An Astronomical Life Salted by Pure Chance

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Abstract

My childhood upbringing in no way suggested that I would become an astronomer, but accidents of fate pushed me in the direction of science, and I have benefited greatly from being in the right place at the right time. I grew up in Seattle, earned B.S. and M.S. degrees in mathematics at the University of Washington, and eventually a Ph.D. in astronomy from the University of California, Berkeley. I was a postdoc at the Mt. Wilson Observatory, an assistant professor at Indiana University, later the Yerkes Observatory (University of Chicago), and still later I became a staff member of the Mt. Wilson and Palomar Observatories. After several years, I returned to the University of California, this time with the Lick Observatory staff at its new academic home on the Santa Cruz campus, where I have been ever since. My research has focused on the relation of Cepheids and RR Lyrae stars to problems of Galactic structure, the binary nature of cataclysmic variables, the decay of angular momentum of solar type stars, and the chemical history of the Galaxy as revealed by the abundances of very old stars in globular clusters and the Galactic halo field. None of this work would have been possible without the help of excellent teachers and mentors, great colleagues, and superb postdocs and graduate students. Most of all, I am grateful for the educational opportunities afforded me by state-supported public Universities.

"....it is not by merit we rise or we fall

but the favor of fortune that governs us all."

Tom Rakewell, Act 1 of "The Rake's Progress," an opera with music

by Igor Stravinsky and libretto by W.H. Auden & Chester Kallman

We ride on a sea of fate, and what happens to each of us cannot be accurately predicted. My own case is no exception. From the time I was three years old, my mother prepared me for life as a musician or an actor: She could not possibly have imagined such a thing as life in academe, much less astronomy. Perhaps it is really the case that I have only played the role of an astronomer, but that is for others to decide. On the whole, the sea of fate has been extraordinarily kind, tossing me in the right direction most of the time. Paraphrasing the immortal words attributed to Yogi Berra: when I came to a fork in the road, I took it. It was always the right fork.

I. FAMILY BACKGROUND AND CHILDHOOD

I came from families of farmers and workers; indeed, the only professional person in my background was my maternal grandfather, a steam-ship engineer. My father, Victor Paul Kraft, descended from German peasants who emigrated in the 1840s to Bethel, Missouri as members of a Protestant religious group numbering a few hundred. Indeed, Bethel is preserved today as a State Park, as a tribute to the kinds of buildings, light industry, and kitchen and farming equipment used by many groups of this kind that came to the middle west in those years. My father was born into the Bethel community in 1893, the oldest child of 10 that survived. Because the next four siblings were all girls (and therefore of no value as far as the old man was concerned!), my father was virtual chattel, working on the farm from a very young age, first in Missouri and then in Kansas, and finally in 1911, the entire family moved to a farm near Turlock in the central valley of California. Dad's education did not extend beyond the sixth grade, but his horizons were broadened by the journey to France during WWI, where he served in the U.S. Army Air Service and learned to repair airplanes. When he returned home to the farm, the French experience oiled his escape: He and the next oldest brother took off for Southern California to take up the wonders of the new "motor trade," i.e., auto repair, and it was in Los Angeles that he met my mother.

My mother's people were Irish, the Ellis's and McCormicks, both families having come to the New World escaping the Irish potato famine of 1847. All were peasants, the main difference being that grandpa was green (Catholic) and grandma was orange (Protestant). Grandpa Ellis was born in Ontario, homesteaded in Nebraska, farmed in Washington, went to sea, studied engineering (self-taught) and eventually wound up as chief engineer of the *Baranoff*, the largest vessel of the Alaska Steamship company. Grandma McCormick, was a southerner born in Texas of railroad people. How she and Ellis met is a mystery, but they eventually settled in Seattle in 1889, the year of the Great Seattle Fire. My mother, Viola Eunice Ellis, the last of four children, was born in 1907. At sea for three weeks a month, grandpa yearned for peace and quiet at home and the charms of a two-year-old daughter as opposed to the troubles of her teenage siblings. As sometimes happens in families, mother was viewed by the adults as the favored child, and perhaps as a result, she became a bit of a handful in her teenage years, so much so that grandma sent her to live with her older sister, by that time married and settled in Los Angeles. It was there that she met my father and they were married in 1926. I came along on June 16, 1927.

They had by then moved to Seattle and Dad secured employment with the Ford agency as an automobile mechanic. The stock market collapse in 1929 and the Great Depression that followed found them remaining in Seattle where Dad's job was at least steady even as the pay was niggardly—unions, strikes, and the concept of hourly pay did not come on the scene until 1936. My parents bought and then lost a home, but at last secured a small lot carved out of grandfather Ellis's five-acre tract on the outskirts of Seattle on which they built a very small home, mine too until I left Seattle in 1949. Thus I grew up under the influence of my mother's Irish family, and knew almost nothing of Dad's people in California. But this turn of events represented my first favorable encounter with fate: Because grandma was orange and grandpa green, they must have agreed that religion was "out of bounds." I grew up in an unchurched home and was never forced to subscribe to a religious creed. Thus did I become a follower of the Enlightenment, long before I was aware of the philosophy behind that historical movement. Dad's personality was shy, retiring, and introverted, but given to quick bursts of anger expressed in words but not physical acts. My mother, who did not graduate from high school, was extroverted and full of romantic notions fostered by movies of the period. She was 15 years younger than he and they did not get along. I was an only child, which has both its good and bad points, but she especially doted on me, perhaps making up for the lack of compatibility in the marriage. It was the era of child movie stars like Shirley Temple, and my mother fondly imagined that I was destined for such a career! I played the guitar, banjo, and piano from the age of four onward and appeared on stage and local radio. I sang, took elocution lessons, appeared in plays and musicals in grade school and high school. Classical piano lessons were taken on and off; I mastered the first movement of Beethoven's Op. 13.

But there were subversive signs. Naturally I was a top student in high school and grade school and therefore despised by most of the other kids. The first signs of my serious nearsightedness were detected when I was a third grader, and that, together with general ineptitude, ruled out serious participation in sports, which is what other boys considered important. But on the academic side I took a serious interest in mathematics even in grade school. Owing to transportation issues arising during World War II (WWII), I was switched from Lincoln High School to Roosevelt High School in the middle of my junior year, again a positive stroke of fate, because Roosevelt had courses in trigonometry and even calculus, which was unusual in those days (1943). Thus did I leave behind any interest in theater and headed into mathematics (but not yet science). On top of that, I joined a "record club" of "intellectual" Roosevelt High students, about 10 in number, who were interested in classical music. We collected the 78-rpm records of the period, and I became familiar not only with the music of Mozart, Beethoven, and Sibelius but experienced a new wider world populated by the London Philharmonic, Boston Symphony, and Vienna Philharmonic; conductors such as Beecham, Toscanini, Koussevitzky, Walter, and Weingartner; the pianists Schnabel and Gieseking; the violinist Szigeti; and the Budapest String Quartet. Worldwide vistas opened and have affected me ever since.

II. COLLEGE YEARS, FIRST TEACHING POSTS, AND MY INTRODUCTION TO ASTRONOMY

I enrolled in the University of Washington (UW) in 1944, intending to enter the school of education to become a public school teacher, but an advisor persuaded me to enter the College of Arts and Sciences as a math major; I obtained a B.S. in 1947. I missed WWII. My physical exam for the military occurred within a few weeks of the atomic bombing of Hiroshima and the Japanese surrender; the authorities must have decided they didn't need someone with my limited eyesight and I was classified 4F.

Fortunately, I lived in Seattle and a respectable public university was part of my hometown: This was an advantage for someone with limited financial resources. I lived at home; this was good for the pocketbook, but not so good for one's social development. I did indeed notice the girls but I was too studious and shy to date and that part of my life was delayed until my years as a graduate student. But in my very first college year, I had an introductory exposure to astronomy, and that too was entirely by accident. Astronomy was taught by one man, Theodor Jacobsen, who was actually a professor in the math department. His main duty during the WWII years was teaching navigation and astronomy to navy officer recruits as part of the V12 program. He was a University of California (UC) Ph.D., having received his degree in 1928 with a Lick Observatory thesis on the radial velocities of Cepheids. As it happened, he taught the College Algebra course to which I was assigned, and thus I had my first glimpse of the small UW Observatory building, classroom, and the 6-inch refractor housed in its dome. There was also a transit instrument under a roll-off roof. My interest in astronomy was awakened and I started taking courses from Jacobsen (e.g., practical astronomy, stellar motions, radial velocities, celestial mechanics, the Talcott latitude method using the transit instrument) and studying such classical books as Moulton's "Celestial Mechanics," Smart's "Spherical Astronomy," and the venerable astrophysics text of Russell, Dugan, and Stewart. The undergraduate physics courses, which veered heavily into rather practical semiengineering matters (recall the country was coming out of a wartime mentality) was full of levers, pulleys, and inclined planes, which held little interest for me. Practical astronomy seemed far more exciting.

In 1947, I entered the math graduate program, taking courses mostly in modern algebra, number theory, matrices and determinants, complex variables, differential equations, and projective geometry, but unfortunately for my later career, I skipped statistics, vector analysis, and the stuff of applied mathematics. I became a teaching fellow in math, in some ways an embarrassment because I became the typical smart-ass 20-year-old teaching algebra and calculus to seasoned 30-year-old veterans of WWII battles, giving the occasional too-hard exams. Perhaps the most important lesson to be learned was coming to grips at last with fellow math graduate students who were as smart or smarter than I, a new and valuable lesson of life. It was hard work for once to keep up. But my studies with Jacobsen continued as I prepared an M.S. thesis on the level effect in the radial velocities of classical Cepheids and was awarded an M.S. degree in math from UW in 1949.

A friendship with Jacobsen began to develop in recognition of common interests in classical music and hiking in the Cascades. Jacobsen was an excellent pianist and was willing to listen as I struggled through the first movements of Beethoven's Opp. 90, 101, and 110—all fairly easy. His tastes ran more toward the more "pianistic" early romantics, e.g., Chopin. We took many one-day trips together to the flanks of Mt. Rainier—Indian Henry, Mt. Ruth, Panhandle Gap, Camp Muir—culminating finally in the successful ascent of Mt. Hood in Oregon. On the astronomy front, he encouraged me to become a member of the Astronomical Society of the Pacific (ASP). My first copy of the *Publications of the Astronomical Society of the Pacific* (PASP) was that published December 1946, in which the formation of the joint authority known as the Mt. Wilson and Palomar Observatories, under I.S. Bowen's directorship was announced. I also attended the dedication of the Palomar 200-inch telescope in the summer of 1948. But perhaps the most significant outcome of the relationship with Jacobsen was his sponsorship of my visit to the Dominion Astrophysical Observatory (DAO) in the summer of 1948, where I served as a summer assistant to R.M. Petrie. This was my first experience at a real observatory. I measured radial velocities from medium-scale prismatic spectra of B-type stars obtained with the 72-inch reflector, as part of the Victoria study of differential Galactic rotation. From this came my first published paper (with Jacobsen) on the orbit of a B-type spectroscopic binary. I could not have imagined at the time that the Victoria experience would ultimately determine the outcome of my life as an astronomer.

But other even more profound changes were on the horizon. My social life broadened when I went on skiing trips, attended dances, and then finally met Rosalie Ann Reichmuth at a student function in the University Unitarian Church. Rosalie was a Journalism major, receiving her B.A. in 1948. We were married in Billings, MT, her hometown, on August 28,1949. In a wonderful marriage that has lasted for 59 years, she has been a constant source of support, both practical and emotional.

III. WHITTIER COLLEGE, AND MY AWAKENING OF INTEREST IN PHYSICS

In Seattle I had already been interviewed and had accepted a teaching position at Whittier College in Southern California, and so after our marriage in Billings, we headed south through the Rockies. Again this job was another stroke of good luck for me. Whittier boasted an amateur-built but perfectly respectable 18-inch Newtonian focus reflector, and the College needed someone to teach astronomy in their required integrated physical science course. Randolph Pyle, the resident math professor, taught in the field of analysis, and I filled a gap for math majors in the field of modern algebra: It was a nearly perfect fit. With an enrollment of about 2000 undergraduate students, Whittier was basically a Quaker college, a sort of California edition of the renowned Earlham College, a Quaker institution in eastern Indiana. Richard Nixon had been a student at Whittier, and when we arrived he was campaigning against Helen Gahaghan Douglas in an attempt to unseat her from Congress. It was Nixon at his red-baiting best near the beginning of the McCarthy era in American politics. Rosalie and I were importuned to support his campaign. We refused, despite the enthusiasm of some of my colleagues, thus following the lead of the physics professor, a traditional Quaker, who would have nothing to do with Nixon and what he stood for.

I worked with a Pasadena engineer to construct a photoelectric photometer for the telescope, using a 1P21 photocathode, following a recipe in *Sky and Telescope*; this was completed only shortly before we left for Berkeley. But observations in Whittier continued over the summer of 1952 with the help of a physics student, Fred McClung, who was knowledgeable in the field of electronics. I was able to get a light curve for the β CMa variable with the largest light variation: BW Vul (Kraft 1953). This was my first real observing experience.

But teaching the physical science course awakened my interest in theoretical physics, a domain of which I was woefully ignorant. It appealed to me because of its close connection with mathematics, and I became at last aware of the basic philosophy of science, namely how the truth of a theory was tested by experimentation. The idea that, in astronomy, one can build a model and then test out the idea in the world of observation was an intoxicating notion, and I knew, with the fervor that only a young person can summon, that this was the life for me. Perhaps in physics? No, I knew too little to embrace this field. But perhaps astronomy: I could catch up in the areas of physics I needed to know. In my naiveté, I went to see Jesse Greenstein, who had been appointed two years earlier to head the new Caltech astrophysics program paralleling the opening of the 200-inch telescope. He listened politely to the story of my background, but rightly concluded that Caltech was too advanced, especially in physics, for me and advised that I go elsewhere. In later years, after I had become an adjunct professor at Caltech in virtue of my appointment to the Mt. Wilson staff, we often joked about this: I was too poor to enter the Caltech program but later was teaching topics in stellar spectroscopy to Caltech graduate students. The world is full of amusing ironies.

But what was I to do? With the birth of my older son Kenneth on July 10, 1950, I was now a family man, and financial support was thin. I applied and was admitted to the astronomy graduate program in Berkeley in September 1951.

IV. BERKELEY, STRUVE, AND MY INTRODUCTION TO RESEARCH ASTRONOMY

Financial support from the Berkeley astronomy department was not forthcoming, but we did arrive with $2000 in savings and a $1000 Jake Gimbel Scholarship loan administered by the Bank of America. We succeeded in obtaining an inexpensive apartment in the Albany UC Veterans Village, which by this time was open to those who had not served in WWII. Nearby was a state-run nursery school that later our two-year-old son could attend, and this allowed Rosalie to work in the University library. However, these windfalls were dwarfed by another amazing stroke of luck: The woman who worked for Otto Struve (I do not now remember her name), measuring radial velocities of β CMa stars, unexpectedly quit. My time as a summer assistant at DAO now paid off—these spectra, taken with the 60-inch Mt. Wilson reflector, were nearly identical in dispersion (prismatic) and scale with the spectra I had measured in Victoria. Moreover, both sets were B-type stars. Struve turned to me and I had a job!

But it was more than just a job. It provided the opportunity to work with Otto Struve, the world's foremost stellar spectroscopist, to see how he carried out research, to follow his example, and to witness his hard work and dedication. I was part of a team that turned out several papers on β CMa stars (e.g., Struve et al. 1952). The department by that time had shed its old emphasis on solar system mechanics and orbit calculations and had undergone a transformation with the importation of astrophysics faculty from Yerkes Observatory: Louis Henyey and John Phillips as well as Struve. Of great help to me was Henyey's one-year course in physical foundations of astrophysics, where I learned classical mechanics, statistical mechanics, thermodynamics, electricity, and magnetism. Phillips taught atomic and molecular spectroscopy, and in the physics department there was an inspiring course in atomic physics using Max Born's classical book with its valuable appendices. I did not learn much about Galactic structure and dynamics, or cosmology, but I did learn about statistical methods from Harold Weaver using the well-known Trumpler & Weaver text that had just been published.

Instructive, I think, is a thumbnail sketch of the state of astronomy in 1951. The field was still dominated by the optical regime and the study of stars paramount. Radio astronomy, much of it carried out by those who had pioneered the development of radar in WWII, was still in its infancy, with studies of 21-cm emission getting underway in Holland and Australia. Rockets permitted a look at the solar UV spectrum, but astronomy from space was barely a dream. Walter Baade was yet to announce the concept of the two stellar populations although the differing morphology of the Hertzsprung-Russell (HR) diagrams of open Milky Way versus globular clusters was known from rather crude photographic photometry. Photoelectric photometry was getting underway with 1P21 photocathodes, but the *UBV* system and its tie to Morgan spectral types was still two-years downstream. Real quantitative measures of interstellar reddening of B-type stars and therefore absorption and its effect on the cosmic distance scale were just around the corner. The zero-point of the Cepheid period versus luminosity law was too faint by about 1.5 mag. The first calculations of the evolution of dwarfs into giants would come in 1953, and although energy generation via the CNO cycle was known from Hans Bethe's work as early as 1938, recognition of the importance of the proton-proton chain was just beginning. It was generally believed that all stars had the same chemical composition, the differences in spectral type and luminosity being explicable entirely as a result of the Boltzmann and Saha equations. The largest telescope in the world was the 200-inch at Palomar, followed by the Mt. Wilson 100-inch, the 82-inch at McDonald, some 72-inch reflectors in Canada, Australia, and South Africa, and finally the 60-inch again at Mt. Wilson. The 120-inch Lick reflector was now funded and in the planning stages, but would not be operational for nearly a decade. There was no National Science Foundation and no NASA, and the idea of a national large telescope accessible to all was an urgently needed yet unrealized dream.

Into such a state of knowledge we came as graduate students to Berkeley. Contemporaneous with me were Roger Lynds and Beverly Turner, known as "BD," who later were married, and who eventually took up positions at Kitt Peak; and Bill and Amelia Wehlau, who later went to the University of Western Ontario. Merle Walker was appointed a Carnegie Fellow in Pasadena after receiving his Ph.D. in 1953. Most of all, I was influenced by John (Jack) Crawford who came with an M.S. from the University of Chicago. He was a real physicist who had worked in the Manhattan Project; I learned a lot of valuable physics from him. About then, Hermann Bondi, a colleague of Fred Hoyle's, came as a visitor and lectured on the possibility that stars could accrete material from the interstellar medium. In the *Astrophysics Journal* appeared a paper by Alfred H. Joy (1954) on a very short-period (17 hr) binary AE Aqr, which showed the spectrum of a G-type dwarf plus a hot emission-line object in which the envelope was thought to be ejected from the hot star in a manner similar to other early-type stars known at that time. Inspired by Bondi and Hoyle, Jack and I proposed a reversed picture: The G-type dwarf was swelling up and spilling matter through the inner Lagrangian point into a ring or disk that was gradually being accreted by a hot white dwarf (Crawford & Kraft 1956). Jack was largely responsible for the physics of the accretion disk; I made new observations of the system using the the Lick 36-inch spectrograph. The latest calculations showed that a dwarf could swell up in response to stellar evolution and thus fill the inner Lagrangian surface; could that not be happening in AE Aqr? As far as I am aware, this was the first time that mass transfer into an accretion disk in a close binary had been invoked.

Earlier, I had had my first real research and solo publication experience thanks to Struve, who had brought back from the Mt. Wilson plate files Alfred H. Joy's beautiful Coudé spectrogram of the 27-year eclipsing binary ε Aur during the eclipse of 1930. The object consisted of a B-type star and a mysterious so-called "I component"; during the grazing eclipse of the B-type star, its light passes behind the outer atmosphere of the I component producing a second set of spectral lines. My task consisted of analyzing these lines to determine the atmospheric pressure and temperature of the I component, thus to see if the continuous opacity predicted matched the depth of the shallow eclipses. This more-or-less worked out (Kraft 1954), and I benefited enormously from suggestions made by Bengt Strömgren, who was visiting Struve at the time, and who kindly listened to my surely amateurish presentation in Struve's office. It is my impression that great figures like Struve and Strömgren always had time to hear you even if you were only a graduate student.

Struve could be harsh in his criticism of science he thought to be slapdash, but he could also be kind and helpful. A man of formidable visage, tall, with a gait and bearing suggesting that of a military officer, he was not given to socializing and glad-handing. But he did succumb once in accepting an invitation to join some of us for coffee at a place just outside the north gate of UC Berkeley. His secretary, Miss Ness, had told us of his pleasure at going to the movies, and on inquiry, we learned that he couldn't stand Barbara Stanwyck. Knowing of his history as a White Russian officer in WWII, I got up enough courage to ask what he thought of the present Soviet leadership (this was 1954!). His was a one-word reply: "Gangsters." As I reflect on it now, I find this word a perfect fit for the Washington administration that came to power in the year 2000.

Struve also took time to obtain a 100-inch Coudé spectrogram of the 16-day Cepheid X Cyg at mid-rising branch of the light curve and turned this material over to me. At these phases, the spectrum of X Cyg develops strong chromospheric Ca II emission. I discovered that many of the absorption lines were doubled at this phase, in a manner similar to that of the Type II Cepheid W Vir, which suggests that there are two atmospheres: an outer optically thin (in the continuum) one through which the observer sees a newly emerging one, the two being separated by a shock wave in which the Ca II arises (Kraft 1956).

I had been awarded a Lick Observatory Fellowship in 1953 and began thesis work on the Ca II emission in classical Cepheids under George Herbig's direction, based on small scale spectrograms obtained with the 2-prism spectrograph attached to the 36-inch refractor. This required many trips to Mt. Hamilton, sometimes even for half-night runs. I spent the summer of 1953 in residence on Mt. Hamilton, separated from my family except for occasional weekends. The Fellowship was a great boon both scientifically and financially: Our second son Kevin was born in Oakland's Kaiser

Hospital on August 26, 1954. The Ph.D. thesis was completed in 1955 and I was awarded the degree. Struve did not think the thesis was up to the standards I had set in my earlier papers, but the text and conclusions were much revised and (I believe) improved before it was published. Struve's comments on the first version were a bitter pill to swallow, but they were an accurate assessment.

In my last year in Berkeley, George Preston arrived from Yale as a new graduate student. We became good friends; he and his family moved into the same low-cost UC housing project that we lived in. He was eventually to have a strong influence on the future course of my scientific life.

V. NATIONAL SCIENCE FOUNDATION POSTDOCTORAL FELLOWSHIP AT MOUNT WILSON

Once again, I benefited from being in the right place at the right time. Not long before I finished at Berkeley, the National Science Foundation (NSF) had been established, and I was awarded an NSF Postdoctoral Fellowship, the first, I think, in astronomy. Of course, one followed one's dream of emigrating to the land of golden opportunity, which in those days meant Pasadena. The Mt. Wilson staff took me in under the rules that governed Carnegie Fellows, and I was assigned an office on the ground floor of 813 Santa Barbara Street, across the hall from Rudolph Minkowski. I would be allowed observing time on the Mt. Wilson 60-inch and 100-inch telescopes with all the usually available observing equipment; Palomar observations were allowed only for regular staff members, not for postdocs.

In addition to Minkowski, I met such figures as Alfred Joy, Olin Wilson, Armin Deutsch, Paul Merrill, Milton Humason, Director I.S. Bowen, Bill Baum, and Allan Sandage. Unfortunately, Walter Baade was on leave that year, so I met him only as he was about to depart and did not benefit from his wise counsel.

I received instruction in the use of the 100-inch Coudé spectrograph from Olin Wilson and the 100-inch Newtonian focus spectrograph from Don Osterbrock, who had joined the Caltech faculty as part of the transplant of the Yerkes style to the west coast, the same migration that brought first Jesse Greenstein and later Art Code and Guido Munch. I started a program of observing cataclysmic variables (novae and dwarf novae—also known as U Gem or SS Cyg stars), hoping to detect radial velocity variations in the manner of AE Aqr. The search was greatly stimulated by Walker's (1954) discovery that Nova Her (DQ Her) (1934) was an eclipsing binary with the incredibly short period of 4 hr 39 min; up to then, DQ Her had been the most widely observed and studied nova in history. There was some similarity spectroscopically between AE Aqr and DQ Her; I thought it possible that all novae and dwarf novae might be structured like AE Aqr, and thus inaugurated a program of obtaining radial velocities for such objects. But short dwell times would be needed if the periods were as short as that of DQ Her; I set a limit of 40 min, and that meant a limiting magnitude of $V = 14$ with the Newtonian focus spectrograph at a dispersion of 200 Å mm^{-1}. This will seem incredibly slow to modern observers, but we were limited by the poor quantum efficiency of photographic plates. A tiny group of these objects was tried, but mostly I was unsuccessful owing to the poor late fall and winter observing conditions, and it was only in the late spring that I collected enough material to find the variations in radial velocity of the hydrogen emission lines in DQ Her, which accompanied the eclipsing binary orbit. These results were published after I had left Mt. Wilson for Indiana (Kraft 1958a).

After one year as NSF Fellow, I needed to find a job. Only a few were available; the one at Case Institute went to Merle Walker, and I was left with a choice between a six-year nonpermanent appointment at Harvard and a one-year assistant professorship at Indiana University (IU). At Indiana, I would serve as a temporary replacement for Frank Edmondson, the chairman, who was on leave of absence as program director for astronomy at the NSF. In those days, one did not apply for an advertised position and send a resume. There was a sort of underground network, in which chairs and observatory directors found out which young people were available from the "good" places, and you were "recommended." Thus you might receive a letter or a phone call asking if you were interested. As a family man, the choice between Harvard for six years, up and out, or one year at Indiana was not immediately obvious. But it happened that Struve and I were on Mt. Wilson at the same time; he at the 60-inch, I at the 100-inch. I remember our conversation in the Monastery just before lunch as Struve sat smoking and enjoying the Monastery's rocking chair. His advice was: "Mr. Kraft, I think you should go to Indiana." Struve knew that Indiana bought three weeks of time per year with the McDonald 82-inch telescope, and assumed that I would get some or maybe all of that. I took his advice, and it turned out to be another fortunate turn of events.

VI. INDIANA UNIVERSITY AND YERKES OBSERVATORY

We arrived in Bloomington after an all-night drive across Illinois, pulling a utility trailer through a typical middle western late summer thunderstorm. This was my introduction to any place east of Montana, but after the flatlands of Kansas and Illinois, the hills of southern Indiana were a welcome sight to a westerner. Here, I had my first formal teaching experience with graduate students as I lectured on stellar spectroscopy and stellar atmospheres. With Edmondson away in Washington, my faculty colleagues were James Cuffey (photometry), John Irwin (stellar motions and galactic structure), and Marshall Wrubel (stellar structure and evolution); I learned a great deal from all of them. Irwin was interested in using Cepheids to map out the spiral arms of the Galaxy and had spent time in South Africa in a systematic photoelectric survey of southern Cepheids. He "rediscovered" the existence of U Sgr in M25 and S Nor in NGC 6087; studies of the color-magnitude diagrams of these clusters later led to a redetermination of the zero-point of the period-luminosity relation based on cluster parallaxes. I joined this effort (Kraft 1957), looking for coincidences in the position of open clusters and Cepheids, following up later with radial velocities to confirm membership (Kraft 1958b).

Wrubel, also a Ph.D. from Yerkes, was a prominent theoretician who also directed the IU Computer Center. From him, I learned how to employ an IBM 650, punched cards and all, to measure line profiles of Cepheids and late-type supergiants in a quest for rotational velocities, as a test of stellar evolutionary tracks. High dispersion Coudé spectra from the 100-inch were in my possession, based on the Mt. Wilson period, but IU had no microphotometer from which paper tracings could be made; I had to make trips to Yerkes. But reductions from photographic blackening to intensity were tedious, and a small army of Indiana graduate students joined me in that effort (Kraft et al. 1959). It was a scientific but also social experience with students not much younger than myself. We also joined together to test and install a small grating spectrograph, designed and built by Edison Petit of the Mt. Wilson staff, at the Newtonian focus of IU's Goethe Link Observatory 36-inch reflector.

I struck up a friendship with a graduate student from South Africa, Don Fernie, who became my first Ph.D. student. Little was directly known about the masses of classical long-period variables, but at least one was a member of a visual binary system, X Oph, and a portion of the orbital arc was known. The companion was an ordinary K giant, which dominated the blue spectral region when the variable was near minimum light. If the absolute magnitude of the K giant could be determined, then the total mass of the system could also be estimated. Fernie and I used the 100-inch Coudé, as visitors, to obtain an excellent spectrum of the K giant, and the resulting thesis (Fernie 1959) suggested that the long period variable had a mass near one solar mass.

In the same run at the 100-inch, I was able to get Coudé spectra of the recurrent nova T CrB, which Roscoe Sanford (1947) had shown was a spectroscopic binary M giant plus hot blue hydrogen emission line star. Except for the very long period of 230 days, this looked to me like a scaled-up version of DQ Her or AE Aqr, so I tried to reconstruct the orbit of the blue star from the Hβ emission line, which itself had to be "reconstructed" on microphotometer tracings. The minimum mass of the suspected white dwarf turned out to exceed the Chandrasekhar limit of 1.4 $M_{\odot}$ (Kraft 1958c). Later work showed that the derived orbital amplitude might well be too large (Paczynski 1965) and that a mass of the blue star less than 1.4 $M_{\odot}$ was entirely plausible.

I did get the benefit of Indiana's purchase of McDonald observing time: I was able to use the 82-inch Coudé spectrograph to measure radial velocities of Cepheids and B-type stars in a couple of galactic clusters, this to confirm the membership of the Cepheids. This was relatively successful and was part of a joint project with Allan Sandage and H.C. Arp, who were responsible for the cluster photometry.

In 1957, Edmondson was offered the directorship of an important observatory out west, but he declined and remained at Indiana as chair of astronomy. But shortly after, the IU administration agreed to make my position, as fifth member of the Department, permanent, so I have to assume this was a concession made in response to the offer to Edmondson. Once again, fate had steered me in the right direction when I followed Struve's advice.

Even so, the seemingly idyllic life of an IU professor of astronomy was not in my future. Half way through my second year in Bloomington I received a letter from Director G.P. Kuiper asking if I would accept an assistant professorship at Yerkes Observatory (University of Chicago). Leaving Bloomington would be hard: IU had good astronomy, time at McDonald, college life, cultural advantages, a superb music faculty. When the Metropolitan Opera made its "western" tour, it came to Bloomington, not Indianapolis, and during 1957, IU was host to concerts by the Vienna Philharmonic and Boston Symphony Orchestras. Was this to be traded for life in a small town of 1000 inhabitants in the middle of nowhere? But Jesse Greenstein had made this move from Harvard in the late 1930s, and Yerkes remained what actors would have called the "big time." The senior faculty consisted of justly famous astronomical figures: G.P. Kuiper, W.W. Morgan, S. Chandrasekhar, and W.A. Hiltner. A Yerkes revival was taking place with the hiring of a distinguished new junior faculty, consisting of such individuals as Geoff and Margaret Burbidge, Helmut Abt, Joe Chamberlain, and Kevin Prendergast. With the prospect of virtually unlimited observing with the 82-inch, then the world's third largest telescope, how could a sane man refuse? Rosalie heard me say: "Rosalie, we have to go." To which she replied, "Of course," knowing she would have to leave this cultural center and the fledgling, if growing Bloomington Unitarian Fellowship. It was a hard, if scientifically sensible, decision.

We moved into the University of Chicago-owned house on Dartmouth Street that Kuiper had occupied before becoming Director. One could walk to the Yerkes offices using the same trail through the woods that Director Frost had used in the 1930s; indeed, the wire that guided Frost, who had become blind, was still to be found alongside the trail. Although we had lost the cultural advantages of Bloomington, we improved our bridge game, made home brew in the basement, and our children had the advantage of the excellent Williams Bay schools. In the winter, we saw the northern lights for the first time and had the experience of shoveling snow out of a north-facing driveway. In December, one had to learn how to negotiate the "polar ice cap" at the Yerkes front door; one night Helmut Abt failed and was rewarded with a broken arm. Such was life in Williams Bay.

With Hiltner's financial help and technical advice, I began a program of interference filter photometry of the G band in Cepheids and late-type supergiants. Cepheids vary in spectral type from F5Ib to about G8Ib around the cycle. The discovery that U Sgr was a member of M25 led to the determination of its intrinsic B-V colors as a function of spectral type. G band photometry, which measured the substantial variation of G-band strength with spectral type, could then be applied to all Cepheids to predict the intrinsic *B-V* color, and thus the value of interstellar reddening and absorption. Because Cepheids lie in the Milky Way and are subject to typically 0.5 to 1.0 mag of *V*-band absorption per kiloparsec, one must know the reddening if one is to have an accurate estimate of distance, the goal being that of using Cepheids to map out the spiral arms of the Galaxy. The program, executed at the 36-inch and 82-inch Cassegrain foci over 1.5 years, proved successful and provided estimates of reddening for more than 38 Cepheids (Kraft 1961). Central to the project were the light and color curves of Bahner, Hiltner & Kraft (1962) and the excellent new photometry of M25 and U Sgr by Joe Wampler and Al Hiltner (Wampler et al. 1961). The derived intrinsic color versus spectral-type relation for supergiants, based on G-band strengths, generally agreed with that derived by Kron & Svolopoulos (1959) based on the intrinsic colors of nearby virtually unreddened supergiants. Astronomers today might be amused, amazed, or perhaps shocked to learn that a major part of the program was executed in a single continuous run at McDonald lasting seven weeks (!), divided between the 36- and 82-inch reflectors.

I received a letter from Jesse Greenstein telling me that he had used the 200-inch nebular spectrograph to obtain spectra of DQ Her during the eclipse phases and asking if I would be interested in joining him in an analysis of this material. Of course I would! The spectra were amazing. By continuously trailing at a telescope rate slightly off the sidereal rate, Greenstein was able to resolve the rotating accretion disk of the dark star companion. One found the classical "rotational disturbance" as the disk was alternately covered and then uncovered by the dark star. It was also found that the hydrogen emission lines had two components, one from the disk and one from the still expanding shell left from the 1934 outburst; only the He II emission line at λ4686 moved with the disk and gave the most nearly correct amplitude of the stellar velocity curve. The spectra gave rise to two adjoining papers (Greenstein & Kraft 1959, Kraft 1959).

I had been at Yerkes for only a few months when I received a surprising letter from I.S. Bowen, Director of the Mt. Wilson and Palomar observatories, asking if I would accept an appointment as assistant astronomer at the Mt. Wilson Observatory of the Carnegie Institution of Washington. This meant access to the 200-inch as well as the Mt. Wilson telescopes and the opportunity to serve essentially as an adjunct professor at Caltech. I was amazed to receive this letter and, of course, was inclined to accept immediately, although I did consult with several of the senior faculty, all of whom encouraged me to accept.

As it happened, the University of Chicago would soon lose its control over the McDonald Observatory, which of course would make Yerkes a place of less interest for someone in the field of stellar spectroscopy. The University of Texas had originally accepted the gift from Mr. McDonald with some reluctance: In the 1930s, University of Texas (UT), Austin had little interest in research and graduate instruction. It entered into a deal with Otto Struve, then director of Yerkes, that the University of Chicago would essentially operate McDonald and conduct research with the new 82-inch telescope. But with the rise of the NSF, NIH, and NASA, and a growing national interest in scientific research, UT reclaimed its interest in McDonald. Many other western universities, following a similar pattern in pursuit of research and graduate instruction, e.g., the Universities of Washington and Arizona, were much influenced by the model of UC. Thus in the early 1960s Texas took over McDonald and greatly expanded the astronomy faculty in Austin. The Yerkes "renaissance" was eclipsed: Geoff and Margaret Burbidge departed to UC San Diego, Kevin Prendergast to Columbia University, and Helmut Abt and Joe Chamberlain to Kitt Peak National Observatory. Even the senior staff went away: G.P. Kuiper to the University of Arizona and Al Hiltner to the University of Michigan. S. Chandrasekhar eventually moved all his activities to the Chicago campus and W.W. Morgan retired after a few years. Important astronomy continued to flow from Williams Bay, but as a product of a considerably smaller research faculty.

VII. RETURN TO MT. WILSON

Again we hauled a trailer across the country at Christmas time in 1959, detouring via Mt. Locke to visit friends we had gotten to know when I had brought my family to McDonald during some of the observing runs. Arriving in Pasadena just after the first of the year, we found a home in Altadena and settled in to life in lotusland. The factors leading to my selection for such an august appointment are unknown, of course, but as I said earlier, the system by which such selections were made is not that of today. I suspect that, as a postdoc or guest investigator, one had something of an "inside track" because one was known and was considered "trustworthy."

I continued and completed my work on the G-band photometry study of classical Cepheids based on McDonald observations and published several papers on Cepheid color excesses. By this time, H.C.(Chip) Arp had also joined the staff, and we collaborated on a paper comparing the period versus luminosity, period versus color, and period versus amplitude relations of Small Magellanic Clouds (SMC) and Galactic Cepheids, based on the newly developed galactic cluster parallaxes of Cepheids (Arp & Kraft 1961). Still later, a collaboration with Maarten Schmidt (Kraft & Schmidt 1963) led to a redetermination of the Galactic rotation constants (the Oort As and Bs); I was responsible for the Cepheid distances and Maarten for the analysis. Unfortunately even the most distant Cepheids of our sample were so close to the sun that we could not have found the flattened outer rotation curve, then totally unanticipated. Using these distances, I found (Kraft 1965) that the Cepheids did not distinctly outline the Galaxy's spiral arms, as many had hoped. Perhaps in their evolutionary lifetimes of 10 to 100 million years, Cepheids had simply drifted a bit out of the arms into which they had been born as B-type stars. Taking advantage of access to the 200-inch nebular spectrograph, I started again a program of high time-resolution spectroscopy of novae and dwarf novae, hoping to detect radial velocity variations with periods of the order of a few hours. This proved highly successful and led to a first paper on dwarf novae (Kraft 1962), in which I could confidently claim that all were close binaries with periods usually in the 4–6 hour range. The appropriate model was that of AE Aqr or DQ Her: one component a main sequence star, the other a white dwarf, the latter accreting material from a disk fed by the red star's overflow of its lobe of the inner Lagrangian surface. I gave the Warner Prize lecture on this subject at an American Astronomical Society (AAS) meeting in 1963. A bit later, I demonstrated that many novae exhibited the same binary characteristics as dwarf novae with similar short periods (Kraft 1964). The most exciting and novel example was the case of the recurrent small amplitude nova WZ Sge. How its very short binary period (82 min), then the shortest known, was discovered is a story worth telling, exemplifying as it does the caprice of fate.

In those days, with photographic plates as nonlinear detectors, one did not hold a star fixed in a given location on the slit but rather, in an effort to increase the effective signal-to-noise, one trailed the image on a slit of fixed length repeatedly, producing a "widened" spectrum of (hopefully) uniform optimal blackening at the detector. Because WZ Sge was a star of fifteenth mag, it was at the limit of the 200-inch's nebular spectrograph in an exposure of 40-min duration. Greenstein's earlier spectrum (Greenstein 1960) had revealed the wide hydrogen lines of a white dwarf, with double-peaked narrow emission in the absorption line centers, indicative of a rotating low-density circumstellar ring or disk. The *V* (or violet-displaced) component corresponded to the part of the ring moving toward the observer, and the *R* (or redward-displaced component), the part moving away. The first series of three consecutive 40-min spectra I obtained showed that the flux ratio of the two emission components varied: in spectrum 1, $V/R > 1$; spectrum 2, $V/R < 1$; in spectrum 3, $V/R > 1$ again. Could it be that there existed a third component, which varied in velocity with an amplitude equal to the separation of the emission components and had a period near 80 min? I went back to the prime focus, but this time I took a spectrum trailed continuously on a long slit at a rate just offset from the sidereal rate. Amazingly, the conjecture was true: I designated this emission component as the "s-wave"; it had a period of 81.5 min and an amplitude equal to the separation of the two stationary emission components. I could not have detected this phenomenon if the first of the three spectra had been taken at a significantly different orbital phase or if the exposure time of these spectra had not been closely half the period. The original trailed spectrum is illustrated in Kraft (1964). A hunch was fulfilled in reality: Is that not one of those profoundly exciting moments every scientist dreams about?

But the saga of WZ Sge did not end there. First, I learned from George Preston that a young Polish photometrist named Woytek Krzeminski was visiting Lick Observatory and would be pleased to study WZ Sge in a run at the Crossley reflector. Regular aperture photometry would not be easy because WZ Sge was located within a few arcsec of a bright star, but Krzeminski succeeded brilliantly in finding the photometric variations (eclipses?) and sharpening the period determination. This led to modeling of this unusual system (Krzeminski & Kraft 1964), in which the s-wave emission component arose in the collision of the rotating ring with the material being ejected by the M-star companion. Earlier I recalled hearing what seemed to me an arcane lecture on "gravitational waves," given by Chandrasekhar during my time at Yerkes. The prediction was that copious amounts of angular momentum and energy would be lost if two white dwarfs were found in an orbit with a period of a few hours. I wondered whether this picture might apply to WZ Sge and asked Jesse Greenstein about it. Jesse, who was a fount of knowledge on all matters of physics, referred me to the classical text by Landau & Lifschitz (1951) and suggested I see Jon Mathews in the Caltech physics department. This gave birth to a paper (Kraft, Mathews & Greenstein 1962) entitled "WZ Sge, a Possible Radiator of Gravitational Waves." We showed that WZ Sge might collapse as a system on an interesting cosmological timescale if the two components consisted of an M dwarf and a white dwarf. I believe this constituted the first appearance of any reference to gravitational radiation in the purely astronomical literature.

But WZ Sge highlighted a further deficiency intrinsic to our early model of novae and dwarf novae. In the case of AE Aqr, we had assumed that the red component was a star leaving the main sequence, swelling up to fill the inner Lagrangian surface. But it was becoming increasingly evident that the red stars in these systems had to be much less luminous and less massive than a star undergoing evolutionary swelling: in one or two cases, parallax measurements suggested $M_V \sim 8$ or 9 for dwarf novae. W.J. Luyten (University of Minnesota) and I put together proper motion and radial velocity data for 10 systems and derived a mean of $M_V = 7.5$, much too faint to permit a role for stellar evolution as a forcing mechanism. What, then, was driving the mass flow through the inner Lagrangian point? Only one thing made sense: gravitational wave grinding. The inner Lagrangian surface must be shrinking around the red dwarf! I speculated about this at a session on variable stars at the 1964 Hamburg IAU meeting, but didn't know how to work out the physics. The problem was taken up later by John Faulkner, who accurately predicted the mass transfer rates, etc. (Faulkner 1971). This work, along with that of Merle Walker, established the special binary nature of these objects, but later work by others probed fundamental questions we left completely untouched. A series of papers by Sumner Starrfield (Arizona State University) and his associates probed the mechanism of the outbursts and its relation to the mass transfer rate and white dwarf properties. Many of these objects were found to have strong magnetic fields that interacted significantly with the accretion disk and gas flows. Paula Szkody and other photometrists studied the quasi-periodic, extremely short period "flickering" first noted by Walker in DQ Her, generic to almost all such systems. A good review of these properties is that of Brian Warner (1995), who gave an IAU invited discourse (1994) on the subject. The bottom line to these investigations is this: Gravitational radiation is the mechanism by which these binaries transfer mass from the main sequence component to its white dwarf companion. The systems evolve in response to this process, and the nova explosions result from the accretion of hydrogen-rich material onto the surface of the white dwarf.

Struve died in 1963, and I recall that Director Bowen came down the hall at Santa Barbara Street to tell me the news. It was a shock, of course, although many of us knew that Struve was not in good health. I had already started a program of studying rotational velocities of stars in Galactic (open) clusters, and Struve's death stirred me to redouble my commitment to the field of stellar rotation that Struve had pioneered. Access to the 100-inch Coudé spectrograph allowed one to measure rotational velocities of main sequence stars nearly down to solar luminosity in the Pleiades, Coma, Praesepe, and the Hyades, with resolution in projected rotational velocity $v \sin i$ near 5–10 km sec^{-1}. I was able to show that the distribution of $v \sin i$ corresponded to a Maxwellian law in most of these clusters, and that the mean specific angular momentum along the main sequence went essentially as the mass $M^{+0.6}$. This work complemented a series of similar studies by Helmut Abt using Kitt Peak facilities.

It had long been known that rotational velocities of main sequence stars dropped precipitously in the middle F-type stars to values similar to the slow rotation of the sun. As Olin Wilson showed (Wilson 1966), this is just where stars begin to show Ca II emission arising from chromospheric layers. The mechanical energy associated with the outer convection zone then generates chromospheric emission, coronae, and hot stellar winds. The winds couple the stellar magnetic field and exert a torque on the star; Brandt (1966) showed that the solar wind had a net angular momentum near the Earth suggesting that the solar rotation would be significantly reduced on a 10^9-year timescale. Wilson (1963) reported that, at a given mass, Ca II emission in open cluster stars declined in strength with increasing age. Was it not possible that rotational velocities also declined in parallel with the Ca II emission?

This proved to be the case when main sequence and near main sequence stars in the Solar Neighborhood were sorted by age, i.e., their degree of departure from the zero-age main sequence as determined by the gravity index c1 of the Strömgren photometric system (Strömgren 1966). At the same time I was able to show, based sometimes on half-night exposures of stars as faint as F5 to G0 ($M_V \sim +3$ to $+4.5$) in open clusters, that rotation declined at a given spectral type as cluster ages increased. One could find late F-type stars rotating at 25 km sec^{-1} in clusters as young as the Pleiades (Kraft 1967). It was thus clear that stars fainter than $M_V \sim +3$ were losing half their angular momentum via stellar winds on a time-scale of order 10^9 years.

Astronomy as a research activity would appear to be pretty far removed from the cares of the world, but some of the physics and astronomy people in Pasadena were developing a concern, especially at the time of the Cuban missile crisis, that the public really knew very little of the dangers posed by nuclear weapons. I recall joining a group of Caltech scientists interested in spreading the word and participated in plans to speak to Rotary Clubs and similar groups on the effects of exploding a 20 megaton H bomb over Los Angeles. Ecological consequences were explored when the word "ecology" was new. I recall the looks of horror on the faces of businessmen as I described the destruction, massive firestorms, and the lingering effects of radiation from fallout. The futility of backyard fallout shelters, a "remedy" that the federal government was pushing at the time, was abundantly clear. Fortunately the good sense of the American people prevailed in the end as the whole concept was firmly rejected.

VIII. RETURN TO UNIVERSITY OF CALIFORNIA (SANTA CRUZ) AND LICK OBSERVATORY

In my first year at Mt. Wilson, George Preston came as a postdoc from UC Berkeley, and we renewed the friendship started when we were graduate students. He had developed a rational way of calculating the average color and magnitude that a Cepheid or RR Lyr star would have if it were in fact not variable—this as a means of determining its proper place in the HR diagram, relative to nonvariables. Later, after he had accepted a faculty position at Lick Observatory, he invited me to Mt. Hamilton in 1963 for a visit of a few days, as we worked on a joint project with Sidney Wolff, then Preston's graduate student. A discussion arose whether I might consider returning to Lick, but I politely declined because I could not imagine life for my family in the isolated community of Mt. Hamilton, a place even more remote than Williams Bay. But my outlook would change as the Lick astronomers were transferred later to a campus venue.

By 1967, life in southern California had become rather unattractive. By then, we had moved to Claremont. My younger son loved long-distance bicycle trips but now often came home gasping as the smog situation grew worse. My older son developed a real muse: He was going to become the world's greatest rock and roll guitarist (his dream). He founded a band, but long-haired hippy freak musicians were not so welcome in a conservative town. I began feeling uneasy about the rather authoritarian atmosphere of Santa Barbara St., even though I had been advanced from assistant astronomer to astronomer. I had worked with several Caltech graduate students (Chris Anderson, Kurt Anderson, Jim Gunn) on a few Mt. Wilson projects, and more interaction with graduate students was something I felt good about.

I took up the offer of a professorship in the astronomy operation spawned when the Lick scientific staff moved from Mt. Hamilton to the new Santa Cruz campus of the University of California (UCSC). The appointment was 80% astronomer, 20% professor in the UCSC astronomy graduate program. As sad as it was to leave good colleagues such as Olin Wilson, Armin Deutsch, Bev Oke, and Jesse Greenstein, who had offered much excellent scientific advice, I was excited by the prospect of working with new colleagues: Albert Whitford (director), senior staff members George Herbig and Stan Vasilevskis, and junior members George Preston, Merle Walker, Peter Bodenheimer, Tom Kinman, and Peter Conti. Of course, I was also looking forward to observing runs with the Lick 120-inch reflector, then the world's second largest telescope.

Both of us having graduated from the Berkeley Ph.D. program, Preston and I were great admirers of Otto Struve and of the "Yerkes Observatory style," in which an observatory-based graduate program included a cadre of theorists to complement the observers. Although Lick had appointed Bodenheimer, a theoretician in the field of stellar structure and evolution, to the mountain-top staff shortly before the move to UCSC, we saw the need for theoretical expertise in a number of other areas. At the time, the NSF was offering a group of "Science Departmental Improvement" grants, and with Whitford's approval Preston and I wrote a proposal for such a grant to Lick/UCSC in the amount of $600 K. Following a site visit involving the NSF astronomy program director and an evaluation committee, the funds were secured for a five-year program supporting the appointment of five faculty positions, as well as postdocs and graduate students. Early faculty appointments included John Faulkner, Bill Mathews, and George Blumenthal (now chancellor of UCSC), all of whom went forward with distinguished research careers. Although hard financial times were soon to hit UC during Ronald Reagan's governorship, which led to a significant truncation of planned programs at UCSC, founding chancellor Dean McHenry eventually (1972) honored the commitment to the NSF and shouldered the financial support for the new appointments when the NSF grant ended.

But all was not well in our astronomy operation. George Preston from 1966 onward had taken the graduate program's chairmanship, but became increasingly irritated by the interference of the UCSC Vice Chancellor in Lick affairs. In addition, after serving as Director for a decade, Whitford felt under pressure to step down, and Preston resigned to accept a position at Mt. Wilson Observatory. It was assumed that a new Lick director would soon be recruited from the outside community of astronomers, and as a grateful new appointee, I naively accepted the position of Acting Director for what might be a year. But this was one of the few times when fate threw me a curve I could not hit: The year stretched into five (!), interrupted only in 1970–71 when I accepted a visiting scientist appointment at JILA, University of Colorado. In this period, George Herbig took on the role of Acting Director. Finally in 1973, Don Osterbrock left Wisconsin to accept the Lick Directorship; his thoughts about this are keenly summarized in his own Annual Reviews biographical sketch (Osterbrock 2000). But with Osterbrock, we now had a director who had superb credentials as both theoretician and observer, so our vision of the rebirth of the Yerkes style at Lick was fully realized.

Relieved of duties I had not sought, I left again, this time as a visiting astronomer at Kitt Peak National Observatory, followed by two months at the Institute for Advanced Study in Princeton, where I gave a few lectures. Rosalie and I attended our first IAU General Assembly in Warsaw in 1973. I was elected President of the AAS in 1974 for the usual two-year term. It was not a time of momentous decisions and science was not yet viewed with suspicion by Congress and the public. Earlier, I had been unexpectedly elected to the National Academy of Sciences (1971) and the American Academy of Arts and Sciences (1974).

IX. CHANGES IN RESEARCH DIRECTION

At the beginning of my research work at Lick, I naturally tried to continue investigating high energy sources, switching now to X-ray binaries in which the collapsed object was thought to be a neutron star or black hole. The optical component of Cyg X-1 was a B-type star, and if its mass was as large as a normal main sequence or near main sequence star, that would imply a large mass for the X-ray component. There remained the question of whether the B-type star could be a lower mass object on a blue-loop track, departing from the giant star domain. Several UCSC graduate students (Bregman et al. 1973), at my suggestion, obtained photometry of a large number of Cyg X-1 field stars, which showed that the object had to be at a large distance, too large for the "blue loop" hypothesis to be valid. At the same time, low resolution spectrographic work on Cyg X-2 (Kraft & Demoulin 1967, Kraft & Miller 1969) using the prime focus of the 120-inch, proved frustrating, as no conclusive binary orbit could be found. It seemed to be an auspicious time to abandon further commitment of observing resources to the study of such objects.

This led to my first, and almost sole, excursion into extragalactic astronomy. From the Caltech graduate program came Kurt Anderson and Bob O'Connell as postdocs supported by the Departmental Improvement Grant. Kurt and I re-explored the He I metastable absorption feature that Olin Wilson had discussed years before in NGC 4151, finding it triple and shortward displaced, indicating that material was being ejected at velocities of 280, 550, and 840 km s^{-1} from this Seyfert Galaxy (Anderson & Kraft 1969). Bob and I observed a couple of Zwicky compact galaxies and obtained mass estimates from the rotation curves (O'Connell & Kraft 1972). I remember that it was hard work to accurately guide these extremely faint objects on the slit of the 120-inch Coudé spectrograph in the days before TV, but we did have an "image rotator" to counteract the field rotation inherent to the Coudé focus!

But the most significant research reorientation for me was an awakening of interest in the abundances of the chemical elements. This was work ideally suited to graduate student interactions: Equivalent widths (EWs) needed measuring, curves of growth needed construction, and all this became part of practical instruction for students of stellar atmospheres. I took away with me to JILA in 1970 the EWs of the classical Type II Cepheid W Vir that UCSC students and I had measured, to see if we could improve on the classical analysis by Abt (1954). Minor modifications were discussed in our subsequent paper, but we reconfirmed the existence of the two-atmosphere shock-wave model with only slightly revised abundances (Barker et al. 1971). A similar study of another Type II Cepheid (TW Cap) was carried out with Kurt Anderson (Anderson & Kraft 1971).

But it was the mysterious variable star FG Sge that really tweaked my interest in, and switch to, the abundances of the elements. Here was a supergiant F-type star that was apparently moving on a 50-year timescale along a horizontal track in a M_{bol} versus T_{eff} diagram from the hot B-star domain to its present location; it was also the central star of a planetary nebula. A record of its increased visual brightness and its spectral and abundance properties had been reported by Herbig & Boyarchuk (1968). But by 1972, the spectrum had developed anomalous additional lines of s-process species (i.e., elements produced by slow neutron capture on Fe-peak elements) that appeared to strengthen with increasing time. At the suggestion of one of the JILA staff members, I asked Ed Langer, a Colorado University Ph.D. knowledgable in the field of stellar interiors and nucleosynthesis, to join the UCSC group over the summer to study this object. In the end, we published a paper (Langer, Kraft & Anderson 1974) showing that FG Sge was convecting s-process material into its atmosphere in "real time" as it moved to the right in the HR diagram. Later, as the carbon abundance increased, it turned into an R CrB star (Gonzalez et al. 1998); the predictions of theory were borne out in some detail. On the personal level, Ed Langer's summertime appearances in Santa Cruz became a regular activity for many subsequent years, as he worked with our so-called Lick/Texas group on abundance problems especially in globular clusters.

Exploration into the structure and metallicity of the Galactic halo began as a result of Dennis Butler's thesis (Butler 1975), in which he calibrated Preston's (1959) delta-S measurements by [Fe/H] values, based on analysis of 120-inch Coudé spectra of field RR Lyraes. It then became possible to study delta-S values from low-resolution spectra of RR Lyraes in globular clusters and extract cluster [Fe/H] values, at a time when these were not known with great precision. Observations were greatly aided by the invention of the Wampler/Robinson image-dissector scanner, which made delta-S measurements of cluster and Galactic nuclear bulge RR Lyraes accessible. Meanwhile, under Tom Kinman's direction, the redoubtable mountain-top observer Gene Harlan had begun using the Lick astrographic telescope to discover RR Lyraes in select fields extending from the north Galactic pole down to the bulge in a study to determine the density gradient of the halo. Butler, newly arrived graduate students Nick Suntzeff and Ed Kemper, and I joined Kinman to obtain delta-S, therefore [Fe/H], measurements of these newly discovered RR Lyraes; the work culminated in an extensive series of papers that went on into the 1990s (e.g., Suntzeff, Kinman & Kraft 1991) showing that out to distances within about 25 kpc, the RR Lyr mean metallicity was fairly flat, with wide scatter ranging from [Fe/H] near -0.8 to -2.2.

X. GLOBULAR CLUSTER CARBON AND NITROGEN ABUNDANCES—SYNTHETIC SPECTRA

About this time, Bob Zinn (1973) in his Ph.D. thesis discovered that M92 giant stars showed wide variations in the appearance of the G-band of CH in a common giant star luminosity range in M92, thus indicating that the cherished assumption of intracluster abundance uniformity could no longer be maintained. During my visit to Kitt Peak in 1973, I came into contact with then staff member Duane Carbon, a Ph.D. from Harvard, who suggested modeling the CH, CN, and NH regions of globular cluster giant star spectra by first modeling the solar spectrum at high resolution to obtain reliable log *gf* (transition probabilities) values for the many lines of these features, then predicting the band strengths in the cluster stars based on local thermodynamic equilibrium (LTE) models, suitably degraded in resolution to fit the low resolution spectra that could be obtained for such faint objects. With Carbon, graduate students Suntzeff and Kemper, Ed Langer, and visitor Charles Trefzger (University of Basel), we at Lick embarked on a program of observing giants brighter than $M_V \sim 0.0$ in M92, M15, M3, and M13 at spectral resolution of about 8 Å, using the Wampler/Robinson image-dissector scanner operated at the 120-inch Cassegrain focus. We observed the CH, CN, and NH bands, comparing the observations with Carbon's synthetic spectra. We were able to estimate values of [C/Fe] and [N/Fe], and show that, in M92 at least, although wide variations in these abundances could be found at any given location in the HR diagram, on the average [C/Fe] fell by a factor of 10 as stars progressed from the main sequence to the tip of the giant branch (Carbon et al. 1982), indicating that the external stellar layers had been processed by convective mixing to regions in or near the hydrogen-burning shell. Stars near the main sequence had to be observed using the Palomar 200-inch telescope, thanks to an observing run with Bev Oke (Langer et al. 1986). Suntzeff's thesis (1981) dealt with M3 and M13, for which similar variations were found at a given luminosity, but in which the systematic decline with luminosity in mean carbon abundance was not so clearly indicated. A similar study of halo field dwarfs was carried out with visiting Brazilian astronomer Beatriz Barbuy (Carbon et al. 1984), with the result that [C/Fe] did not appear to depend on [Fe/H] and [N/Fe] did not follow the expected secondary origin of N.

Nowadays spectral synthesis is a widely adopted technique in a variety of fields both galactic and extragalactic. Carbon's spectral synthesis program was a notable pioneering effort for which, in my view, he has not received the credit he deserves.

XI. CALIFORNIA DARK SKY SITE? LARGE TELESCOPE PROSPECTS?

It was becoming increasingly clear that the days of Mt. Hamilton as a dark sky site were numbered, owing to the accelerated growth of San Jose and the Silicon Valley in general. Efforts to limit the effect of high pressure Na lamps were spearheaded by Sandy Faber, who testified in support of low pressure Na street lighting before numerous political entities in the Santa Clara valley. This proved a highly successful venture, but did not completely satisfy the desire to find a higher and truly darker place for UC astronomy than Mt. Hamilton. Under Osterbrock's leadership, we began to explore the idea of a 100-inch-class telescope for Lick at the summit of Junipero Serra, a 6000-foot mountain in the Los Padres National Forest, the highest peak in the Santa Lucia range 50 miles southeast of Monterey behind Big Sur. At first, the Sierra Club opposed any thought of a telescope atop a peak in this wilderness area, but we proposed to limit public access by building a tramway from a base facility to the summit available only to UC staff. A compromise, engineered by Leon Panetta, then congressman from the Monterey Bay area (and later policy advisor to President Clinton), was worked out between Lick and the Sierra Club embodying this limitation. But this was only the tip of an iceberg of complications: Junipero Serra it seems was also an Ohlone Indian sacred mountain. Hopes for a resolution of the Indian concerns were dashed as they consistently declined to meet with us for discussions.

At the same time, some of the younger Lick astronomers, and their colleagues on the other UC campuses, were growing restless. Although we had lost Tom Kinman, who had accepted an appointment at Kitt Peak, we had gained the services of Joe Miller, a University of Wisconsin Ph.D., who had carved a unique niche in the field of polarimetry and applied the technique to the study of Seyfert galaxies. Sandy Faber, a Ph.D. from Harvard, had joined the Lick staff in 1972 and was becoming a rapidly rising star in the study of stellar populations in elliptical galaxies and the relationship between Galaxy mass and velocity dispersion. The interests of these two young people as well as Joe Wampler, a Yerkes Ph.D., noted especially as a superb instrumentalist and who had joined the staff before I came to UCSC, all would be served better if UC astronomy were conducted from a site higher, dryer and darker than Junipero Serra Peak. Joe Wampler and Lick IR astronomer Dave Rank, in particular, proposed the revolutionary idea of constructing a 7-m telescope to be located at a truly dark, high, and dry place. Many UC astronomers favored the top of Mauna Kea, a site pioneered by University of Hawaii astronomers. The prospect of Junipero Serra, with its political and sociological problems, no longer seemed attractive.

But ideas, the time of which have come, have a way of emerging from more than one mind at the same time. Wampler and Rank had thought in terms of a 7-m primary in the form of a thin-mirror monolith; Jerry Nelson, a young physicist/astronomer working at the Lawrence Berkeley Lab (LBL), conceived of a still larger 10-m segmented primary mirror consisting of 36 mostly off-axis hexagonal mirrors held in proper interrelationship by computer-controlled edge sensors. In turn, Wampler and Rank proposed increasing the aperture of the monolith to 10 m. Osterbrock appointed a "graybeard's committee" to study the rival concepts and make a decision between them. Senior astronomers from the then-active astronomy faculties within the UC system (Berkeley, Santa Cruz, Los Angeles, San Diego) were appointed. In the end the decision was a close one, in favor of the segmented mirror concept. Committee members feared that the monolith might break in the figuring process, and the idea that a small prototype could be built to test the edge sensor concept weighed heavily in the decision. (Years later, it proved to be the case that each of these concepts has been incorporated in different large telescope projects.) Acting no doubt with the recommendation of advisors in the physics and astronomy communities, UC President David Saxon supported the segmented mirror proposal with $5 M from the President's office, the funds to flow through Lick and be disbursed to the team working at LBL. Thus was born the telescope project that would eventually morph into the Keck I telescope on Mauna Kea.

I was eventually put in charge of the site selection process, and Merle Walker, with the assistance of Gene Harlan, did the actual site work. Places such as White Mountain and La Palma were considered, but Mauna Kea was eventually chosen in terms of superiority from the point of view of weather, seeing, accessibility and local politics.

XII. LICK DIRECTORSHIP AGAIN

After serving for eight years as Lick Director, Don Osterbrock decided to return to 100% teaching and research. It was also clear that times were changing in UC optical astronomy, as demand grew to find not only a better site than Mt. Hamilton, but also to find financial support for the proposed 10-m telescope. What should be done? What role was Lick to play in this systemwide endeavor? A new Lick Director had to be found. I had been with Lick for 14 years and had received my Ph.D. from UC Berkeley; after such a long period, one certainly develops a sense of institutional loyalty. I decided to throw my hat into the ring, and in the end, was appointed Lick Director in September 1981. A post I had hated 10 years earlier I now embraced.

The first order of business was the appointment of a UC systemwide committee to advise the director on the future course of Lick. I needed to find out not only what Lick/UCSC astronomers wanted to do, but also what UCB, UCLA, UCSD, and now those in the Ten Meter Telescope (TMT) project at LBL thought was the best course of action. Was there some common ground? What should Lick do in support of Jerry Nelson, Terry Mast, and the other principals of the work being done at LBL? Meanwhile, Wampler and Rank had suggested an improvement for Mt. Hamilton, e.g., a computer-controlled modified alt-az 80-inch telescope, which could be built in the Lick shops on the cheap. But the systemwide committee advised against dividing Lick technical attention between two projects, so I scuttled the 80-inch proposal in favor of doing whatever was needed to help those at LBL in the large telescope project. The second order of business was providing strong scientific leadership for the Lick shops and labs, organizations enjoying some degree of hard money support from Lick's systemwide budget. My personal knowledge of machines and instruments being next to zero, I was very lucky to secure the support of Joe Miller who took on the role of Assistant Director for Technical Services. Although Joe Wampler departed Lick for the European Southern Observatory (ESO), we retained the expertise of Lloyd Robinson, who had done remarkable work in integrating computer technology into our instrumentation systems. Later, Harland Epps from UCLA astronomy, an expert in the science of complex lens designs, would join the Lick staff to improve the capability of our optical lab. Joe Calmes, who had served under Osterbrock in an administrative capacity, began playing a larger role as Assistant Director for Administration, focusing greater attention on fund-raising prospects for the TMT. Meanwhile President Saxon asked Eugene Trefethen, an old-blue from UC Berkeley, a man with notable business connections (he had been CEO of Kaiser Industries and founder of the Trefethen Winery in Napa Valley) to see if funds might be raised among the "movers and shakers" of the California industrial scene. But there are only so many such people, and the Chancellors of the various UC campuses had most of them already "earmarked." Where was the President's office to turn? I revised the shop's and lab's priority lists to put any assistance we could provide to the TMT project and Nelson's LBL group at the top. In response, we secured the "contract" for an autocollimating telescope designed to test the computer-controlled segment alignment concept. Neal Jern, then shop superintendent, did a fine job supervising the fabrication of the telescope, with the guidance of the Lick engineers, principally Jack Osborne. Subsequent tests of the segment alignment mechanism by Nelson's team went well and proved the success of the concept. Attempts to interest California donors in the TMT project proved mostly unsuccessful, although capitalists in technology-oriented Silicon Valley were approached. Joe Calmes even solicited support from monied interests in Hawaii, where the telescope was likely to be located, as site-testing proceeded on Mauna Kea. Oddly, we were approached by a private source, a lady of advanced years who had heard informally of the project via a newspaper article and a docent stationed on Mt. Hamilton. As contacts became more serious, a sum of around \$50 *M* was pledged. Unfortunately, the lady died before some of the critical papers were signed; a fight with heirs loomed, and UC abandoned the agreement. Other possibilities were explored, especially the prospect of partnership with other Universities or observatories, such as Carnegie, University of Chicago, and Caltech.

Despite the increased attention to the affairs of the prospective TMT, I did not totally neglect following up opportunities to improve the observational capability of the Lick 3-m (120-inch Shane) telescope. A contact was made with Miss Clara-Belle Hamilton, granddaughter of the Rev. Laurentine Hamilton, for whom Mt. Hamilton is named. Largely through the efforts of Joe Calmes, Miss Hamilton agreed to donate funds for the Hamilton Echelle Spectrograph, which with its echelle layout and CCD detector, greatly enhanced the speed of the 3-m Coudé. The Hamilton was built in the Lick shops, under the supervision of Steve Vogt, its designer, a young University of Texas Ph.D. who had been appointed to the Lick faculty in 1978. After it went into operation in 1987, the Hamilton spectrograph became the workhorse of spectroscopy at high spectral resolution ($R \sim 30$ K–50 K) especially for problems associated with stars brighter than fifteenth mag, where sky brightness is not a limiting factor. Much of the later initial work by Steve Vogt, Geoff Marcy, Paul Butler, and Debra Fisher involving the discovery of extrasolar planets was done with this device, along with countless other stellar problems including the abundances of the elements in very old stars.

During a period when one takes on serious administrative responsibilities, research turns from exploring new initiatives to completing and/or extending studies already in the repertoire. Thus the RR Lyr halo abundance survey was completed (Suntzeff, Kinman & Kraft 1991), metallicities were obtained for outer halo globular clusters such as NGC 7006 (Friel et al. 1982) and NGC 2419 (Suntzeff, Kraft & Kinman 1988) as well as local group objects: Draco (Kinman & Kraft 1980) and a field of the SMC near NGC 121 (Suntzeff et al. 1986). Also on the positive side, I had the opportunity to work with a new graduate student, Eileen Friel, who not only developed into an inspiring research colleague, but also maintained a semblance of order within my characteristically chaotic research activities. In surveying the actual metallicities of giants in high Galactic latitude fields, she found that the Bahcall-Soneira models gave a good representation of metal-rich stars, but seriously overpredicted the population of metal-poor stars (Friel 1987). Taking a clue from her work, I tried to explore this problem a bit more, especially among the corresponding horizontal branch stars (Kraft 1989).

XIII. A JOINT PROJECT WITH CALTECH; THE KECK I TELESCOPE

Negotiations with possible partners at last bore fruit as UC and Caltech joined to form CARA, the California Association for Research in Astronomy. The initial governing Board consisted of UC Academic Vice President, Bill Frazer; Ed Stone, Caltech's Executive Officer for Physics, Math and Astronomy; Caltech Provost, Robbie Vogt; Ron Brady, UC Vice President for Business and Finance; Gerry Neugerbauer, Palomar Director; and myself as Lick Director. Also created was a Science Steering Committee, at various times consisting of Neugebauer; Kraft; J. Nelson; S. Faber; J. Miller; G. Illingworth; H.E. Smith and E. Becklin from UC; and W. Sargent, M. Schmidt, and W. Soifer from Caltech. Capital money was provided by Howard Keck, a Texas oil business man, founder of the Keck Foundation and member of the Caltech Board of Trustees, and the telescope was to be named in honor of his father, W.M. Keck. The agreement between Caltech and UC assumed that Caltech would fund the construction (from the Keck Foundation grant plus a lesser amount from the Caltech endowment) of a 10-m segmented mirror telescope, that it would remain the property of Caltech, and that UC would pay for operations over a roughly 25-year period, providing as well an annual sum for the construction of instruments. (UC was also to be credited for the $5 *M* that had been spent developing and testing the initial design concept).

As initial instruments, the Science Steering Committee chose a low to medium spectral resolution optical/near-IR spectrograph, a project under the management of Bev Oke (Caltech), and an improved version of Steve Vogt's (UCSC/Lick) Hamilton spectrograph for work at high spectral resolution. The telescope design required the fabrication of a highly aspheric secondary mirror, some 60 inches in diameter. Rather than seeking a commercial source, CARA management, with consent from Project Manager Jerry Smith, turned fabrication over to the Lick optical shop, and this together with the construction of the Keck high-resolution spectrograph, forced expansion, and rehabilitation of the Lick labs and shops. Joe Miller assumed responsibility for construction of the aspheric secondary mirror, with newly hired optician David Hilyard assuming the task of figuring the surface. A major effort went into the calibration of a profilometer that, when correctly calibrated, provided the means by which time-consuming optical tests of the figure could be by-passed. The outcome was a major success for Lick.

Meanwhile the UC contingent discussed internal management plans for the Keck Telescope era. With its ten-campus structure, how was UC to make a sensible interface with both Caltech and CARA? It seemed appropriate to build up a structure based on Lick with an expanded systemwide role for the Lick director, plus a strong systemwide committee to advise the director on such matters as time allocation, new instrumentation, etc. The existence of a number of hard, state-money supported positions in the Lick labs and shops was a distinct advantage, the result of building up the new structure from the Lick base. This did not preclude the the inclusion of other campuses, and subsequently an IR group was founded at UCLA, also dedicated to instrumental development at Keck and Lick. With leadership from Gene Smith (UCSD) and Kraft, a UC management document was drafted, passed through the various UC campuses, and finally approved by the Systemwide Academic Senate Committee and the President's Office—thus in 1988 the University of California Observatories (a.k.a. UCO/Lick) was created. But I must recount here another stroke of remarkable "good luck." It happened that Gene Smith was also the UCSD member of the Systemwide Academic Senate committee just mentioned, which surely provided Gene with the opportunity to inspire the Committee's confidence in our management plan! With Jerry Smith—much experienced in the success of certain NASA projects, e.g., IRAS and IRTF—as Project Manager and Jerry Nelson as Project Scientist, construction of the Keck I Telescope proceeded to a successful conclusion, although not without a few scary moments along the way. An example was the fabrication of the off-axis mirror segments. The idea here was that one could polish a spherical configuration into a suitably constrained circular blank segment that, when relaxed, would acquire the required off-axis shape. This was achieved but, unfortunately, the proper figure was not retained when the edges were cut to fit the hexagonal shape. Happily, subsequent commercial ion polishing came to the rescue. The telescope came into existence in 1988, saw first light in 1990, and after completion and breaking-in was fully operational by 1993; it had such success that an identical Keck II telescope was completed and was fully functional by 1996. Further funding was contributed by NASA for completion of Keck II, along with Caltech and Keck Foundation financing. In the end, time allocation for the two telescopes was split, with 12.5% going off the top to the University of Hawaii (as landlord), one-sixth to NASA-oriented projects, and the remainder split evenly between Caltech and UC astronomers.

XIV. INTERNATIONAL ASTRONOMICAL UNION PRESIDENCY, POST-DIRECTORIAL RESEARCH, LICK-TEXAS GROUP

I formally retired from the UCO/Lick directorship in 1991 and was ably succeeded by Joe Miller, a man with exactly the right credentials for the job. Vistas of new research opportunities opened as I contemplated access to the high-resolution Keck I spectrograph. But first it was time for a sabbatical, and Rosalie and I took off for several month's leave at the University of Texas, Austin, where I hoped to forge a research partnership with Chris Sneden. On my return, Graeme Smith and Jean Brodie kindly organized a 1992 UCSC summer symposium in my honor, entitled "The Globular Cluster-Galaxy Connection" (ASP Conf. Ser. No. 48, 1993), which I greatly appreciated and from which I learned a lot. I formally retired from the Lick/UCSC faculty in the fall of 1992, giving up teaching (except for Ph.D. thesis students) in favor of continued research.

Although globular cluster giants could be observed at fairly high resolution (~30,000) with the Lick 3-m Hamilton in such northern clusters as *M*71, *M*13, and *M*92, the Keck I HIRES permitted access to many fainter clusters (e.g., *M*15, *M*3, NGC 7006) at similar resolution and much greater speed. Sneden had developed a sophisticated computer program (MOOG) for chemical abundance analysis, and we began a collaboration for observation and analysis of globular cluster giants based on Lick and Keck spectra. We called the collection of participants the Lick/Texas Group (LTG), and at various times it consisted of Sneden and his University of Texas graduate students, especially Inese Ivans and Jennifer Sobeck, graduate students from UCSC, as well as the late Charles Prosser, Matt Shetrone, and Jon Fulbright, and professional colleagues, e.g., Nick Suntzeff, Ruth Peterson, Graeme Smith, and Raja Guhathakurta, who joined us from time to time. Ed Langer, who visited Lick often in summers on leave from Colorado College, contributed heavily to the success of our work until his tragic and untimely death in 1999. Here is a brief summary of our findings.

From the earlier work with Duane Carbon, we already knew that carbon abundances on the average declined with advancing evolutionary state among halo and globular cluster giants, and nitrogen correspondingly became enhanced. Earlier work (Pilachowski 1988) reported similar deficiencies of oxygen in some *M*92 giants. Following upon this, we found that an oxygen/sodium anticorrelation was a general property of globular cluster giants, and was especially notable in *M*13 and *M*15, so that either in the cluster giants themselves, or in material later incorporated into these stars, proton-capture synthesis had converted Ne into Na along with O (and C) into N. Matt Shetrone's (1996) thesis work showed that in clusters there was a strong correlation between Na and Al overabundances; this was coupled with a new puzzle, e.g., an overabundance of the rare isotopes of Mg (^{25}Mg, ^{26}Mg) compared with ^{24}Mg among M13 giants. None of these anomalies exist among halo field giants of similar metallicity. What is it that separates field from cluster giants? Later work by others showed that many of these cluster abundance anomalies exist also among main sequence stars (see the excellent summary by Gratton, Sneden & Carretta 2004), and thus represent an ab initio cluster characteristic. If all stars, even those of the halo field, were born in clusters, what is it that enables the halo field stars to survive unsullied by the anomalies? Any scenario of star formation must obviously account for these differences. I was able to report some of the early findings of the LTG in my Henry Norris Russell lecture to the AAS in 1995. Later, in inspecting some of our Keck M3 spectra, Ruth Peterson noticed a remarkable anomaly, an M3 giant with an extremely strong Li I line! With advancing evolution, Li should long before have been completely depleted. It seems that new Li can be produced by the Be-transport mechanism, but is quickly destroyed. It may be that all globular cluster giants do this, but the episode has an extremely short timescale (Kraft et al. 1999).

It has long been known that the ratio of the abundance of the so-called alpha elements to Fe signals the degree to which the mix is affected by the ejecta of Type Ia supernovae compared with Type II, ejecta of the former coming into the mix some 1 to 2 Gyr later than the latter. My last graduate student, Jon Fulbright, studied a large sample of low metallicity halo field stars finding that, on the average, the ratio of alpha elements to Fe divided into two groups depending on Galactic orbital angular momentum (Fulbright 2000, 2002), thus strengthening the growing suspicion that the Galactic halo shows signs of significant kinematic and abundance substructure. The existence of halo substructure had also been found from a complete sample of halo blue horizontal branch stars lying outside the solar circle, in which two populations were detected, one spherically distributed out to about 25 kpc with horizontal branch morphology like that of M3, the other an additional flattened component having a scale-height of 2.5 kpc; the two populations did not differ in metallicity, but were found to differ in the steepness of the Balmer jump (Kinman, Suntzeff & Kraft 1994), as yet a conclusion without physical explanation. Much additional work on the kinematical properties of these populations has been pursued by Kinman and associates.

Earlier, I had the good fortune to collaborate with Nick Suntzeff in an abundance study of nearly 350 giants in ω Cen, more than half of which lay well below the horizontal branch, and were therefore exempt from confusion with giants on the asymptotic branch. We found a metallicity distribution that rose suddenly to a modal value near [Fe/H] $= -1.7$, with a long tail extending to higher metallicities. This strongly suggests that ω Cen was formed from pre-enriched environment leading up to the modal metallicity, and then underwent an episode of self-enrichment toward the higher metallicities observed (Suntzeff & Kraft 1996).

Sometime in 1993, I was very surprised to receive a telephone call from Helmut Abt, then a member of the IAU nominating committee, asking if I would consider being nominated for the Presidency of the IAU. There had not been an American President since Leo Goldberg held the position; his term ended in 1982. I was not particularly well prepared, never having been an officer of an IAU commission, although I did serve as a Vice President from 1982 to 1985. After much thought, I said "yes," partly because some of my heroes, e.g., Otto Struve, had been Presidents many years before, partly out of a sense of adventure, partly out of a feeling that some changes (too pompous to call them "reforms") perhaps were needed. I became President-Elect following the 1994 General Assembly (GA) in the Hague, President in 1997 after the GA in Kyoto, and Past President after the GA in Manchester. I had the enormous good fortune to be President when Johannes Anderson (University of Copenhagen, Denmark) was General Secretary, a man whose scientific knowledge and administrative skills are second to none and who was ably supported by the late Monique Orine who ran the Paris office with great energy and skill. Anderson and I did succeed in making a few changes in the workings of the Executive Committee (EC), which I hope were improvements. First, we changed the focus of the EC meetings so that administrative affairs were put later in the meeting after the review of proposals for Symposia and Colloquia. Second, we insisted on the appointment of Vice Presidents who were to represent widely the various disciplines of contemporary astronomy, not solely the diversity of national origin. Both of these steps were designed to place IAU support for the best science highest among IAU priorities. In these efforts we were greatly assisted by the hard work of the assistant General Secretary Hans Rickman (University of Uppsala, Sweden), one of whose tasks consisted of reviewing the proposals for Symposia and Colloquia for the benefit of the EC. Anderson made progress in the effort to bring our concerns about night-sky and radio frequency contamination before appropriate bodies of the United Nations (UN), but I do not now have knowledge of subsequent progress in these areas. Unfortunately, my participation in IAU affairs had to be curtailed during my term as Past President as I struggled with the personal medical problem associated with retinal detachments.

XV. SOME FINAL REMARKS

With the onset of advancing years, I finally gave up observing at telescopes in 1999, but have kept busy continuing to work on the abundances of *r*-process species (i.e., heavy elements produced by rapid neutron addition to Fe-peak elements) in the extremely metal-poor globular cluster *M*15 in collaboration with Chris Sneden and Jennifer Sobeck. Many more elements can now be studied than was the case when we wrote our original paper (Sneden et al. 2000), owing to the laboratory physics work of J. Lawler at the University of Minnesota. Inese Ivans and I reinvestigated the globular cluster abundance scale on the basis of Fe II, hoping to avoid possible non-LTE effects in Fe I. Finally it was for me a sentimental and very moving moment to be awarded the Bruce Medal of the Astronomical Society of the Pacific (ASP) at its annual meeting in Tucson in 2005. The ASP was the first astronomical organization I had joined and I had been a member for almost 60 years! It was truly like coming home after a lengthy journey.

I have lived a long and very fortunate life full of challenges, but one driven largely by the positive strokes of fate. I was lucky to be in the right place at the right time in a science fraught with fresh opportunities and strong public and private support.

So I end with an appeal. For me, what made most of the difference was the existence of state-supported public universities such as the Universities of Washington and California. These institutions provided the avenue by which poor and lower middle class students might savor the advantages of a higher education and move on. We live now in an era in which these advantages are disappearing, as our social order experiences the decline and fall of the middle class, an increase in poverty, and the division of our society into the very rich and the poor. Many students can no longer afford the fees and tuition that allow access to higher education. What is needed are clearly more tax dollars and general private gifts, along with a new dedication to the idea of higher education as a top priority of our national life.

Finally I express my thanks to Rosalie, friends, scientific colleagues, and graduate students who have carried me along over many years, shared a few modest triumphs, and put up with my failures. I also much appreciate the help of Joe Miller (Lick Observatory), Jerry Nelson (Lick Observatory), and Ed Stone (Caltech) who read portions of the first draft and corrected a number of errors attributable to my faulty memory. I also thank Evan Kirby for assistance in the preparation of this manuscript.

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