar to all extragalactic astronomers, were new to us, and needed to be measured with a two-coordinate screw machine. The rotational velocities thus obtained were the raw material for obtaining the mass distribution throughout these spiral galaxies. Thus began a wonderful collaboration and friendship with Kevin Prendergast. At each point measured, the rotational velocity is derived from the gravitational balance with the integrated mass interior to that point, thus one needs a model for which the necessary integral equation can be set up. Earlier models had led to equations that were awkward to solve; Kevin's expertise in theoretical analysis and mathematics provided the model we three used—an integral converted to a sum of similar spheroids which could be solved analytically quite easily. Thus followed a series of papers, summarized in a chapter in the Sandage volume on galaxies (Burbidge & Burbidge 1975).

The mass distributions that we obtained ended necessarily at the last measurable point on our spectra. We extrapolated the rotation curves further out, assuming a Keplerian fall-off in velocity, and derived mass-to-light (M/L) ratios interior to our last observed points, and assumed these applied to the whole galaxies. However, some 20 years earlier, Horace W. Babcock had made some remarkable measurements on the outer parts of the Andromeda galaxy, M31 (Babcock 1939). He had built an attachment to the spectrograph on the Crossley reflector at Lick Observatory, which enabled him to observe the absorption lines in M31 out to 95 arc minutes either side of the nucleus. He discovered that the rotation curve was still rising, out to his furthest measured point.

While he realized that his absorption-line measurements were subject to considerable probable errors, he noted that the measures, if valid, implied that the M/L ratio in M31 rose from some 1.6 within 30 arc sec of the center, to 18 at 15 arc min out, 43 at 50 arc min out, and 62 at 80 arc min out. This appears to be the first observation that required the existence of considerable dark, or unseen, mass in the outer parts of a normal spiral galaxy.

This remarkable observation appears to have escaped notice until the increasing precision of 21-cm radio observations, which demonstrated horizontal rotation curves beyond the last measurable optical points. Optical measurements of the ionized gas component—Hα and [NII] emission lines—required the 4-m Kitt Peak telescope, as opposed to the 82-inch telescope, plus a much more sophisticated spectrograph, and the skill and determination of Vera Rubin, to show that ionized hydrogen extended beyond our last measurable points, and that the extended rotation curves were *flat.* This, with the 21-cm data, brought into the collective mind of the astronomical community the postulate of dark, unseen matter extending far beyond the obvious visible luminous extent of spiral galaxies. But by then we had moved on to a new and still-abiding interest: the quasi-stellar radio sources—QSRS, QSOs, quasars.

The first identifications of these, by Matthews & Sandage (1963) produced one object, 3C 48, just barely observable at the 82-inch prime focus. We duly and with some difficulty in guiding (since no offset guiding could be done at the 82-inch prime focus) obtained a spectrum. Some possible fuzzy emission features could be seen in the red, but they were barely visible and did not look like any stellar or extragalactic features with which we were familiar, so we did nothing with them. As I described in my 1984 Russell lecture, 20-20 hindsight and careful printing of these spectra in 1983 showed quite well that they were Hβ, [O III] 5007, 4959, but again in hindsight, how could we have convinced anybody at that time that we were looking at a faint stellar object with a large redshift?

During the years 1957–1962 that we spent at Yerkes, mainly working on galaxies, we also continued the collaboration with Fowler and Hoyle, by spending part of the summers in Pasadena. Much of my time there was spent at Santa Barbara Street, using Rudolph Minkowski's 2-coordinate measuring machine on our galaxy spectra. We spent wonderful hours, both social and scientific, with Walter Baade, R. Minkowski, M. Humason, and Allan Sandage. Our good friend Henrietta Swope, who had befriended us earlier in the two years we spent in Pasadena, and who played the role of virtual godmother to our daughter Sarah, preceding, during, and after her birth, was now as always giving us a warm welcome in Pasadena.

Another important friendship developed. Allan Sandage had introduced us to Mrs. Hubble—Grace, widow of Edwin Hubble—during our previous two years (1955–1957) that we had spent in Pasadena. Now, in the years after 1957, every summer during which we spent time in Pasadena, we used to visit Grace and have tea with her in her beautiful home in San Marino. She told us much about Edwin that, I understand, is now guarded in the archives of the Huntingdon Library. Because of his worldwide scientific prominence, Edwin had expected to be appointed Director of the combined Mt. Wilson and Palomar Observatories, so the choice of I. S. Bowen for this position had come as something of a shock, although Ike Bowen's expertise with optical instruments had made him a suitable candidate for the major responsibility of commissioning the Palomar 5-m telescope, which had been mothballed during World War II.

Grace told us that Edwin had died very suddenly after driving home, while he was getting out of the car; as he lay in the driveway Grace thought he had just fainted and did not accept that he had died until doctors convinced her of the bad news. During our visits with Grace, she told us of the work she was doing at the Huntingdon Library on his papers. She showed us his study on the ground floor of her house, which she had kept exactly as he had left it. Grace was a remarkable woman, an intellectual giant, who had many friends in the literary and artistic world. She told us of her and Edwin's friendship with George Arliss and others famous in the theatrical world, and we met Aldous Huxley and Gerald Heard over tea at her house. She had given us many items that we could not otherwise have afforded, before and after the birth of our daughter Sarah who, as a small child, loved the teatime visits with "Aunt Grace" during our later summer visits. Grace who had had no children of her own was extremely kind to Sarah and enjoyed her visits (this is more than can be said of the Hubble 16-year-old black cat, Nicholas, who always retreated into the cellar when Sarah appeared).

Of the many stories Grace told us about Edwin, one demonstrates a side few are aware of. Apparently, during one dinner party, a conversation developed over the relative housekeeping merits of men and women, the hostess maintaining that on the whole men had no ability in this field—for example, consider how bad they were at dusting. Edwin stood up, went over to the door, and, being so tall, reached up and ran his finger over the top of the door, then showed his dust-covered finger to the assembled company. "It was very naughty of him, but she was being provoking," remarked Grace.

THE BEGINNING OF THE YEARS AT THE UNIVERSITY OF CALIFORNIA

After the publication of B^2FH, all four of us received many invitations to give general or specific colloquia and lectures on "The Synthesis of the Elements." One such visit, at the invitation of Roger Revelle, was to the Scripps Institution of Oceanography. Harmon Craig, whom we had first met at the 1952 Yerkes meeting when he was a student of Harold Urey, contributed, with his wife Valerie, to the warm welcome we received at Scripps. Roger Revelle was at the beginning of his creation of the new University of California campus at La Jolla. We were, as I have described, deeply involved in the physics of galaxies, and during the summer visits to Pasadena, Geoff was maintaining the contacts with Fred Hoyle and Willy Fowler.

Having eventually made his point with the radio astronomers in Cambridge that radio galaxies were powered by synchrotron emission, as first published by theoretician Shklovsky, Geoff had next turned to the problem of the energies needed to produce such huge fluxes of radio radiation. Man-made synchrotrons, after all, require far more input energy then the resulting output of high-energy particles; Geoff's calculations (Burbidge 1956, 1959a,b) of the minimum energies in high-energy electrons and magnetic fields in a number of radio galaxies, including M87 and Cyg A, assumed equipartition between field and particles, but he also considered the greater energy requirements if accelerated protons were produced as well as electrons. Time scales were also important; the directivity of the radio jets, as well as the optical and radio jet in M87, clearly demonstrated that the source of the energy was located in the very nuclei of the galaxies, and the radiating particles, with lifetimes dependent on the frequency of the radiation, required continued energy replenishment and a mechanism of ejection. The summertime interactions with Hoyle and Fowler were spent in tackling this problem, and our summer visits to Cambridge began. After looking at multiple supernovae in compact nuclei and quasi-stellar radio sources as ways of producing the observed properties, Fowler, Hoyle, and Geoff settled on the collapse of matter towards the Schwarzschild radius, and a major paper by the four B^2FH authors was written which really initiated the subsequent activity by many in relativistic astrophysics (Hoyle et al 1964). We also collaborated with Sandage, and published a paper on "Evidence for Violent Events in the Nuclei of Galaxies" (Burbidge et al 1963).

Meanwhile, at Yerkes our observational work on galaxies continued, and we became interested in small groups and clusters, and the masses required to hold them together for a Hubble time. A great surprise showed up from our observation of the radial velocity of the fifth member of Stephan's Quintet, a distorted spiral galaxy, whose velocity differed by some 5500 km s^{-1} from the others. I well remember my surprise on taking the McDonald spectrogram out of the washing dish in the darkroom, peering at it with the magnifying glass, and seeing a clear, extended, Hα emission line which could be at no more than $+1000$ km s^{-1} from rest wavelength. This, with other close groups picked out of Vorontosov Velyaminov's catalog, initiated our interest in discordant redshifts—a subject still unresolved after 30 years.

One of our McDonald observing runs was shared with Nick Mayall, who was working with Bill Morgan on the spectral classification of galaxies. Nick's criticisms of the prime focus "B" spectrograph, especially its lack of an offset guider, and his description of the new 120-inch telescope at Lick Observatory, put a spark to a fuse which had been lying ready for ignition. We had been involved with the political troubles at Yerkes, concerned with Gérard Kuiper's wish to start an infrared program that involved negotiations with the University of Texas and culminated in that institutions's taking over control of McDonald Observatory from Chicago. At the invitation of Nick Mayall, Geoff and I took a quarter's leave from Chicago, and spent it on Mt. Hamilton. The 120-inch was newly commissioned; there was as yet no prime focus spectrograph (it was being designed by Albert Whitford and Nick Mayall and was being constructed in the Lick shops; it bore a remarkable similarity to the B spectrograph although with vastly better capability, e.g. choice of gratings, two cameras, offset guiding), and the 120-inch was large enough to accommodate a cage inside the upper end. This cage had an elliptical shape, rather than circular as at Palomar, and was built so as just to fit around a normal-sized person. Stan Vasilevskis initiated me into the equipment for direct photography at the prime focus; this was all that one could do until the spectrograph was completed; again, everything was easier than it had been at the 82-inch prime focus.

To ride with the telescope in that cage was an experience I wish I could share with today's generation of young astronomers. A canvas hood over the top could be zipped up if it was cold and windy, but otherwise one could look out at the spectacular vision of the heavens during a long spectrographic exposure (for direct photography, of course, one's eye was firmly riveted to the offset guiding eyepiece). But spectroscopy was for the future: Our spring quarter visit on Mt. Hamilton ended, and we returned to Yerkes, but were filled with the ambition to move from the University of Chicago to the University of California. Our acquaintance with Roger Revelle, the prestige associated with our part in "The Synthesis of the Elements" and recommendations provided by Willy Fowler, Harold Urey, and Maria Mayer culminated in the offer of positions at the newly formed San Diego campus. (The Mayers had been recruited at La Jolla; she was no longer an "associate" of Joe Mayer, since U. California nepotism rules did not forbid a husband and wife from both having faculty positions, as long as they were in separate departments.) We accepted gladly, although it was a wrench to leave our friends at Yerkes—Chandra, Bill Morgan, the Hiltners, the Chamberlains. Van Biesbroeck had been denied any further observing privileges at McDonald so, at "80 years young," he left Yerkes to spend ten more years at his beloved astronomy in the University of Arizona, on the new 90-inch Steward Observatory telescope on Kitt Peak.

So the next phase of our lives began.

LA JOLLA; NEW DIRECTIONS; THE CHALLENGE OF THE QSOs

The quasars, or QSOs as I shall henceforth call them, were an observational challenge that could be tackled with the new telescope and prime focus spectrograph at Lick, as they were being tackled with the 2.1-m telescope on Kitt Peak by Roger Lynds. We quickly developed a friendship with the Lynds family, and Roger and I delighted in sharing the excitement of spectroscopy and measurement of redshifts of these enigmatic objects. Once Cyril Hazard and the radio astronomers in Australia had produced the accurate position of 3C 273 from lunar occultations, and had identified it with a 13th magnitude stellar object, the riddle of the spectra of the handful of other stellar radio sources identified by Matthews and Sandage was solved by Maarten Schmidt, to whom Hazard provided the accurate position of 3C 273. The story of Maarten's identification of the Balmer emission lines in 3C 273, and his use of Osterbrock's table of ultraviolet emission lines predicted for planetary nebulae, to identify the emission lines in the existing handful of other identified 3C objects and—to the astonishment of the scientific world—to identify lines in 3C 9 at a redshift $z > 2$, is too well known to repeat here.

These large redshifts, interpreted by Hubble's law as representing the distances of these objects, immediately posed theoretical problems because, to be visible at such enormous distances, and indeed to be strong radio emitters, the QSOs must be emitting an enormous energy output. Variability discovered in 3C 273 set stringent limits on the size of the emitting region, and this combination provided a challenge to the theorists. Geoff, following his theoretical work on the radio galaxies, saw at once that if the redshifts were *not* due to the expansion of the Universe, the objects could be closer and the energy problem could be alleviated. This idea is described in the first monograph on the QSOs, which we wrote and published in 1967, and in subsequent papers.

I am continually surprised by the almost religious fervor with which most astronomers demand a single "Big Bang" act of creation for the Universe.

To question that observing very large redshifts implies observing the universe quite near its origin, (i.e. to question the notion of a singular origin of all that comprises our presently observed universe) has, during the past 30 years, been treated as heresy, and the punishment meted out to doubters and questioners and to those who find observational data that conflict with the standard big bang picture, while it does not involve physical torture and death as during past centuries, is severe indeed—deprivation of the opportunity to carry on one's research, either by denial of access to telescopes or by denial of funds to carry out research. The notion of any form of "steady state" universe was, from 1948 onwards, when Hoyle, Bondi, and Gold first proposed it, an anathema, and young aspiring astronomers continue to fear for their livelihood if they dare to question the standard dogma. But I wish to engage in no further polemical discussion here; this account of my lifetime in astronomy leads to more interesting topics.

POLITICS IN ASTRONOMY

The next few years in La Jolla were a time of expanding facilities, the formation of a group in infrared astronomy, and observational work at Lick and the Kitt Peak National Observatory (KPNO) on galaxies and QSOs. We became increasingly interested in the problem of the source of energy of the QSOs, the mechanisms of ejection of highly directed streams of charged particles in radio galaxies, and the problem of discrepant redshifts in small groups of galaxies. This was a period when the number of QSOs discovered and observed was growing rapidly, and Geoff foresaw that it would soon be timely to collect all the data into a catalog. Also, the first observations of absorption lines in QSOs occurred in this period—in 3C 191, by Roger Lynds and ourselves. Shortly thereafter, the discovery of a doublet of narrow absorptions in the short-wavelength wing of CIV λ 1549 emission in the QSO PHL 938 posed a fascinating problem, because the wavelength separation of the pair did not agree with the separation of CIV $\lambda\lambda$ 1548, 1551. It is interesting that the first person to point the way to identifying this doublet was Fred Hoyle, who was visiting La Jolla; he remarked that, strangely, the ratio of wavelengths of the doublet agreed with the ratio of MgII $\lambda\lambda$ 2796, 2803. Kitt Peak observations by Roger Lynds, and mine at Lick, showed the identifications of absorptions with $z_a \ll z_e$ in Ton 1530, PKS 0237-23, and PHL 938. Were these produced by gas in nearer galaxies along the line of sight to distant QSOs, as predicted by Bahcall and Spitzer, or were they related in some still not understood way to the QSOs themselves? Around this time, the first BAL QSO was discovered by Roger Lynds—PHL 5200. I recall Roger telling me that when he first took his spectrum out of the washing dish in the darkroom at Kitt Peak, and saw the enormous gap in the continuous spectrum shortward of C IV emission, his first thought was that there was a flaw in the photographic emulsion at that place! If bulk ejection of absorbing gas at velocities $\sim 0.1c$ was occurring, might some of the absorptions at $z_a \ll z_e$ also be the result of ejected gas?

During these years, Geoff and I used to spend happy weeks during the summers, both at Leiden Observatory, by the hospitality of Jan and Mieke Oort, who became our very good friends, and at Cambridge, by the hospitality of Churchill College and the Observatories. The Fowlers also spent several weeks of the summer in Cambridge, and the work with Fred Hoyle and Willy Fowler continued there. Around this time, Fred saw the need for an Institute of Theoretical Astronomy adjacent to the Cambridge Observatories, and began the fund-raising effort that resulted in the building of this extremely successful Institute. The architecture and furnishings were mostly the result of Fred Hoyle's vision; the Institute of Theoretical Astronomy (IOTA), later the Institute of Astronomy, is now known as the Hoyle Building.

However, scientific research began to mesh with scientific politics. Geoff was appointed as the U. California scientific representative on the AURA Board, that operated KPNO and the new CTIO, and he was also elected to the AAS Council. Both Geoff and I began to think seriously about the problem of gender discrimination in astronomy, which I have described earlier in this article. Outstanding astronomers such as Annie J. Cannon and Cecilia Payne-Gaposchkin had never been considered for the major awards in astronomy, such as the Bruce Medal and the Henry Norris Russell Lectureship. Women were recognized by a prize resulting from a legacy by Cannon, the Annie Jump Cannon prize, which was available exclusively for women. I was well-enough recognized now by the astronomical community to do something about this discrimination which had first hit me in 1947. Thus, when I was informed by the AAS President and Council that they had decided to award the 1971 Cannon Prize to me, I wrote them a letter (which as can be imagined went through many drafts before I sent it) declining to accept the prize and explaining my reasons: The prize, available only for women, was in itself discriminatory. I treasure among my papers a 1971 letter from Beatrice Tinsley, expressing her strong approval of my action.

This struck a powerful blow in the revolution for the recognition of women who had over decades and indeed centuries achieved major advances in astronomy, and for opening up all opportunities for young women entering the field. Geoff, on the AAS Council, told me of the consternation caused by my letter about the Cannon Prize, particularly on the part of the President and Treasurer, who anticipated various legal problems arising from the terms of Cannon's will. The membership of the AAS was also divided over the issue, as became clear later during the effort to pass the Equal Rights Amendment to the U.S. Constitution. But, young and old, enough fair-minded men have been, and are, dedicated fighters for equal opportunities—the AAS Executive Officer, Peter Boyce, is a shining example.

Other political activity was afoot. An excellent telescope, the Anglo-Australian Telescope, was under construction, and it was clear that something comparable was needed by the U.K. in the northern hemisphere; the excellent work by the U.K. radio astronomers had to depend on the U.S. for the necessary complementary optical observations. The Cyg A breakthrough had come about by identification and spectroscopy by Baade and Minkowski.

So a Northern Hemisphere Review Committee was formed by the U.K. Science Research Council (SRC); its membership included the two Astronomers Royal, R. v. d. R. Woolley and H. Brück, Sir Bernard Lovell, Fred Hoyle, J. M. Cassels (Professor of Physics at Liverpool University), and, as well-respected expatriate British astronomers, Wallace W. L. Sargent and Geoffrey Burbidge. The charge to the committee was to recommend the best way to provide first-class optical observational facilities in the Northern Hemisphere.

The report that resulted from the committee's deliberations produced a majority and a minority statement. The majority favored diminishing the power of the Royal Observatories, Greenwich and Edinburgh, and forming a new center, in Cambridge, Sussex, or Manchester, with the responsibility of searching out a first-class site for one or more well-instrumented major optical telescopes, necessarily out of England, presumably somewhere in Southern Europe, and setting up a management structure. The minority, the Astronomers Royal, were against a third center, and, such was their power in the British establishment, the Northern Hemisphere Committee Report was suppressed by the SRC and has never been published. It does, of course, exist in the archival papers of at least two of the majority members.

At this time, however, the retirement of Sir Richard Woolley from RGO (the Royal Greenwich Observatory) at Herstmonceux was imminent, and the SRC had to look for a new appointment. During the summer of 1971, while Geoff and I were in Cambridge, I was approached by Sir Brian Flowers, head of the SRC, and asked whether I would consider accepting this appointment if it were offered. Despite my foray into the U.S. political scene as a spearhead for abolition of gender discrimination, I knew then, and have always known since, that I do not have the temperament to direct a major scientific establishment. I explained this to Flowers, and pointed out that Geoffrey did possess the right abilities and temperament, and the position should much preferably be offered to him. The weak response was that the consensus of U.K. astronomers wanted "an observational astronomer" in the position. Filled with doubts, I said I would think it over, and let them know my decision. Geoff and I then left Cambridge to spend a few weeks at a summer school in Italy.

Skilled politicians, willing to use methods that verge on the unethical, know how to play upon the loyalties of those unskilled in politics. In Italy, over a very bad phone line, I was informed that the British press had learned that I was a candidate for the job, and there would be write-ups in the newspapers, which would be bad for the future of U.K. astronomy and the Northern Hemisphere Observatory (NHO) if I were to refuse the job. I was also informed that the Crown would split the position of Astronomer Royal from the RGO Directorship (they had been linked for some 300 years), and a male scientist would become Astronomer Royal. An SRC senior appointment would be offered to Geoff, and we could continue our joint work at Herstmonceux.

So, misgivings laid aside but not forgotten, I accepted, visited London and the SRC headquarters in the autumn of 1971, and we went on leave of absence from UCSD in the summer of 1972; I knew then that it would be foolish to resign my faculty position at UCSD.

MORE POLITICS; THE ANGLO-AUSTRALIAN TELESCOPE; SEARCH FOR A NORTHERN HEMISPHERE OBSERVATORY SITE

It quickly became apparent that the RGO Herstmonceux staff was divided into two halves. There were our good friends the Pagels and the younger astronomers, largely recruited by Woolley, whose overriding interest was in astronomical research, together with the astronomy department at Sussex University. And there were what I came to label "the old guard"—the sunspot and solar astronomers, the Nautical Almanac group, the Time Service group and two groups who wielded much power—"Staff Side," and the union. Almost immediately after my arrival, the research astronomers were asking me how soon I would manage to engineer the moving of the Isaac Newton 100-inch telescope (INT) to a good site, out of England, where it could be used for frontier astronomical research. Located as it was, some 300 feet above sea level, in an area named Pevensey Marshes, the years of waiting for this telescope to be built had clearly ended in the disastrous choice of its site, S.E. of London (the prevailing wind is usually NW). Next, the first staff meeting of the senior staff was attended by myself and, by my invitation, Geoff. The kindest of "the old guard" was clearly deputed to come to my office sometime afterwards, and to tell me as gently as he could, that Geoff would not be welcome at any future meetings. This caused me much distress, and clearly was out of the spirit of the implied—but *only* implied, promises of the SRC. We endured the situation for a few weeks, then Geoff and our daughter left Herstmonceux and returned to La Jolla. They came back to England for Christmas, 1972, and during this time went through the INT observing books and totaled up the number of hours of observing actually achieved during its 3 years of operation. The telescope was available to university as well as RGO astronomers, upon acceptance of good observing proposals. It was heartbreaking to read in the observing record comments such as this: "Exposure ended; kept the dome open, but watched the guide star slowly disappear into the murk. Closed the dome." Walking to the dome in the early evening, one would walk through a gentle dewy mist rising through the grass. The total annual observing hours averaged 600–800 per year, in comparison to some 2000 per year at the best sites worldwide.

Efforts to initiate plans for moving the INT, when a good NHO site had been found, met bitter opposition from the old guard staff, and the local Hailsham dignitaries (the right honorable Quintin Hogg had been Member of Parliament for Hailsham). Never one to suffer fools gladly, Geoff wrote what became a famous letter to *Nature* (Burbidge 1972) exposing the situation at Herstmonceux. Brian Flowers summoned us both to his office in London, and a bitter confrontation ensued.

At this point, only two factors kept me at Herstmonceux. One was the fact that I was, by virtue of my position, a member of the U.K. half of the Anglo-Australian Telescope Board (Fred Hoyle was chairman, and Jim Hosie was the SRC representative). Meetings were mostly in Canberrra, with trips to Siding Springs, where the dome was under construction. Details concerning the telescope design and construction, and discussion of the bids to be accepted from contractors, were under way. It was Fred Hoyle who pointed out that the telescope design, largely based on the design of the KPNO 4-m Mayall telescope, should be corrected for a problem with the declination mount and drive which had been found at KPNO and had needed correction.

The other factor that kept me at Herstmonceux was the continued loyalty of the research astronomers and the Sussex astronomers and students, too many to name here, who fulfilled the promise of their abilities when the AAT and NHO came into being. The search for a good site for the NHO proceeded at a snail's pace, but the expertise of Merle Walker (of Lick Observatory) was enlisted for comparing various possible sites in the Canary Islands, the Azores, and southern Spain.

The site eventually chosen, on La Palma, has proven to be very good, and the removal of the INT to that site has enabled astronomers to do forefront research with it. The adjustment to the mounting, necessary because of the difference in latitude, had already been considered in a preliminary way while I was at Herstmonceux, and the full-scale engineering design and construction turned out excellently.

Eventually, after one and a half unhappy years, I set about submitting my resignation as Director of RGO. Some meetings in London ensued; I was asked to delay my resignation so as not to upset AAT and NHO negotiations, so I delayed as long as I felt useful. At two meetings in London, after my resignation was finally accepted, I was informed that the news media would be on my track that evening when I returned to Herstmonceux. The warnings proved correct. The next morning I set off to drive to Lewes to take the train and attend the wedding of my nephew and godson. A horrible automobile accident on the way landed me in the hospital and nursing home for some three weeks, during which the wrong leg was labeled on the x rays as needing traction to heal a cracked hip joint. I walked on the injured leg for some weeks before the incorrect reading of the x rays was discovered. All in all, I can look back upon those weeks before my daughter came over to help me pack up from Herstmonceux for the return to California, as due payment of my "blood money" to get out of the intolerable situation. At the orthopedic surgeon's office in La Jolla, after new x rays were taken, the doctor came into the examination room where I was awaiting his opinion, and said "You might as well throw away those crutches; they haven't been doing you any good!" And he and the nurse showed me the new x rays, with right and left hips clearly marked! I had been getting on and off planes to Australia and California, and moving around in Australia, using crutches to keep my left leg off the ground, having been enjoined to put my weight "*only* on the right leg." The experience had, to say the least, been painful, but seems to have left no residual damage to my right hip joint.

MORE POLITICS: PLANNING FOR A LARGE SPACE TELESCOPE; U.S. CITIZENSHIP

I had served on the Space Science Board, whose mandate is to consider and make recommendations on all ventures in space science. A large space telescope for the UV and optical, as envisioned decades earlier by Lyman Spitzer, was being seriously considered, since the state of technology for such a telescope, with a 3-m primary mirror, seemed feasible. Much political activity was underway; astronomers were working on Congress and the NASA hierarchy; the enthusiasm and support for the project by Nancy Roman within NASA was of fundamental importance in the ultimate agreement to fund and plan a 2.4-m (rather than Spitzer's 3-m) telescope with imaging and spectroscopic capability down to the MgF_2 reflective cutoff in the ultraviolet (1150 Å).

James Fletcher was the NASA Administrator. He was more interested in solar system exploration than in galactic or extragalactic astronomy. Astronomers who, throughout their careers, had done their observational work from the ground, were enthusiastic about the prospect of a large, well-instrumented telescope in orbit, above the UV-absorbing ozone layer in Earth's atmosphere, and with a diffraction-limited mirror that could take advantage of the absence of atmospheric-induced "seeing" distortion of images, so they had volunteered their time to attend a Washington meeting to help make an impressive presentation to Administrator Fletcher. The result was that Fletcher was convinced; NASA was awarded the finances for this Space Telescope, with a planned lifetime of 15 years, but it was to be placed in low Earth orbit because it would, as Fletcher had dictated to the Space Science Board, be launched by the Space

Shuttle which would visit it during the 15-year lifetime, for updating, replacements of instruments, etc and, in fact, space astronomy would in the future be tied to launches by the Shuttle.

I return to more about the Space Telescope later, after a digression on my involvement in the American Astronomical Society, acceptance as a U.S. citizen, and service as President of the American Association for the Advancement of Science.

When I was informed of my election as President of the American Astronomical Society, I realized that I would be involved in political activity at the Federal level. During all the years since 1955, Geoff and I had maintained our U.K. citizenship. With no basic language change, there is less incentive for the British immigrants to become U.S. citizens. "Green cards" ("pink cards" as they now are) enable one to pass freely through the passport control desks at airports, so the main disadvantage for "resident aliens" is that they are disenfranchised. I felt that, if I were to be involved in congressional presentations and discussions with politicians, I should do so as a U.S. citizen; I would be able to talk with and write letters to people for whom I had voted (or not voted, as the case might be). So the lengthy and tedious business of applying for citizenship began; in San Diego, this involved a long waiting period. Eventually, the effort succeeded, and I felt that I could honestly serve the American Astronomical Society as President and represent it in some acrimonious business with the U.S. Post Office, which was attempting to require a footnote stating "this is an advertisement" to all pages of Astrophysical Journal publications for which there were page charges. Fortunately, reason prevailed, even with the then Postmaster General.

Further political activities arose when the Equal Rights Amendment to the U.S. Constitution was before the nation; it was ratified by all but three states, and most scientific societies were banning annual meetings from being held in the negative states. The AAS membership was divided about 6 to 5 on the issue; I received mail that ranged from vituperous condemnation of me as President for letting this issue come before the Society, to very supportive letters from men and women anxious to see the removal of the barriers and inequalities suffered by women in science, and indeed in life in general.

The years of my involvement in the American Association for the Advancement of Science did lead to political activity. In my opinion, the AAAS is a great and extremely valuable organization, and, in recent years, its involvement in a massive effort toward improvement of education in science and mathematics in the U.S. has helped to raise the level of concern throughout the country. The AAAS "Project 2061" was just beginning in my third year on the AAAS Board, as Past President, and is now doing its work throughout the nation's schools.

QSOs, COSMOLOGY, AND THE HUBBLE SPACE TELESCOPE

Throughout these years, work in extragalactic astronomy continued. There was a break in the collaborative work with Geoffrey, when he was offered the Directorship of the Kitt Peak National Observatory which he accepted; he moved to Tucson in 1978, and held this position until 1984 when he returned to the University of California.

The next Space Telescope planning involved the choice of the first-generation instruments. Apart from the obvious guide-star acquisition and tracking instrumentation and an on-axis camera, other instrumentation was open for discussion. European participation in the telescope was eagerly accepted; one of the four instruments would be a camera/spectrograph designed and built by astronomers in Europe. The remaining "slots" were allocated to two US spectrographs and a fast photometric system. Teams were rapidly formed; I was invited to join the Harvard/Smithsonian Faint Object Spectrograph Team. Contractors were chosen, instruments were designed, and proposals to NASA were submitted.

For the U.S. spectrographs, many optical experts realized that detectors were of prime importance, and these had better be photon-counting detectors of proven design and performance. Two teams were chosen, both using the Digicon detector of McIlwain and Beaver (Beaver & McIlwain 1971, Beaver et al 1972). One was for the Goddard High Resolution Spectrograph, the other, a small team headed by Richard Harms of UCSD, was for the Faint Object Spectrograph (FOS). Team members were re-designated by NASA; I found myself on the UCSD FOS team.

The award of such a major NASA contract to UCSD provided a considerable perturbation to the smooth running of the business office of the Physics Department. Management of the contract with Martin Marietta, Denver, was seen by NASA to be inadequate, and our Chancellor McElroy was told in no uncertain terms that if something were not done to improve the management, the contract would be canceled. The Chancellor's remedy was to create a new Organized Research Unit, the Center for Astrophysics and Space Sciences, in a building where the High Energy Astrophysics and the Infrared groups already had their labs and offices. The choice of a director for this ORU posed a problem, because of the perturbation, just mentioned, within Physics. As a Co-Investigator of the FOS, I was asked to take on this job. The usual term for director of an ORU is five years, but I was asked to stay on longer. After the failure of some attempts to recruit well-known astronomers from outside UCSD, which were carried out rather ineptly, Larry Peterson, head of the High Energy Astrophysics group, agreed to take on this rather thankless task, and my service ended in 1988.

After launch of the Space Telescope in 1990, the subsequent discovery of the faulty figuring of the primary 2.4-m mirror is too well known to discuss here.

All I wish to emphasize is that astronomers are used to coping with few photons, atmospheric turbulence, less-than-perfect telescopes and instruments, and, one and all, they rose to the challenge and exciting results have been produced by the Space Telescope, named, perhaps unfortunately for his memory, the Hubble Space Telescope. My first observation with the Faint Object Spectrograph, carried out at Goddard Space Flight Center, did cause me euphoria similar to what I felt when taking my first direct photograph of a galaxy—to be looking for the first time at far-UV light never before detected, coming down from outer space.

The QSOs have continued to provide puzzles. The redshifts, if due to the expansion of the universe, require very large energy releases from "the central engines," and highly relativistic outflow of charged particles. There are also the observations of associations between galaxies and QSOs where the QSOs have redshifts very different from the galaxies (the old problem of "discrepant velocities"); these are well documented by Burbidge et al (1990) and Arp (1987). I find myself very much attracted by the new ideas of Hoyle et al (1993), who have described a Quasi-Steady-State-Cosmology, in which matter is created in successive epochs, rather than in a single event marking the "beginning" of the universe. We observe matter pouring out of the centers of active galactic nuclei, and streams of relativistic particles from the centers of radio galaxies and QSOs; the concept of these outpourings as "creation events" in the presence of very strong gravitational fields appeals to me philosophically and observationally.

CONCLUSION

When I was invited by the ARAA Editorial Board to write this account, one reason given was that most of the younger observational astronomers are not aware of what it was like to use optical telescopes before TV, 2-dimensional photon-counting devices, and computers made possible today's remote observing—either remote in the sense that one is in a warm lighted console room next to the telescope dome, or remote at a terminal in an entirely different location. Thus I have concentrated on my past experience, and the literature cited contains publications of which most astronomers who entered the field less than 10, 15, or even 20 years ago will be unaware. In recalling the past, I became aware of how much I have owed to various friends throughout the years, and, above all, that this is an account, not of one life in astronomy, but of two—Geoff's and my own.

ACKNOWLEDGMENT

I close with particular thanks to Betty Travell for her patience, accuracy, and continued help in the major work of preparing this manuscript.

Literature Cited

Arp H. 1987. *Quasars, Redshifts & Controversies.* Berkeley: Interstellar Media
Babcock HW. 1939. *Bull. Lick Obs.* 19:41
Beaver E, Burbidge M, McIlwain C, Epps H, Strittmatter P. 1972. *Ap. J.* 178:95
Beaver EA, McIlwain CE. 1971. *Rev. Sci. Instr.* 42:1321
Bidelman WP, Keenan PC. 1951. *Ap. J.* 114:473
Burbidge EM, Burbidge GR. 1951. *Ap. J.* 113:84
Burbidge EM, Burbidge GR. 1953. *Observatory* 73:69
Burbidge EM, Burbidge GR. 1957. *Ap. J.* 126:357
Burbidge EM, Burbidge GR. 1975. In *Galaxies and the Universe,* ed. A Sandage, M Sandage, J Kristian, p. 81. Chicago: Univ. Chicago Press
Burbidge EM, Burbidge GR, Fowler WA, Hoyle F. 1957. *Rev. Mod. Phys.* 29:547
Burbidge G. 1972. *Nature* 239:117
Burbidge G, Hewitt A, Narlikar JV, Das Gupta P. 1990. *Ap. J. Suppl.* 74:675
Burbidge GR. 1956. *Ap. J.* 124:416
Burbidge GR. 1959a. *Ap. J.* 129:849
Burbidge GR. 1959b. In *Radio Astronomy IAU Symp. No. 9,* ed. RN Bracewell, p. 541. Stanford: Stanford Univ. Press
Burbidge GR, Burbidge EM. 1953a. *Ap. J.* 117:407
Burbidge GR, Burbidge EM. 1953b. *Ap. J.* 118:252
Burbidge GR, Burbidge EM. 1955. *Ap. J. Suppl.* 1:431
Burbidge GR, Burbidge EM, Sandage AR. 1963. *Rev. Mod. Phys.* 35:947
Greenstein JL. 1984. *Annu. Rev. Astron. Astrophys.* 22:1
Gregory CCL. 1966. *Q. J. R. Astron. Soc.* 7:81 (Obituary by EM Burbidge)
Hoyle F. 1946. *MNRAS* 106:343
Hoyle F. 1954. *Ap. J. Suppl.* 1:121
Hoyle F, Burbidge G, Narlikar JV. 1993. *Ap. J.* 410:437
Hoyle F, Fowler WA, Burbidge GR, Burbidge EM. 1964. *Ap. J.* 139:909
Matthews TA, Sandage AR. 1963. *Ap. J.* 138:30
Mayer MG, Teller E. 1949. *Phys. Rev.* 76:1226
Page T. 1975. In *Galaxies and the Universe,* ed. A Sandage, M Sandage, J Kristian, p. 541 Chicago: Univ. Chicago Press
Schwarzschild M. 1954. *Astron. J.* 59:273