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AN ASTRONOMICAL RESCUE

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After school, on the dreary afternoon of November 22, 1928, I went down to the Seattle Public Library to seek some forbidden books, i.e. books on astronomy. There I found the second volume of Russell-Dugan-Stewart *Astronomy,* with the tantalizing title of "Astrophysics and Stellar Astronomy." I checked it out and read it with great enthusiasm; the text was always fascinating if not always intelligible to a high school sophomore. My elders were annoyed, so I had to read it surreptitiously, at school or at home. From that moment on, I knew what I wanted to do in life but did not yet appreciate the obstacles that were to be cast in my way. There was no surer road to ruin than to have followed the advice of my elders, particularly that of my worthless old man.

In the autumn of 1928, I was living in Seattle with my oldest brother, Leeon. The old man had parked my mother and me there until he deemed me old enough to work as a slave laborer on his crackpot mining venture.

Old Worthless had spent his youth in the late nineteenth century amid the dying embers of the Old West, while his elders spun marvelous tales of a frontier golden age that never was. A competent printer, he made a living at that trade in various places, ending up in Tacoma where I was born in 1913. But in 1922, he threw it all overboard to "go walkabout" in California, in Alaska, and once more in California, ostensibly looking for gold. He taught three of my older brothers the printing trade, but Lee, who was seven years older than I, finally just ran away to sea. After 1922, the old man could not be coerced to support his family. He had devoted an increasing fraction of his time to activities at which he was abysmally incompetent: first politics and then searching for gold. His father had had good luck in the California Gold Rush of 1849. The old man thought (or professed to think) he could do the same with identical, archaic, 1849 techniques in 1929, in an area that had been exhaustively mined by such primitive methods many decades before.

The old man's real ambition was to regress to the life-style of the 19th century frontier, complete with log cabin, water hauled in buckets from the river, and

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minimal comforts, but with the horse replaced reluctantly by a model-T Ford, and with candles replaced by a gasoline lantern. To implement this chimerical scheme he staked out some gold-mining claims at the very northern edge of California. He had inveigled my brother Louis to assist and from time to time also enticed various gullible people to help him. These assorted n'er do wells, drifters, boobs, and klunkers were not reliable enough. He needed real subservient slave labor: That is where I came in.

The gold-mining enterprise was based on three fallacies: 1. Gold was there and could be extracted by archaic, crude, mid-19th century methods; 2. the place was ours—actually it was a mining claim for which no valid title was ever held, either by Old Worthless or by anyone else; and 3. it would provide a splendid refuge when the social order collapsed. No religious fanatic believed more fervently in the Second Coming than Old Worthless believed in the imminent collapse of the social order. I was puzzled that I was the only one at "camp" who clearly understood that the whole crackpot enterprise was nothing but backbreaking nonsense, certainly doomed to catastrophic failure. That I was able to escape was little short of a miracle. It came about by a remarkable confluence of circumstances.

Basically, my quarrel with Old Worthless was not really about my studying the despised subject of astronomy; it was about getting an education. Although the old man would not put himself on record as being opposed to an education for me, he was gung ho to see that I did not secure one. After all, who needed a lot of book learning if the complete collapse of the social order was coming any day now?

A grade school friend of mine in San Francisco, James Fidiam, told me about the Astronomical Society of the Pacific (ASP) and had loaned me a stack of their leaflets. Especially after getting a glance at Russell's book, I was eager to join up. The opportunity came in 1929 when my brother Paul gave me three dollars for my birthday. I was severely chided for squandering such a precious gift to join that "crazy society in San Francisco," rather than spending it on socks to be worn while wading in icy water with leaky boots tending a sluice line. In one of the first issues of the ASP publications that I received, I saw an article by Donald H Menzel on the interpretation of the spectrum of Jupiter and wrote him about it. The idea I suggested turned out to be quite incorrect, but we struck up a correspondence. I also wrote some Mt. Wilson astronomers, Adrian van Maanen and Seth B Nicholson, who were very sympathetic and helpful. van Maanen arranged for a friend, Tod Ford, to send me the two volumes of Russell-Dugan-Stewart. I studied these volumes diligently and tried to learn math and physics from some rather poor texts that Del Norte County had sent me. My contact with a couple of amateur astronomers, Rev. Harold N Cutler and a Canadian, AR Dunlop, turned out to be very helpful. The latter sent me a spectacle lens of 1 meter focal length and a microscope eyepiece with which I contrived a primitive telescope, which did suffice to show me, for the first time, the rings of Saturn, and resolved Mizar; Cutler sent me a mirror grinding kit. I started to grind a 4-inch mirror, but did not finish before the curtain was rung down on the dark age of my life. My brother, Paul, who had given me a spyglass in 1927, now sent me a prism. Using a photographic lens for a collimator, I was able to make a primitive spectroscope with which I saw my first spectral lines: the unresolved yellow D lines of sodium.

Paul and his wife, Doris, came up for Thanksgiving in 1931. The old man agreed to let my mother and me take a brief holiday in Oakland and San Francisco. Thus it came to pass that I met Donald Menzel, who was teaching a couple of courses at UC Berkeley that autumn. He suggested that I try his Astronomy 1 final, which I did the following week. I also took some exams in math and physics where I did not fare quite so well. Menzel went to Merton Hill, the UC director of admissions, to suggest that I be admitted as a special student. "What?" said Hill, "That fellow did not even complete high school." Menzel then told my story of how I had walked in straight out of a mining camp and had written the best exam in the class. Don won his point; six weeks afterwards I was enrolled at UC on trial. Paul and Doris had engineered my getting away from Old Worthless, but it was my sister Jane and her husband Frank who clinched the escape by providing board and room. Before Menzel returned to Lick he persuaded the chairman of the Berkeley Astronomical Department to provide me with part-time jobs at the observatory and working in his garden. Prof. Armin O Leuschner was a charming sympathetic gentleman who supplied another essential ingredient necessary to make good my escape.

The long uphill climb that followed was not easy. Two and a half years of starvation, misery, and deprivation under the yoke of Old Worthless had done their deadly work. My basal metabolism had dropped severely, slowly tending towards the level of myxedema; had it not done so I might not have survived the ordeal at camp. Recovery was slow. Each night I slept ten hours as though drugged. Also, the Berkeley medics discovered a cardiac valve problem—a consequence of my bout with rheumatic fever in 1920. This affliction accounted for my shortness of breath and miserable performance in sports; the heavy labor at camp would have killed me in a short time had I not escaped.

It was hard to study, and recovery was slow and difficult. Thus, in 1932, I found the university very different from the high school from which I'd been dragged in 1929. Math and physics were often not easy, but they were logical and yielded to sustained perseverance. I had the good fortune to have a special friend, Russell Cowan, who taught me most of what I was ever to know about mathematical analysis and particularly differential equations. I've always been, not just bad, but terrible at languages and would surely have come to grief in German had it not been for tutoring by Prof. Leuschner's mother-in-law, Mrs. Denicke. Worst of all was an English Literature course taught by a Prof. Baker, who knew the exact interpretation of every utterance by each character in *King Henry IV*. I made the mistake of arguing that alternative explanations could not be excluded—so I flunked. At the same time, we had a janitor at the observatory who lectured to me on the Bible as he washed the blackboards and mopped the floors. He knew the exact meaning of every symbol used in the Book of Revelations. The two great cornerstones of British literary tradition are Shakespeare and the King James version of the Bible. The janitor gave by far the most inspiring presentation. Looking back after sixty-odd years, I now feel that I was unfair. After all, how could a Byzantine tale of intrigues against an English king of whom I'd never heard and did not give a hoot compare with the vivid Gospel story of the Great Harlot, Babylon, the mother of Abominations and the wondrous Beast whose number was 666?

Despite my escape from the 19th-century frontier, I still found myself in an astronomical department steeped in 19th-century traditions. Although William F Meyer taught a course on practical aspects of astronomical spectroscopy, C Donald Shane presented modern ideas on such topics as stellar atmospheres, and later Robert J Trumpler covered topics in statistical astronomy and galactic structure, the big push was on orbit theory. Celestial mechanics and orbit theory have an elegance that was almost totally obscured by the tedious massaging of equations to render them suitable for logarithmic calculations. Mechanical calculating machines had appeared but were totally despised by people like Russell Tracy Crawford. The department was severely ingrown. Until Trumpler arrived there was nobody who had not come up through the ranks of the Berkeley Astronomical Department. Although Leuschner was a terrible lecturer, he was also one of the most inspiring teachers I've ever had. He would suggest new ideas and approaches in such a way as to fire the enthusiasm of even this dyed-in-the-wool spectroscopist. We students came to regard the astronomy department as a kind of family, in which Leuschner was the patriarch, Sturla Einarsson was the kindly old prof on whose shoulder you could cry, and Donald Shane was the Dutch uncle who saw that we were always aware of the hard realities of life.

The physics department was active and forward looking. Their colloquia were often inspiring. Thanks to an invitation from my mentor there, Harvey White, I was able to attend a joint excursion by the Stanford and Berkeley departments to Lick Observatory in April 1934. In one of the most beautiful physics lectures I have ever heard, Prof. Felix Bloch of Stanford described experimental evidence for the existence of an elusive particle called the neutrino. I came away convinced that this strange particle must exist but did not think it would be important in astronomy.

The atmosphere at Lick, where I went to work during the summer of 1937, differed profoundly from Berkeley's. I lived in utter terror of committing some faux pas that would incur the wrath of Director William H Wright and his courtiers. Alas, that is exactly what came to pass. One afternoon when I went down to open the Crossley dome to allow the telescope to come to some sort of temperature equilibrium before the night's observing, I saw Wright watering his lawn and asked him how he liked being a gardener or words to that effect. He became very angry and said that if members of the Lick staff wanted to joke with me, that was their privilege, but that under any and all circumstances, I should show proper respect for my superiors. I apologized and thenceforth took great care not to offend him; somehow I managed to survive. That summer at Lick, I had the very good fortune to serve as Nick Mayall's assistant, observing globular star clusters and galaxies. From him I learned the real basics of observational astronomy. In particular, I recall the work we did in 1938–1939 on the rotation of the spiral galaxy, M33. I've worked with many excellent astronomers, mostly as a junior partner, but the M33 study is the one of which I am most proud. Nick was a meticulously careful, patient teacher. Also on the Lick staff were Arthur B Wyse and eventually Gerald Kron. Wyse had been my first teacher in astronomy at Berkeley. He was a handsome man of great charm and ability and a superb teacher; his great range of interests made him an especially valuable asset to young, aspiring astronomers. Kron, a student of Stebbins, developed photoelectric photometry at Lick. The great tradition at Lick had been the measurement of radial velocities of stars and nebulae. Menzel had been their single, lonely, theoretical astrophysicist, and he had found it agreeable to move to Harvard in 1932 since the then-director, Aitken, had no use for theory. It was precisely because I was interested in the interpretation of observations that I decided to go to Harvard to work with Menzel. I was the first Berkeley student to desert the fold, enlist under the banner of the arch rival, and make a go of it. For this act of apostasy I was never forgiven!

In contrast to the Berkeley I knew in the 1930s, Harvard was a very stimulating and inspiring place. There, Shapley had assembled a young, enthusiastic, capable staff. Besides Menzel there were Bart Bok (galactic structure), Fred Whipple (mostly solar system problems), Cecilia Payne-Gaposchkin (mostly spectroscopy)—to mention the most capable go-getters. Among the students my closest associates were Leo Goldberg and James Baker, both of whom later obtained jobs at Harvard. One quickly became involved in some type of research, but unless you were a pure theoretician or were concerned only with simple stellar imaging, you found difficulties. Aside from venerable objective prisms on small telescopes, the spectroscopic equipment was nonexistent. There were no slit spectrographs, even under construction. Shapley was concerned with problems that could be done by direct photography and had no interest whatsoever in spectroscopy. I was amazed that Menzel had not pushed for the construction of at least one spectrograph. To obtain data for my thesis on planetary nebulae, I had to return to Lick in 1938 and 1939.

Menzel, in collaboration with Baker, was developing the series, *Physical Processes in Gaseous Nebulae,* an endeavor in which I soon became involved.

Also, I was able to work on high-dispersion stellar spectra, using tracings of Mt. Wilson Coudé plates, which had been made for me by Louis Berman. Data on the spectrum of Gamma Geminorum were secured when Menzel supplied Yerkes with some atomic f-values. Menzel & Baker's series proffered the possibility of obtaining nebular chemical compositions. Ira S Bowen and Wyse had secured excellent data with the aid of Bowen's image slicer. In his last paper, Wyse presented spectroscopic data for 10 nebulae, but the interpretation required collision strengths and A-values. Malcolm Hebb and Menzel had calculated collision strengths for [O III]; hopefully other ions could be done. Michael Seaton later greatly improved the basic theory. The last paper of the Harvard nebular series gives methods for obtaining nebular chemical compositions from both permitted and collisionally excited lines. Except for a few additional physical processes such as dielectronic recombination and charge exchange, the basic methods in use today remain essentially the same as described in that 1945 paper. What has changed has been the vast improvement in atomic parameters, such as A-values and especially collision strengths, and construction of realistic theoretical nebular models made possible by today's powerful computers.

Other engaging possibilities appeared. Fred Whipple and Cecilia Payne-Gaposchkin had attempted to explain supernova spectra by a spectrum synthesis method. Their evident success inspired hope that some nonconventional approach might work on Wolf-Rayet stars. I secured a number of spectra at the Crossley telescope and set out to interpret them by a rather simple semi-empirical routine, whereby I found excitation temperatures and ionic concentrations for both N and C sequences. I suggested that WR stars were the remnants of massive, luminous stars that had ejected their outer envelopes into space. At the Yerkes, 1941 American Astronomical Society meeting the paper drew mixed, largely unfavorable reviews from some expert stellar spectroscopists. Cecilia and I developed an even worse heresy. She had noted that in their nebular stages, nova spectra were often so different as to suggest that actual chemical composition disparities can exist between objects; we wondered whether the methods of the Harvard nebular series could be applied here. We explored the idea. Although we were hampered by the lack of accurate atomic parameters, we were able to convince ourselves that for any reasonable values of the relevant atomic parameters, the observed intensities of ionic lines of O, S, Ne, and Ar would imply chemical composition differences. Our views were vigorously denounced; one prominent spectroscopist remarked that most astrophysicists would be so aware of the uncertainties involved that they would not make such radical assertions as we did. The most ferocious attack of all came from our own bailiwick. We never published the paper. It was found among Cecilia's documents by her biographer.

My research record by now included a contribution to the nebular theoretical team effort, the measurement of the rotation of M33 made with Nick Mayall, some high-dispersion stellar spectroscopic studies, an investigation of Wolf-Rayet stars, and an exploration of H II region excitation as a function of distance from the center of M33. During the war years I hoped that this record would result in an offer from an observatory or university department wherein I could continue observational work. As the war ended, none of the well-known astronomical institutions offered or promised me anything or even suggested that I was on their "list."

Early in 1942, I went to work in the Harvard physics department, teaching elementary physics, electricity and magnetism, and physical optics at the undergraduate level. I still continued to work on nebular and spectroscopic problems as spare time permitted. I had noticed that the 3729/3726 nebular line ratio of [O II] did not agree with Pasternack's extensive calculations for strengths of forbidden lines. Wilbur Ufford, JH VanVleck, and I began an intensive study of electronic spin-orbit interactions in ionized oxygen. Ultimately, we achieved success and found that the observed ratio depended on electron density. Thus, when collision strengths were calculated for ionized oxygen, a powerful tool was provided for obtaining electron densities in nebular plasmas.

I left the Harvard physics department in June 1943 to work at the Radiation Laboratory (Rad Lab) in Berkeley on the electromagnetic separation of U^{235} and U^{238} isotopes. My most enduring memory of this project was of the utter contempt of the military-directed mind for the misguided scientist who believed he should use his utmost training and ability to attack the technical and/or scientific problems at hand. After an initial hands-on introduction to the actual electromagnetic (EM) process, I was assigned to the magnet group. Next I worked with Prof. Harrie Massey on what might be called a basic physics group, involving both experiment and theory. A primary objective was to improve the efficiency of the EM separation process. The experimenters had been following a trial-and-error approach, but Massey proposed to examine fundamental properties of arcs in magnetic fields, using both theory and experiment. Here was a program well suited to my training in atomic physics. I felt that at last I was now able to contribute something worthwhile.

Suddenly in June 1944, the authorities decided to remove me from Massey's team and ship me to Oakridge to serve as an office boy, file clerk, and courier for the reports office. The only real qualifications for this job were an ability to read and write and a profound respect for security; my scientific skills were just so much useless baggage. I protested and before they confronted me with dire threats, a technician accepted the job. I was temporarily off the hook, but my intransigence was neither forgotten nor forgiven. In October 1944, there was a wave of optimism that Germany would immediately collapse. A directive was circulated to the effect that the lab would be downsizing and those who had been asked to go to Oakridge and had demurred would be the first to feel the ax. They were as good as their word, although the purge was delayed until

Germany fell in 1945. One May morning, Massey came into his office to find that half his group had been dismissed. He had not been consulted. My name headed the list. Thus, months before the bomb was first tested, I was sacked.

At this juncture I could have been confronted with a first-class personal disaster. In the autumn of 1944, the Hoosiers[1] asked if I would be interested in making a definite commitment to come to Indiana University when my work at the Rad Lab was done. I mentioned this in a letter to Donald Shane, on whom I had always relied for good advice in my student days. He replied that it would be a mistake for me to pledge myself for more than one year; he was very emphatic on this point, stating it three times in his letter. Shane had already been appointed the new Lick director, effective at the end of the war. Fortunately, I did not know this or I might have been badly confused. After a careful consideration of the possible ramifications of such a course of action, I unequivocally rejected Shane's recommendation. Later I learned that had I been so unwise as to follow his advice, the Hoosiers would have come to the totally wrong conclusion that I had a job lined up in California. (Actually, I never received any "feeler" or offer from California until 1961, and that was from UCLA.) Indiana certainly would not have agreed to a one-year contract and would have broken off negotiations. After the Rad Lab purge, I would have been unemployed with no viable prospects for any decent job.

A week or so after receiving Shane's interesting letter, I began active negotiations with the Hoosiers. By the day of the purge every detail had been worked out and I was appointed immediately as an assistant professor at Indiana University with teaching duties and pay starting in September 1945. I now had about three months in which to get back into astronomy. Actually, I had been doing astronomy all along, on evenings and by pooling my days off into blocks of time so I could go observing at Lick. I secured a series of observations with the Crossley slitless spectrograph for an extended sample of planetary nebulae, in order to assess plasma diagnostics and ionic concentrations. David Bohm and I had examined the velocity distribution of electrons in a nebular plasma by comparing rates of processes that destroyed a Maxwellian distribution—inelastic collisions, recombinations, bremsstrahlung—with those that restored it, mostly electron-electron collisions. We found that a Maxwellian distribution did prevail. Massey taught me practical methods for calculating wave functions for collision strengths for O^+ and N^+. We also examined the possibility that the O^- ion might contribute to the solar atmospheric opacity as does the H^- ion. We concluded that the oxygen contribution was not significant. I also wrote the text of the last of the nebular series papers and the rough draft of much of my 1956 book, *Gaseous Nebulae.*

Although the Rad Lab purge had very severely hurt us financially, there was a silver lining in that I was able to spend a month at Mt. Wilson, where I worked

[1] People living in Indiana.

with Minkowski on the IR spectrum of NGC 7027 and traced Coudé plates of O and B stars for curve of growth abundance analyses. Contacts with Mt. Wilson astronomers, particularly Rudolph Minkowksi, Paul Merrill, Roscoe Sanford, Olin Wilson, and later Bowen turned out to be most valuable for the years to come. In August, I met Frank Edmondson at McDonald Observatory and secured some nebular spectra with the 82-inch reflector.

When I arrived at Indiana University (IU) in September 1945, I found not only an attractive campus but an enlightened administration. No telescopic facilities suitable for my work were available in Indiana, but IU bought time at McDonald Observatory via the University of Chicago, which operated the place. The arrangement turned out to be satisfactory. Even more important, though, was the opportunity to work at Mt. Wilson. At first, my efforts were directed mainly to analyzing Coudé and other high-dispersion plates of early-type stars. Initially, I used Unsöld's method but later introduced some refinements of my own.

During my sojourn in Indiana I wrote the first rough draft of my astrophysics volumes, *Atmospheres of the Sun and Stars* and *Nuclear Transformations, Stellar Interiors and Nebulae,* circulated copies to colleagues, and tried them out on my students. I received a great deal of good advice. The astrophysics text was to have been published by Blakiston. They did not like the manuscript I submitted and made a number of suggestions, which I tried to carry out. Finally, after I'd moved to Michigan, I sent them the rewritten, revised, and expurgated edition. They replied that although I had done everything that had been requested, they were not going to publish it anyway. Blakiston had been absorbed by Doubleday, which had no interest in books for such a limited market. They were gung ho for Velikovsky's *Worlds in Collision.* Other publishers rejected it because "there was no profit in a book of such few likely sales." Eventually, it was published by Ronald Press Company. Years of rewriting and negotiation were required, but in the end I think that the effort paid off. A second edition of the stellar atmosphere volume appeared in 1961. The subject has been enormously transformed since then, as a consequence of new theoretical insights and the development of powerful computational techniques. Non-LTE effects and even expanding atmospheres can now be handled on an almost routine basis.

At Indiana, I taught several advanced classes, some of them based on notes written for the astrophysics books. Frank Edmondson and Jim Cuffey worked in quite different fields from my own. There was nobody with whom to discuss astrophysical problems, although for fundamental physics, conversations with Emil Konopinski were most enlightening. Alas, living conditions were depressingly inferior to what we had enjoyed at Berkeley; primarily for that reason I never really became reconciled to Bloomington. The spirit of encouragement and support by Indiana University was superb, however, and is gratefully recalled.

In May 1947, upon the occasion of a visit to Yerkes Observatory, the newly appointed director of McDonald, Gerard P Kuiper, told me that from then on, cooperation of the type that Indiana University had enjoyed would be no more. If I wanted to work on some problem with a Yerkes astronomer on a private collaboration, that would be acceptable, but thenceforth there would be no independent research at McDonald by outsiders. Two weeks after I had returned home, I received a letter datelined Yerkes to the effect that if I would desert the Hoosiers and work as a hack lecturer and glorified teaching assistant for 10 months a year in Hutchings' program in the College of the University of Chicago, observing time at McDonald sometime during the two remaining months would be assured me. In the ensuing day-long interview in Chicago, nobody asked me a single question about my research interests. Their controversial educational program was inspired by the so-called Great Books of the Western World. I never realized how dull and turgid so much of this material was until an occasion when I was in Turkey with nothing else to read. The scientific selections contain some especially poor writing. I happened to be at the College the day the physical sciences' topic was the characteristics of fluids. What seemed to be the most important aspect of the subject was what Archimedes thought of fluid properties. Soon after my return to Bloomington, two letters arrived almost simultaneously. The one from the University of Chicago offered me a nontenured and therefore purely temporary job as a menial in the College. The other, from my old classmate Leo Goldberg, proposed that I come to the University of Michigan to participate in the development of a graduate program in astrophysics somewhat along the lines we had both known at Harvard. Would to God that all my important decisions in life were as obvious as this one!

Michigan was an inspiring and exciting place in the late 1940s and thereafter. Leo, who had been a staff member at McMath Hulbert Observatory, became director and chief of the astronomy department in Ann Arbor and immediately set out to develop a modern research and graduate studies program. At that time, the University of Michigan had three observatories: the original at Ann Arbor with its venerable 37.5-inch reflector, the modern McMath Hulbert Observatory, and the Lamont-Hussey Observatory in South Africa devoted to double star work. Robert McMath, a highly successful businessman, and his father had turned a small amateur enterprise into a remarkable observatory. Attention was originally directed to solar phenomenology, but with the advent of the lead sulfide cell in the late 1940s and the vacuum spectrograph in the 1950s, emphasis was placed on high-dispersion solar spectroscopy. Leo, Edith Müller, and I undertook a quantitative analysis of the solar atmosphere. Later, after I moved to UCLA, I continued to pursue this problem. Using a method of spectrum synthesis developed at UCLA by John Ross on spectra made with equipment designed and built by Orren Mohler and operated by Walter Mitchell at the Snow Telescope on Mt. Wilson, several students and I undertook a study of abundances of various metals in the solar atmosphere. The most interesting were the assays we ran for Os, Ir, Pt, Ag, and Au. This brought back shades of my experience in the mining camp: still searching for gold, but this time in the sun! The enthusiasm of Harold Urey provided great inspiration for our solar composition work.

The most rewarding aspect of my Michigan sojourn was the opportunity to work with enthusiastic, eager graduate students. It was a time of great excitement in our scientific field. Radio astronomy was coming on line, instruments carried in rockets probed the ultraviolet solar spectrum, and the stellar evolution story was beginning to unfold. Bill Liller, Joe Chamberlain, Jun Jugaku, Karl Henize, Lowell Doherty, Helene Dickel, Ted Stecher, Anne Cowley, Jean McDonald, and Albert and Nancy Boggess are among those with whom I worked most closely. Bill Liller later became a staff member at Michigan. After he completed his thesis, we worked together at Mt. Wilson on planetary nebulae with a scanner that he had built. With Joe Chamberlain, Jun Jugaku, and a research associate, Gunther Elste, I worked on Mt. Wilson Coudé spectra, via model atmosphere methods.

My research and graduate teaching, not only at Michigan but also for a year as a visiting professor at the University of Toronto (1961–1962), was greatly helped by the fact that from the late 1940s to the early 1960s, the Mt. Wilson Observatory, thanks to its guest investigator program, largely functioned as a de facto national observatory. Thanks to the foresight and generosity of Ira S Bowen and his successors, many of us cloud-bound midwestern and eastern astronomers had a chance to carry out research with top-notch equipment. Telescope time was at a premium to us visitors; hence, there seemed to be some advantage in concentrating on Coudé spectroscopy, where the ratio of assessment to observing time was high. The arrangement seemed too good to last and it was. By 1959, as a consequence of increased demand by Mt. Wilson astronomers and Caltech staff and students for time on their own telescopes, allocations to guest investigators, although yet good, could not be quite as generous as they had been previously.

In 1960, Goldberg quit Michigan to accept an appointment at Harvard, while I took a sabbatical in Australia, where I was able to use Liller's spectrum scanner on southern objects. My heart had long been set on going to the southern hemisphere; the experience was very rewarding. Don Faulkner, now of the Mt. Stromlo and Siding Springs Observatory, and I measured spectral energy distributions in bright stars, the spectra of H II regions in the Magellanic Clouds, and spectra of Eta Carinae and of several planetary nebulae. With the new Coudé spectrograph, Ted Dunham and I observed Eta Carinae. A big personal decision was awaiting me upon my return to Michigan.

Beginning in the mid 1920s, Michigan astronomers had cherished the dream of getting a telescope in the two-meter range. Their hopes had been dashed by the Depression, but Director Heber D Curtis did succeed in getting a 230-cm disk. Before I arrived, Robert McMath had contrived to arrange funding for a duplicate of the 60-cm Case Schmidt, which was to be placed in an observing site near Portage Lake, about 17 miles from Ann Arbor. In return, the McGregor Foundation, which had financed the 60-cm Schmidt, acquired title to the 230-cm disk. McMath then connived for it to be given to the British for their Isaac Newton Telescope, which was to be built in the south of England, a site hardly any better than that at Portage Lake. By getting rid of the disk he hoped that agitation by Michigan astronomers for a large telescope of their own at a good site could be silenced; indeed, it was for a time, as far as the Michigan administration was concerned.

In the late 1950s, as momentum gathered for a national observatory, the argument was made that such a facility would take care of *all* the needs of US astronomers who did not have access to large telescopes in a good location. At least until he went to Harvard and took up the cudgel for their proposed 1.9-m telescope in Chile, Leo Goldberg emphatically expounded the argument that Kitt Peak Observatory and eventually Cerro Tololo would remove the need for places like Michigan to have their own "large" telescopes. The idea appealed mightily to administrators: They would not have to put up a cent. This was a matter on which I was in complete disagreement with Leo and McMath. I felt that in order to have a viable competitive program, an astronomy department must have guaranteed access to a decent telescope, either by sole ownership or by a consortium, such as the present Michigan-MIT-Dartmouth arrangement.

In the spring of 1960, shortly after Leo had announced he was going to Harvard, Fred Haddock and I went up to see the University of Michigan's vice president, Marvin Niehuss. I tried to tell him that I could not do 1960 astronomy with a pre–World War I homemade telescope. "What are you people complaining about? You have a brand new building, haven't you? If you all leave we will replace you." Thus, the time had come to move on!

Shortly after my cheery interview with Niehuss, I went to Australia on my sabbatical. One cloudy evening while I was at the observing station at Mt. Bingar some hundreds of miles west of Canberra, my wife, Rosalind, phoned to read me a letter from Dan Popper asking if I would like to come to UCLA. "Don't accept by return mail," she admonished. "Check out the deal!"

The UCLA astronomy department then was small. Besides Daniel Popper, there were George Abell, one temporary appointee, and Samuel Herrick, a space navigation expert. Dean Blacet suggested that the department's survival would be in jeopardy unless a PhD program was developed. A committee including distinguished academics from other institutions was appointed to consider the matter. They reported favorably on the idea but a complication developed. Influential Berkeley academics bitterly opposed the proposal. In their view, Berkeley should be the science campus, and UCLA should be content as the campus for poets, artists, theater people, etc. This arrogance was resented; the approval of a PhD program in astronomy became sort of a cause célèbre. Any UCLA offer to me hinged on the outcome. The dispute finally reached the office of the University of California president, Clark Kerr, for resolution. Dan Popper was delegated to argue UCLA's case in person. President Kerr listened carefully to the arguments and gave Dan his blessing in the late summer of 1961.

On October 17, 1961, I accepted the appointment at UCLA and arrived for duty in September 1962. Now, for the first time in my life, I was at a university that had first-class equipment of its own where I would not be a victim of the ignorance of lunkheaded referees. For example, in response to my request for Cerro Tololo time to continue spectroscopic work on 30 Doradus, which I had initiated at Mt. Stromlo in 1960, I was told to work on an equivalent object in the northern skies!

In collaboration with a number of graduate students, I worked on high-dispersion solar spectroscopy and abundances as noted above; on Coudé spectroscopy of B stars and Apec stars; and also on spectra of planetary nebulae, with the prime focus spectrograph on the 3-m at Lick, with the Lallemand electronic camera in collaboration with Merle F Walker, later with the image tube scanner, and finally with the Hamilton echelle spectrograph.

It has been a pleasure to work with my UC colleagues Merle Walker, Harland Epps, and Ben Zuckerman; and also with Lindsey Smith, Donald J Faulkner, Bernie Mills, and Douglas Milne from Australia; and especially for many years with Stanley Czyzak and his associates from Ohio, Gordon Wares of the Air-force Cambridge Lab, and Walter Feibelman from NASA Goddard (in the context of *IUE* observations). Particularly rewarding has been the opportunity to work with bright, eager, promising graduate students, including John P Oliver, Paul Etzel, Ed Krupp, Ron Polidan, Ed Rhodes, Steve Drake, Dana Kerola, and especially William H Clarke, Jim O'Mara, Steve Little, John Ross, Jim Kaler, Geraldine Peters, Mike Dworetsky, Sarah Heap, CD Keyes, George Jacoby, Karen Kwitter, L Likkel, and Siek Hyung.

Any idea that I might have had that Marvin Niehuss represented the nadir of academic administrators was dispelled after my arrival at the University of California. In the mid-1960s, some of our superiors proposed that astronomy, meteorology, space physics, and geology/geophysics all be combined into one polyglot superdepartment. As would be obvious to any freshman aspiring to become an astronomer, such a conglomerate of unrelated fields would ruin any prospect of developing a viable astronomy graduate program. What student, looking over prospective graduate opportunities, would choose such a muddled and confused departmental setup? My colleagues in meteorology, geology/geophysics, etc were as dismayed as I was. Beyond elementary physics and mathematics, the curricula did not overlap at all. Although this nonsensical proposal percolated for years, no such serious attempt was made to wreck our program.

There were other deplorable policies. One cynical, blatant scheme of age discrimination targeted a number of us. Congress had passed a law decreeing that nobody could be discharged from a job solely on the basis of age, up to 70, with one glaring exception: Academics—and only academics—born before July 1, 1915 could be purged anytime after the age of 65 at the whim and caprice of their superiors. A number of us prospective victims organized an informal group to remonstrate against what we perceived as an injustice. We contacted colleagues on other campuses and received much sympathetic support. The views of us dissidents were never allowed to reach the Regents of the University. Some concessions, such as a phased retirement, were made to those who could meet certain strict requirements. In comparison with golden handshakes given recent retirees under what is called the VERIP (Very Early Retirement Incentive Program), these sops were meager indeed. Be it noted that these age discrimination policies, aimed at elderly professors near the top of dollar earnings (which often poorly reflected inflation) were made *not* at a time of recession in California, but at the apex of arms race–induced prosperity. On the other hand, UCLA administrators and Lick staff have been very considerate of the aspirations of those who are formally retired but who yet wish to pursue active scientific work.

I believe that at UCLA we have succeeded in building up an excellent program in astronomical study and research, offering students a variety of superb opportunities. The quality of the academic staff is a tribute to the sagacity and wisdom of my colleagues. They succeeded in attracting excellent people and in building up impressive programs in a number of fields. Endeavors include not only "classical" topics such as stellar masses, luminosities, and temperatures; binary stars; and stellar structure and evolution (Dan Popper and Mirek Plavec); but also some new twists—the origins of binary stars and solar systems, and brown dwarfs (Andrea Ghez, Ben Zuckerman, and Eric Becklin). In some aspects of these problems we have close association with the geophysics and planetary physics group, e.g. in the study of meteorites (John Wasson).

Physics and chemistry of the interstellar medium are studied by Mike Jura and his associates, the relatively quiet center of our own Galaxy is the special province of Mark Morris, while Matt Malkan specializes in the dramatic events occurring in active galactic nuclei. Solar and stellar oscillations are explored by Roger Ulrich, a field in which he is one of the most distinguished pioneers. Investigations in the radio frequency range as a tool for studying the interstellar medium in other galaxies (Jean Turner), the far IR as measured by *COBE* (Ned Wright), and the near IR (Ian McLean and Becklin) give our students an opportunity to appreciate the importance of other spectral domains in modern astronomy. Also, in studying the physics of cosmic plasmas (Ferd Coroniti), they have a chance to see how basic physics applies to a huge range of important astrophysical problems.

The large-scale structure of the universe and cosmology are at present very active fields of astronomical endeavor. Studies of the cosmic microwave background with *COBE* (Wright) are supplemented by insights provided by near-IR imagery and spectroscopy, and by experiments directed to the search for dark matter and the astrophysical role of neutrinos (David Cline).

A first-rate program in astronomy and astrophysics should expose students to a number of challenging opportunities that can stimulate their imaginations and induce them to take an active part in a research field that they find particularly exciting and rewarding. This was less true in the days of my youth. Astronomy was fun but physics was more so and when you applied physics to astronomy it was the most fun of all. The Berkeley curriculum of the 1930s with its emphasis on practical astronomy, interpolation in tables, and least squares with illustrations all drawn from surveying had more in common with my daughter's courses in civil engineering than with what is being taught nowadays in astronomy.

Thirty years ago at the 1964 Hamburg meeting of the International Astronomical Union, a speaker at one of the invited discourses said he had no sympathy for astronomers who continued to work through the murky atmosphere instead of responding to the challenge of the space age. Indeed, the *International Ultraviolet Explorer;* the *Hubble Space Telescope;* and cryogenic satellites operating in the X-ray, gamma-ray, and infrared wavelengths are opening vast new vistas. Those who struggle with the murky atmosphere have not been idle as evident by the new-technology telescopes, with their large apertures, sophisticated instrumentation, and adaptive optics. All of this, together with powerful computing techniques and sophisticated theoretical insights, have produced an exponential growth in our science. The astronomer's workhorse of the late 19th and most of the 20th century was the photographic plate, which is burdened by the double curse of low sensitivity and nonlinearity as a detector. Devices such as the image tube, the Lallemand electronic camera, the "Boksenberg Box," the image tube scanner, and finally the CCD detector together with infrared arrays have opened up vast new fields for exploration. To one whose astronomical training started with making eye and ear observations of star transits, the transformation has been like stepping into a Star Trek adventure. We are truly in the Golden Age of investigation of the Universe.

Technological advances offer wonderful opportunities for the coming years. In addition to new space telescopes, the new generation of large reflectors equipped with adaptive optics will produce high angular resolution and high signal/noise ratios for faint objects, thus enabling us to probe more closely to the edge of the observable universe, obtain high dispersion photometry and spectra of faint sources, and dredge up wonders of which we have not yet dreamed.

Great advances in computers and servomechanisms have made possible not only such wonders as the Keck Telescope and adaptive optics but have provided a terrific boost to theoretical studies ranging from the history and stability of the solar system to stellar and galactic evolution. Can we expect this progress to continue at anything like the present rate?

After 1945, many scientific projects were funded as a consequence of the armaments race and later the space race, and often the military asked astronomers to serve them in their various schemes. Would I please give up what I was doing and help them build a Hell bomb? Recalling the cynical opportunism I had witnessed at the Radiation Laboratory in Berkeley and especially the swift kick in the face as the abiding reward for faithful service, my first reaction was to snicker. But I realized there was nothing funny about the situation. Contrary to their current propaganda, the greatest threat was not a nuclear attack and assault on the West by a ferocious Red Army but the mere existence of these weapons themselves. Sooner or later, these devices will fall into the hands of religious or political fanatics who will not exercise the restraint of a fearful, decaying Potemkin Empire. What wonders what might have happened had nuclear bombs been available to terrorists such as those who bombed the World Trade Center! I firmly believed in the 1950s and I firmly believe now that efforts expended on the developments of such weapons are efforts directed to global genocide to hasten the extinction of the human race.

Even now, with Bolshevism consigned to the scrap pile of history, excuses are found for militaristic extravaganzas. Consider the scuttling of the Superconducting Supercollider as "too expensive," while yet one additional much-more expensive aircraft carrier is funded, because to do otherwise would cost jobs! Of course, unlike the military, we cannot have all the really high priority items we would like, but it will be a tragedy if basic support for new advances is not forthcoming.

One problem is that we have done a miserable job of raising the level of scientific literacy in this country. Not only the vast bulk of the public, but also elected leaders are woefully lacking in understanding basic principles of science and engineering that underlie our increasingly technological civilization. It is like having a fishing village ruled by shepherds. Support for science must come from the public, and the public must appreciate what they are paying for. Unfortunately, news reporters who cover scientific developments and policies are often incompetent. The propaganda of hired hacks of polluters and anti-environmental groups and their sometimes equally badly informed opponents are often treated with the same or even more respect than the considered evaluations by Nobel laureates—like giving salves for cancer treatment equal status to genetic insight based on studies of the human genome.

Given the thrill and excitement of modern research, it is no wonder that vast numbers of capable young people are attracted into astronomy and other natural sciences where they could make tremendous contributions of lasting benefit to all of mankind if they were given the opportunity. What are their chances given the present atmosphere of government support? I am not optimistic. After their degrees, many excellent people obtain one or two postdoctoral appointments, are unable to find tenure-track or permanent employment in astronomy, and drop out. The academic world has always been competitive, but now it is overcrowded and overcompetitive. It is likely that most of the people who get PhDs in astronomy at this epoch will not be able to find truly satisfactory work in this field. This argument was presented to me by my elders two thirds of a century ago. My reply now—as it was then—is that what you have to know to do astronomy, particularly mathematics, physics, and now computers, would qualify you to do many other things. Why not enjoy doing what you like to do for a few years, even if afterwards you have to turn to something else? The situation is not unique to astronomy. Our colleagues in physics and mathematics suffer similar employment problems. Since the greatest satisfaction and accomplishment in my scientific career has been in attracting, mesmerizing, wheedling, persuading, and—above all—teaching young folks to have an interest in astronomy, I feel particularly sad about this situation.

Finally, I would like to express my gratitude to the community and the many folks who have helped me in my career and work, from H Zanstra who sent me star charts and words of encouragement in 1928 to my loyal Korean associate Siek Hyung. A realization of my good fortune is all the more appreciated when I consider the misery, poverty, and frustration of the existence my worthless old man had tried to force on me, and how chillingly close to success he had come.