

June I. Dreamter

AN ASTRONOMICAL LIFE

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PERSONAL

People write history although never certain what the future could learn from the past. Professional historians recreate a possible past with emphasis on what documentary evidence exists; they reimage it, conditioned by their own world view. Events remembered by participants may differ from history so much as to be nearly unrecognizable. My prefatory chapter will be quite personal and anecdotal; it is only one possible account of institutions and events of the over 50 years through which I have lived as a scientist. The mental landscape recreated is in part memory, in part illusion, but not necessarily deceptive. It naturally puts me too much at the center of events. Another landscape could be found in the roughly 600 pages of transcribed, personal oral history, and in many shelves of archives. Which is the true picture? I would have liked, sometime, to describe objectively the growth and maturity of the institutions where I have been and to help document the explosive growth of the knowledge and funding of astronomy in the United States. But a personal approach should give readers a feeling for the startling change in style of research, dramatic even though spread over 50 years. Ten or more years spent for an experiment in space, multiauthor papers, and computer-generated theory are quite alien to me. Here I limit myself to my own activities and interests, involvements with government, and the characteristics of a few of those leaders in astronomy with whom I have worked. By chance I am the first US-born astronomer to write a prefatory chapter for this series. My work is rooted in personality; my public activities reflect my private world. I was born in the Year of the Comet (1909), a comet that appears again on a Palomar CCD image (1982); 1909 is not a lost world, for me.

I lack the often-quoted advantages of an impoverished and embittered childhood; my earliest memories are of being indulged. My paternal

grandfather had come to America in 1888; in the usual miraculous American way, he and my father prospered. When I was eight, my grandfather gave me a brass telescope on a tripod. With it I lectured my friends on the planets, stars, and nebulae. I founded an "Interesting Topics Club" to describe the miracles in the sky. As in many Jewish families, there was an excellent and varied library. At home I read C. Flammarion, J. Verne, S. Newcomb, and the Splendors of the Heavens. In a basement laboratory, I used a prism spectroscope (from Gaertner Scientific), an arc, a rotary spark, a rectifier, and a radio transmitter. From Kayser's Handbook of Spectroscopy, with spectroscope and arc, I tried to identify atoms; line series were simple, but not so the new multiplets and energy-level diagrams. I read contemporary literature as well as science as I skipped through the New York public schools, until I entered Horace Mann School for Boys (a private high school) when I was eleven. My first hero was my Latin teacher. He threw me out of the classroom regularly until I caught up with my classmates, three years older, who had already studied Latin for two years. (I still can read Latin.) Mr. Nagle introduced me to the "ideal" of hard work. No longer a child's oyster, the world was a fascinating sea, navigable by work. I studied and enjoyed chemistry, then the most appealing experimental science; physics, with levers, pulleys and magnets, was dull.

In 1926, when 16 and at Harvard, I met Naomi Kitay, whom I was fortunate enough to marry in 1934, after she graduated from the Horace Mann School for Girls and Mount Holyoke College in 1933. I had one younger brother; she, however, had a brother and three older, educated, and talkative sisters! Our youths were spent in comfortable, patriarchal ways no longer imaginable, dependent on stability and middle-class good manners. Perhaps the important feature of this prospersous, decent world was that it was an easy one to rebel against and eventually to leave. It was naturally expected that I should continue the family business and prosper. When I chose not to do so, later, and still in the depths of the Great Depression (1934), it was the radical nature of the change toward an academic career that convinced our two families that we really wanted that world. No one believed that there was such a paid profession as being an astronomer. To teach meant to be poor. This story will move back and forth in time frequently, since my career did not move in a straight line. The period I describe (and only through its impact on myself) is covered in a more complete and unbiased fashion in Struve & Zebergs (71).

After my Harvard AB, October 1929 saw the decisive collapse of the US and world economy. I was faced with a "duty" to help my family survive the business crash. I worked through disasters to our manufacturing and realestate business. People did jump from skyscrapers. The banks closed, business stopped, the streets filled, and strangers spoke to each other. The actions of President Roosevelt in responding with Federal intervention shaped the future; they were resisted but inevitable. During four years I learned a great deal: how to talk to different kinds of people; how to handle money; what poverty did to good people; the unattractive nature of the politics of extremists.

I might have become some type of theorist if an originally planned stay at Oxford had happened in 1929. Certainly I would have been less of a manager and leader. The Depression changed attitudes toward government involvement in education and research. European university life had been part of the State's responsibility, sometimes represented at a high level by a Minister of Education. Technical schools were often linked to the modernization of industry. But in the United States, research groups were largely within the government departments, not independent agencies, and provided no university support. The list is small although honorable. The Geological Survey, the Bureau of Mines, Coast and Geodetic Survey, the Weather Service, the Navy and its Observatory, the Bureau of Standards, and the National Advisory Committee for Aeronautics were among the few successful groups in the physical sciences. An established profession in research, where a job would carry financial support, was rarely part of a budding scientist's expectation. Astronomy was particularly small. Teaching was the usual outlet, normally incompatible with doing much research. Thus I have lived through a complete change of attitude-to the complete Federal support of graduate and postdoctoral education in the 1980s. The present scheme has an emotional unreality to me, possibly to others who lived through the Depression and the wars. It still seems always precarious. Much of my activity in planning for funding of science had its origin in this sense of danger to a delicately balanced enterprise. Only the stronger feeling for the giant opportunity provided by new technologies helped balance my apprehension. But unlike many colleagues, I always looked for and welcomed private support; G. E. Hale had already shown how well such riches could be harnessed. All institutions that have paid me or provided me extraordinary research opportunities were founded or touched by his genius-the National Research Council, Yerkes Observatory, Mount Wilson Observatory, Palomar Observatory, and Caltech. Unfortunately, I never met him.

HARVARD

God offers for every mind the choice between truth and repose. Ralph Waldo Emerson

For undergraduates, astronomy was taught in a building on Jarvis Street (now buried under the Law School), with associated transit circles, clocks, and courses in practical astronomy and navigation. The main text was

Russell, Dugan & Stewart; the professor (H. T. Stetson) went on two eclipse expeditions, and so I did some tutoring. At first there was no contact with the Harvard College Observatory (HCO) staff or knowledge of the rapid growth of astrophysics at HCO. In 1928-29, I attended a lecture course by the HCO staff that covered stellar astronomy, astrophysics, and some extragalactic research. There were a few graduate students at HCO. A young theorist, H. H. Plaskett, had recently come from Canada and impressed me strongly; he arranged that I meet E. A. Milne that summer at Michigan to plan for study at Oxford in 1929. The interests of C. Payne (stellar atmospheres and their composition) and of D. H. Menzel (solar astrophysics and gaseous nebulae) were part of a modernization program to have been lead by Plaskett, but Plaskett did not stay at Harvard. I became closest to Miss Payne, a person of wide culture and astronomical knowledge. The obvious discrimination against her as a woman scientist worthy of normal academic recognition exacerbated the stressful life she led. She was unhappy, emotional, in rivalry with Menzel and Plaskett. But with me, she was charming and humorous as we exchanged quotations from T. S. Eliot, Shakespeare, the Bible, Gilbert and Sullivan, and Wordsworth. Her Stellar Atmospheres (61) is one of the great theses in astronomy.

My undergraduate education was diffuse, covering a variety of topics. The first I heard of quantum mechanics was in lectures by E. C. Slater on visits from MIT. My advisor suggested that quantum mechanics was only one more fad and that I should instead take more classical physics! There was no physics experimental lab, and in fact I had lost my early gadgeteering interests. I listened to A. N. Whitehead on the philosophy of science and the foundations of mathematics. An illness prevented my trip to Oxford, so I stayed for the AM (1930), working at the Observatory. My first research was on the temperature scale for B and O stars. I planned to use the mean "color equivalents," c_2/T , tabulated by E. Hertzsprung (and others) from visual and photographic magnitudes, to determine a main-sequence temperature scale. Miss Payne had noted the abnormally low color temperatures of O and B stars, in contrast with those indicated by their lines of high ionization and excitation potential (using Saha and Boltzmann theory). I found their mean color temperatures to be lowest at right ascensions of 3 to 7 hr (notably in Orion) and again at 17 to 21 hr. What I had found was the general interstellar reddening of B stars by dust in the galactic plane. Instead, I explained away the reddening as a seasonal systematic error in atmospheric extinction. Why? First, the concept of air mass had just been developed by the meteorologist C. G. Rossby and was fashionable. Second, H. Shapley claimed to have disproved interstellar reddening, since he had found blue faint stars in the Milky Way using

Selected Area magnitude sequences. In addition, reddening by Rayleigh scattering would have severely distorted energy distributions (λ^{-4} rather than the λ^{-1} observed). Both ideas, although fashionable, were irrelevant; Shapley's observations were incorrect. I lacked the independent wisdom to establish the existence of interstellar reddening in my first paper (15). I remember the excitement at HCO when R. J. Trumpler's paper on galactic clusters (73) arrived, showing that a general interstellar absorption existed. Trumpler records his own unwillingness to accept general reddening at first, although he had proved absorption to be real. He notes that Wallenquist (77) had also detected reddening in a galactic cluster. There was a lesson to be learned, but science has always provided an ample lifetime quota of such shocks for would-be pioneers.

During the depression years in New York City (1930-34), I luckily met the physicist I. I. Rabi; in his usual no-nonsense style, he asked me what a bright boy was doing in the real-estate business. He offered to let me do volunteer work in his laboratory at Columbia, designing and winding deflection coils and computing particle trajectories. Since his work for the Nobel Prize was done in 1937, my fear of electromagnetic theory may have been unfortunate. Rabi introduced me to J. Schilt, who also offered volunteer work. He had 75 plates of the globular cluster Messier 3, taken with the Mount Wilson 60-inch in 1926; I searched for new RR Lyrae variables and gave periods (16) and light curves for 199 stars. W. J. Eckert at Columbia was using the first large array of IBM mechanical calculators for celestial mechanics. My work on Messier 3 was monotonous, carried out by eve and using hand calculators, but it convinced me that I had more love for astronomy than for money. I visited Shapley at Harvard, who told me that science had advanced too rapidly for me to hope to catch up. I persisted, and returned for the summer school of 1934. The total financial support was a \$400 scholarship in my second graduate year and \$700 in my last.

During my four-year absence, HCO had changed radically; groups of graduate students were associated either with Menzel or B. J. Bok. A detailed history of the years 1930–39 at HCO would be a fascinating study in the growth of US astronomy and in the training of a generation of leaders. In retrospect, HCO now appears to have been unfortunately isolated from the great observational efforts at Mount Wilson and Lick, possibly because of some past hostility to Shapley. HCO students were expected to learn a little about everything. Some strengths of the Observatory were in its patrol-plate collection covering both hemispheres, the Henry Draper objective-prism spectra, and the photographic survey of the Magellanic Clouds. Few students were involved with Shapley's extragalactic programs. In the liveliness of the HCO approach to a wide variety of astronomical topics, and its inheritance from sky surveys, one could disregard the modest quality of the telescopes. H. N. Russell, active in laboratory spectroscopy, analysis of complex spectra, and links with the new atomic physics, was a frequent visitor from Princeton. Among the summer visitors, O. Struve had the sharpest impact on me; he showed what seemed then an almost excessive regard for astrophysical depth and details of the interpretation of binaries. He also used atomic physics; with him, stellar astronomy was clearly part of current physics. From Menzel I learned the physics of ionized gases and recombination theory, together with more atomic spectroscopy. Many of Menzel's students became leaders of American astronomy, e.g. L. H. Aller, J. G. Baker, L. Goldberg. With Bok were E. Lindsay, S. W. McCuskey, F. D. Miller, and C. K. Seyfert. F. L. Whipple, with a West Coast background, was then a young instructor. He already had a devouring interest in planets, comets, and meteors. Shapley, along with other senior US astronomers, had begun to take part in rescuing scientists from Europe; some of these became our close friends.

I was briefly in Bok's star-counting circus but counted only a few stars. I shared his interest in the absorbing dust clouds and undertook observations to determine the interstellar-reddening law and the physical theory of reddening. Three papers (17-19) came from my thesis. I applied the classical theories (dating from 1908) of G. Mie and P. Debye to compute the extinction of light by dust particles smaller than or near the wavelength of light. Such computations had been carried out by C. Schalén (in Sweden) in 1929 and by E. Schönberg and B. Jung (in Germany). It was a mathematical boundary-value problem, which except for small particles requires computation of a series of multipole amplitudes using special functions (which were often not fully tabulated). I computed the integrated extinction by a power-law frequency distribution of particle sizes. The observed reddening law was derived from the calibrated photographic spectrophotometry of objective-prism spectra of 38 B stars, obtained with the 24-inch reflector at Agassiz station on a few, cold and fortunately clear nights. The ratios of fluxes between reddened and nearly unreddened B's gave an extinction law near $\lambda^{-0.7}$. In my thesis, I mention photoelectric photometry by J. Stebbins and C. M. Huffer; later this method was used, with six photoelectric colors, by Stebbins and A. E. Whitford. I studied the radiation pressure on grains, important for the interstellar medium as a whole only if interstellar hydrogen is neglected. Star counts by Bok's group had established the existence of dense dark nebulae, which I grouped into five large "cloud complexes." These are essentially groups of giant molecular clouds, within which much of the general absorption arises. The grains for which I did computations included metals, silicates, and frozen water (the latter two having a high ratio of scattering to extinction, i.e. high albedo) important for reflection nebulae. In 1937 the gas producing then-known interstellar lines seemed only a trace constituent. At my oral examination, Shapley asked me how to find interstellar hydrogen, rather than dust. I suggested recombination lines (eventually seen in the H II regions) and subordinate lines of H I (such as Struve had found in circumstellar shells). I did not mention $L\alpha$ which seemed hopelessly unobservable; it was 20 years before Sputnik.

A new venture closes my Harvard adventures; I had a long interest in amateur radio. A flurry of publicity (May 1933) marked K. Jansky's discovery of "cosmic static"-but I didn't notice it. His other papers, published in engineering journals, had little effect on astronomers. I have written elsewhere (for the fiftieth anniversary celebration of radio astronomy) about my involvement. Fred Whipple and I attempted to explain Jansky's radio signals by thermal emission from dust in the galactic center; we assumed that the space densities of stars and dust grains increased inward by a steep power law. We (78) used S. Chandrasekhar's radiationtransfer formulation for spherical atmospheres to derive the maximum dust temperatures in the Galaxy's central blaze of stars and dust, but we could not reach antenna temperatures above 30 K, even with 10,000 times the radiative energy density in our part of the Galaxy. Our purely thermal explanation failed to explain Jansky's observed fluxes by a factor of 10,000. At that time, neither relativistic particles nor magnetic fields were conceivable parts of the astronomical repertory. A polite editorial in the Boston Evening Transcript gives us credit for a very large failure, but it was a failure that persisted for 15 years.

EARLY YEARS AT THE YERKES OBSERVATORY

I was fortunate to obtain a National Research Council Fellowship for 1937–39, one of the few available in the physical sciences. The stipend was \$2200 and permitted a choice of where to work. I went to the Yerkes Observatory of the University of Chicago at Williams Bay, Wisconsin. It was indeed a change of scene. When my wife and I drove from Harvard, we reached—at 20 miles past the Hudson River—the farthest point west we had traveled, although we had traveled extensively in Europe. Williams Bay was a town of 600 people on a beautiful glacial lake in farming country. Yerkes and Harvard differed as much as did their landscapes, and I have benefited much from both. Yerkes was entering into its great period, with an expanding faculty and instrumentation plans for the McDonald Observatory near Fort Davis, Texas. Yerkes (1897) was the first observatory built by Hale; his 40-inch refractor was its main instrument, and his taste dictated its elaborate architecture. My fellowship was from an organization born from his revitalization of the National Academy of

Sciences after World War I. But Yerkes was fully reshaped under Struve's leadership. I believe it was the first US working observatory to have theoretical astrophysicists on its staff. Struve also imported onto the Yerkes staff leading astronomers from abroad such as Chandrasekhar, G. P. Kuiper, B. Strömgren, P. Swings, and visitors like A. Unsöld and K. Wurm. Elsewhere, among observing astronomers, xenophobia was not uncommon. Another innovation was the construction of the 82-inch reflector at McDonald as a joint project of the Universities of Chicago and Texas. Completion of the 82-inch in 1939 marked the first major telescope construction since the Mount Wilson 100-inch in 1918 and the beginning of the migration to the good observing climates of the Southwest.

Yerkes was 80 miles from the University of Chicago and 60 miles from the University of Wisconsin. Graduate students lived in the Observatory and thus lacked exposure to contemporary physics. Courses were given by Yerkes staff and visitors; research was at first only with the 40-inch, later with the 82-inch. The use of the 40-inch was romantic, exhausting, and often cold. Having new optics in development for the 82-inch (with F. Ross as advisor) made it more common to experiment with new instruments than at Harvard. Struve and Kuiper were interested in state-of-the-art developments, although at first neither had the experimental skills common on the West Coast. Struve believed in quick responses to new opportunities; he had been interested in the airglow and in faint surface photometry and spectroscopy. Some new advances were the McDonald coudé, the nebular spectrograph at Yerkes, a Fabry photometer on the 40-inch, some photoelectric photometry, near-infrared photography, and later Kuiper's near-infrared spectroscopy. Important scientific neighbors were Stebbins and Whitford, the first of the converted physicist-astronomers I met. Young scientists at Yerkes included W. W. Morgan, an artist in spectral classification (whose work could be done with the 40-inch), and L. G. Henyey, a theorist with whom I became closely involved. Our first paper (44) was based on the newly invented nebular spectrograph. We collaborated on eight observational and theoretical papers and five (classified) reports on optical design in a few years. His perfectionism blended with my somewhat coarser energy into confidence that we could finish anything we tried. I have been lucky to enjoy such well-matched collaboration often. Henyey's life was unfortunately shortened by an illness he had suffered from since childhood. His Yerkes thesis was on reflection nebulae and showed elegant mathematical skill. The observed sizes and surface brightnesses of the nebulae required high albedo; my thesis suggested that the dust could be ice or silicate glass. We solved many difficult radiation-transfer problems analytically, some with methods related to V. A. Ambartsumian's and Chandrasekhar's invariance and reciprocity theorems. We used the Fabrylens photometer at the focus of the 40-inch refractor, setting on empty space between the visible stars in the Milky Way, to measure the diffuse galactic light (46). This was stimulated by Struve and C. T. Elvey's discovery of the high surface brightness of a dark nebula. We found the dust to have a high albedo and a forward-throwing phase function. The birth of the Yerkes nebular spectrograph may illustrate the style of Struve's leadership. On a cloudy night when I was assigned the 40-inch, I found Struve and Henyey in the library. Struve posed the question, "In principle, what is the most efficient possible spectrograph?" We decided it was one with the fewest possible components, ultraviolet optics, and the fastest focal ratio camera. We set a slit on top of the far end of the 40-inch tube, omitted the collimator, and put a wooden box with the McDonald quartz prisms and f/1 Schmidt at the eye end, 69 feet away. I used it two nights later on another cloudy night; a long exposure at the zenith gave a magnificent spectrum of an aurora. Spectra obtained of emission and reflection nebulae were also exciting (44, 45). H α was nearly everywhere in the Milky Way, with known emission nebulae often only brighter patches. Henyey's spectrum of Comet Encke shows NH and OH strongly, now first made visible by the UV optics. An improved 150-foot-long nebular spectrograph was built at the unfinished McDonald Observatory site; with it, Struve and Elvey completed the discovery of H II regions, soon after explained by Strömgren. The nebular spectrograph permitted my brash first venture into extragalactic astronomy and cosmology in a paper (20) written only 13 months after I had left Harvard, early enough to be only the tenth McDonald Contribution. It used the energy distribution of Messier 31, measured on 7 spectra, to determine temperature from the spectrophotometric gradient, c_2/T , on the Greenwich system. The galaxy had a roughly blackbody energy distribution, at 4200 K, with some ultraviolet deficiency. Comparing this distribution with photoelectric colors by Stebbins and Whitford gave me the range of color temperatures for E to Sc galaxies as 4900-6200 K. A serious cosmological problem then arose in reanalyzing Hubble's work on the effect of redshift on galaxy magnitudes, since these temperatures were much lower than those that Hubble had used. Galaxies therefore dimmed too rapidly with increasing redshift to be compatible with the Hubble counts as a function of magnitude. I determined the redshift corrections as a function of $d\lambda/\lambda$. When I later met Hubble, I was at my boldest. In 1930, I had failed to use color temperatures to discover interstellar reddening; by 1939, I had to believe in good observational data. This was a most valuable lesson : believe in data, improve data, try new experimental techniques, and (at least for me) try to do a first theoretical interpretation. While still limited to using photographic techniques, I could see that a "new" astronomy existed that had always to be renewed. Important new directions with a different instrument soon opened for me.

The McDonald 82-inch (now renamed the Struve telescope) was dedicated in May 1939. Under the shadow of World War II, Europeans enjoyed a Texan barbecue and rode o. I met Milne again; for the first time, my wife and I met W. S. Adams, W. Baade, E. P. Hubble, R. McMath, J. Oort, and A. Unsöld, all of whom played an important part in the future of astronomy. On the first 82-inch observing run, I helped Struve take coudé spectra of τ Scorpii for Unsöld to analyze in Germany; these became testing grounds for improved composition analyses by successive generations of Unsöld students.

That year effectively ended my youth—I passed 30, my interests changed, and the war altered my life. Struve said that high-dispersion spectroscopy was exciting and possible and that v Sagittarii was interesting. The 82-inch coudé prism spectrograph rivaled that which T. Dunham was developing on the Mount Wilson 100-inch. I obtained both coudé and cassegrain spectra to determine abundances over a range of temperatures, gravities, and apparent composition anomalies. Most important was the analysis (21) of v Sagittarii. This hydrogen-poor object provided the second quantitative analysis of a star of abnormal composition; L. Berman had recently analyzed a carbon-rich star, R Coronae Borealis, in a pioneer study. My first analysis at coudé resolution (22) was of α Carinae—a star far south, even for McDonald. Its composition proved normal, although it was a supergiant; that paper outlines the practical method of differential-curveof-growth analysis, what Unsöld called "grobanalyse." Since few atomic parameters were then available, the transition probabilities were derived by hook or by crook, using solar gf-values obtained by Menzel and Goldberg, strengths from supermultiplets or transition arrays, and opacity theory to set the atmospheric mass. No models existed-the ionization and excitation temperatures had to be estimated from metallic lines. Reliable spectrophotometry of the continuum, which would be used nowadays to determine the effective temperature, did not exist. It was v Sgr, with a dominantly helium atmosphere, that introduced me to complex problems in stellar spectroscopy and atmospheres. Plate IV of the v Sgr paper (21) is still one of the most dramatic illustrations of an important composition abnormality; it was so used by Burbidge, Burbidge, Fowler & Hoyle in 1957 (3). The curve of growth was made more sophisticated by Unsöld's methods of weighting functions and thermal stratification. The book Spectroscopic Astrophysics (47) recapitulates Struve's early work in stellar spectroscopy, as well as the later research stimulated by his work.

On 7 September 1941, the fiftieth anniversary of the University of

Chicago and three months before Pearl Harbor, the American Astronomical Society (J. Stebbins, President) met in Williams Bay during a 5-inch rainstorm. The dedication of Yerkes Observatory had occurred in 1897, leading to the founding of the American Astronomical and Astrophysical Society (S. Newcomb, President) in 1899. For the meeting, I arranged an exhibit of 50 years of astronomical photography, including 30foot-long blowups of coudé spectra from both Mount Wilson and McDonald that covered walls of the 40-inch dome. The group photograph (4) brings back pleasant memories. There are past and present leaders; friends there included G. Randers (later head of the Norwegian atomicenergy project), J. S. Hall, W. A. Hiltner, and L. Spitzer. These names bring hints of the future in plasmas, magnetic fields, and electronics. At the meeting, the AAS took responsibility for publications; the Astronomical Journal was transferred from Dudley Observatory to Yale (with D. Brouwer as editor), the Astrophysical Journal to the University of Chicago Press (with editor Struve, later Chandrasekhar). Photoelectric photometry had spread; Stebbins and Whitford had improved the interstellar reddening law over my photographic determination. Eight years later, Hall and Hiltner independently discovered interstellar polarization. Before the meeting, I had become a friend of G. Reber as I revived my interest in radio astronomy. The technology-centered growth of modern observational astronomy was starting.

In the front row of this group is my wife, Naomi, fully occupied both as manager and actress at a summer community theater and as mother of a one-year-old son (George; now professor of astrophysics at Amherst College). Our second son, Peter, was born in 1946; he is now active in music and drives a mobile library near Oakland. But informative and nostalgic as it may be, the picture is a prelude to large, and sad, changes that followed the 1941 meeting of the AAS.

YERKES: THE WAR YEARS

Struve was strongly oriented toward international problems of astronomy and had close ties with Europe. He saw war as inevitable and was concerned that his staff would join large laboratory engineering groups elsewhere. Several young astronomers had left Yerkes and Harvard for defense well before Pearl Harbor. He felt that the future of astronomy was threatened unless astronomers stayed together, proffering talents in research groups. The military draft also threatened to produce an unfortunate loss of scientific talent. A discussion of Struve's efforts, and his correspondence with Shapley and Russell, is in De Vorkin's (9) study of the Yerkes Optical Bureau. A more general discussion of astronomers involved with military

optics is in Dunham (10), who headed the section of the Office of Scientific Research and Development (OSRD) with which I became involved. De Vorkin quotes extensively Struve's (70) pessimistic, but farsighted, thoughts on problems concerning the intellectual survival of astronomical research. Until I read De Vorkin's account, I did not know how intensely Struve had struggled to keep the telescopes in operation and to retain a group at Yerkes. By mid-1942, Henyey and I (with D. Popper, briefly, and G. Van Biesbroeck) were designing lenses and optical systems. Later F. Pearson, under Hiltner, provided a working optical shop. I negotiated with Dunham, the OSRD, or potential military users to clarify an idea for a needed instrument. The OSRD had other, larger groups at Harvard, Mount Wilson, and Rochester. No satisfactory texts on optics existed; optical aberration theory was taken from an old National Bureau of Standards (NBS) handbook. US industry had longstanding proprietary relations with Germany. Most of our effort was spent fitting requirements on space, weight, and materials; much was wasted due to rivalry under conditions of secrecy. I retain little affection for the optical industry.

We had much to do. Henyey could outline the mathematical theory of a required lens system, often overnight; within limits, optimum lens systems could be quickly defined. But the tedium of ray tracing with hand-cranked calculators is incredible; in 90 seconds we could trace a single ray through an optical surface with six-digit accuracy, interpolating in trigonometric tables. Two independent computations were needed. We soon developed a more efficient ray-tracing method. I struggled with priorities for rare glasses and machinery. I carried one-of-a-kind optics to a proving ground and remember sleeping on a misshapen box containing a gunsight. We designed and built extraordinarily complex lenses-including one for photography through a periscope. But radar was taking over, making optics a secondary solution. As during the depression, I learned much about new types of people, in this case the military, and benefited. Scientific spinoffs were the development of ultrafast lens and mirror systems and the use of unusual materials. Before Henyey left for Berkeley in 1947, we designed a fast system for X-ray fluoroscopy of the digestive system for the Billings Hospital in Chicago. We obtained patents, one of which helped to lead, after several changes, to wide-screen movie projection. (I still own 7500 shares of worthless stock in such an enterprise.) Several of our devices are in museums, including the f/2 140° wide-field camera. With this, our Galaxy was photographed as an edge-on spiral with a central dust lane.

The Yerkes staff was severely depleted by the war, but telescopes were fully operated. I had several observing runs at McDonald but no time to think. I was not drafted; I got to know some senior officers. Like other scientists who "had won the war," I was unconsciously being prepared for major changes in my scientific career and outside activities. At Yerkes, before 1941, the younger staff had not been given planning or operating responsibilities —that was not Struve's style. Yet for reasons of personality, availability, and some outside friendships, I found myself involved in new organizational structures for astronomy. By 1947, the Office of Naval Research (ONR) had instituted a small grants program, and I was on that committee. Somehow, before the birth of the National Science Foundation (NSF), colleagues and I were involved in its plans, and I became the first chairman of its astronomy advisory committee. Scientists returned to Yerkes with new gadgets. Kuiper learned to use a PbS photoconductive Cashman cell, leading him to infrared planetary spectroscopy at McDonald by 1946.

Because of his work, Reber was familiar with radar. He and I wrote the first résumé of the rapid pace of discovery in high-frequency electronics and its applications in radio astronomy (62). W. T. Sullivan's (72) history of that field includes my account. Henyey, P. C. Keenan, and I computed the freefree radiation of H II regions and failed, again, to account for the intense low-frequency power in the Milky Way. A book edited by Kuiper (49) records an epochal symposium marking another new technique for astronomy: research from space. It includes a brief account of the traumatic, unsuccessful flight, in 1947, of a high-resolution solar spectrograph I built for a V2 rocket. Funded at \$7000 by J. Van Allen's Applied Physics Laboratory at Johns Hopkins, it made me the first, but certainly not the last, conventionally trained astronomer to have an experiment fail in space! I predicted the continuous ultraviolet spectrum of the Sun as depressed by absorption lines and continua, with quite low boundary temperature (49). The book in which this paper appears includes many new names: Van Allen, R. Tousey, E. Durand, J. J. Hopfield, H. Tatel, H. Friedman. The sponsorship of astronomy by the NACA, the predecessor of NASA, opened a new road. Rocket scientists of the Naval Research Laboratory, the Bureau of Ordnance, together with old hands in astronomy like Goldberg and Whipple, created an advisory apparatus. The space program thus started with scientific goals in which some astrophysical, as well as solar-system, problems were recognized, although it was a long and difficult struggle that still continues.

An important feature of the success of astrophysics since 1945 has been its assimilation of physicists and engineers with other formal backgrounds. It succeeded in converting them into hyphenated-astronomers. Fortunately (and it was an actively discussed problem), the AAS and the *Astrophysical Journal* co-opted such people to present and publish their work. New areas of technology and relevant physics had to be understood by older astronomers; they were, but with omissions. The new fields were co-opted. I had been anxious to see radio observations part of astronomy; a letter from me to Struve (1946) explains some practical difficulties facing us were we to undertake the new venture and make a radio-astronomer faculty appointment at Yerkes. They involved military and industrial secrecy, overhead, engineering-level salaries, and operations at McDonald with Navy funds. The project fell through, but a few years later, ONR and NSF funds were available, and Harvard, Michigan, Cornell, and the Carnegie Institution of Washington entered that field.

Theoretical astrophysics was less directly affected, but new concepts entered this field, notably from hydrodynamics, turbulence, shocks, and convection. Theoretical problems arising from observation are seldom at a level of abstraction requisite for mathematical solution. But observations of the solar flux and limb darkening confirmed theoretical distributions of temperature with depth (6). Theoretical astrophysicists became increasingly confident and broad in fields such as atmospheres, galactic dynamics, the patchy interstellar medium, and the internal structure, energy generation, and evolution of stars.

Before modern computers, a model atmosphere remained a serious task. For stars, the empirical approach is exemplified in papers (23, 24, 38) based on McDonald coudé spectra. The compositions of Am (metallic-line) and normal F and G stars were determined by the differential curve of growth. The method was used for 15 years until model atmospheres became available and "coarse analysis" was replaced by "fine analysis." I found abundances relative to the Sun for a dozen stars, with an accuracy of ± 0.3 dex (rarely better, often worse). The Am stars, however, had apparent deficiencies from +0.5 to -1.5 dex. Morgan (55) had discovered many complex peculiarities among the A stars, which are now generally called "chemically peculiar" stars. His results should have suggested that we would probably soon exhaust theoretical explanations of such anomalies if they were taken as deep-seated. Most F and G stars had nearly solar composition; I attempted to explain away the Am phenomenon by anomalous charge-transfer ionization, rather than composition. The current explanation is a competition between gravitational diffusion and selective radiation pressure in nonconvective atmospheres. The important fact is that Morgan's chemically peculiar stars were all in the nonconvective range of surface temperatures, although the explanation did not come for 20 years.

TRANSITION

... but something ere the end, some work of noble note, may yet be done, Alfred Lord Tennyson, Ulysses

The uniformity of stellar composition, coupled with some variety (as in R CrB, v Sgr), led to exploration of the effects of nuclear physics on

composition. Magnetic fields in space were used by E. Fermi to accelerate cosmic rays. Spitzer enlivened an AAS meeting by introducing plasma physics and doing an illustrative "dance of an electron in a magnetic field." Among World War II advances was the RCA photomultiplier tube, the 1P 21, which made everyone a master of precision photometry. In this epoch of new ideas I was faced with a personal decision; Struve had relinquished the Yerkes directorship, Henyey had left for Berkeley, and I had received several tempting offers involving leadership. My family had lived 11 years in a country village; we missed the intellectual and cultural activities and the more active life of the city. I was asked to come to Caltech to help it prepare for operation of the Palomar Observatory, which it owned, and to create a graduate school and gather the scientific staff for Palomar. We left Yerkes with genuine regrets. I arrived in Pasadena in June 1948 for the dedication of the 200-inch, prepared to become involved in enormous changes. I entered a world in which I had to become two persons-scientist and organizer. Struve ran and financed Yerkes. I had not wanted to be involved, nor was I consulted, in organizational matters at Yerkes; I had not raised money, nor had I been an active committee member. Now, however, organization, administration, and relations with the university, the government, and the public became at least half my life. But I decided, firmly, not to abandon research, which also took more than half my life. In 1949, at age 40, the bibliography of my published papers numbered 73; in 1959, there were 158; in 1969, 261; and in 1979, 353. A few are wrong. Perhaps 20 percent are ephemera or related to the public side of science, including encyclopedia contributions, review articles, and published committee reports. The numbers show that I remained an active scientist, good or bad, but prolific.

I no longer remember how the balancing act was possible; in a running biography, there are about 75 named lectures and memberships on 50 major committees and study or advisory groups. At one time I held over 20 simultaneous committee memberships. Had I stayed at Yerkes, I would possibly have been a better astronomer, but I had already traversed most of the road from near-theorist to observer. My experience was far from unique in my generation. Historians will need to study the broadening of technology, the changes of institutional arrangements, and the daily novelty (which was somehow to be fully funded) that became an expected feature of scientific life. Contemporaries managed in different ways to remain active scientists under overwhelming pressures. In Europe, university life and research had been part of the government apparatus. In the US, in 30 years we lived through a culturally diverse transition to a pluralistic arrangement in which some older, independent units flourished, while completely new institutions, observatories, and consortia appeared. In optical astronomy, one major given fact was that large telescopes existed

before Federal support was available. In radio and space astronomy, the Federal role immediately had to become central.

CALTECH AND THE MOUNT WILSON AND PALOMAR OBSERVATORIES: ORGANIZATION

 What means were there to examine what it was like before heaven and earth had taken shape?
 Who planned and measured out the round shape and nine old gates of heaven?

Anthology of Heavenly Questions (China, 4th c. B.C.)

The California Institute of Technology (CIT) is a young institution with a historic devotion to research, tracing back to R. A. Millikan, G. E. Hale, A. A. Noyes, and T. H. Morgan. The post-WWII growth of research in other universities makes it now seem less exceptional than in the past. Fine, and few, students and a relatively large faculty are fortunate characteristics. Another major factor in its development as a center of astronomical research is the 200-inch, built on Hale's persuasion and given to CIT by the Rockefeller General Education Boards. The success of the 60- and 100-inch reflectors at Mount Wilson had led to planning for the 200-inch by astronomers of the Carnegie Institution of Washington (CIW). CIT and CIW joined in a full partnership agreement for the joint operation of the Mount Wilson and Palomar observatories (MWP) the year I arrived. The agreement was negotiated during the Mount Wilson directorships of W.S. Adams and I. S. Bowen. Bowen, who had been a CIT professor of physics, became director of the combined observatories. Each institution fully supported its own mountain and staff; the staff were to use the instruments of either mountain as their science required, as were CIT graduate students. The director had his offices at Santa Barbara Street in Pasadena, two miles from CIT. Management of the final Palomar construction and instrumentation was under the direction of B. Rule (a CIT engineer) in the Robinson Laboratory at CIT, where I established the new teaching and research faculty. Bowen did much of the optical design and supervised the final refiguring of the 200-inch mirror. The CIW staff was large but aging; it included outstanding observers of whom I name only a few: W. Baade, E. Hubble, M. Humason (comprising the nebular group); A. H. Joy, P. Merrill, R. Sanford, O. C. Wilson and R. E. Wilson, H. D. Babcock and, soon, H. W. Babcock (spectroscopists); S. Nicholson, R. Richardson (the solar group). CIT had only F. Zwicky and J. Anderson (who had run the 200-inch construction project after Max Mason). I was responsible through the dean of the faculty (E. C. Watson) to Millikan and soon to CIT's new president, L. A. DuBridge, with whom I had excellent rapport. The organizational structure was, and remained, mysteriously complex, but it worked. The Observatory Committee (of which I was a permanent member) advised the director on MWP matters. Faculty appointments at CIT needed approval by the normal chain: approval by the Observatory Committee and by the presidents and trustees of both CIT and CIW. The budgets had two fully independent sources, and both needed joint approval; CIT paid the operating expenses of Palomar and its faculty salaries. Later, with Federal assistance, CIT established the Big Bear Solar Observatory and the Owens Valley Radio Observatory, the latter independent of MWP and reporting through the Caltech division chairman (R. F. Bacher for many years). Astronomy was one of three "options" within the Division of Physics, Mathematics, and Astronomy. Much astronomical research was conducted within Physics: cosmic ray, X-ray, radio, and infrared astronomy, theoretical astrophysics. In Planetary Sciences were geochemistry and the advanced camera developments, such as the CCD for the Space Telescope. Astronomy spread over five other buildings and to the Jet Propulsion Lab. Some teaching was to be done, at first by volunteers from the CIW staff; some theses were done under their guidance. This management plan was hopelessly complicated; it worked for a long time because of loyal devotion to science and the marvelous telescopes available. My major task was to suggest and carry through (although with difficulty) the new appointments. In the Robinson Lab I inherited a five-story building designed by Hale, filled with a 15-year accumulation of squatters from other departments and with laboratory equipment from another epoch. I threw out mountains of junk and evicted tenants, including the Dean of Graduate Studies and the Department of Mathematics. There was an uncompleted solar tower; now we have the Big Bear Solar Observatory. Robinson has since overflowed physically, with computers (three VAX's and the VLBI processors on two floors outside the old building frame), laboratories, and shops. An ambitious plan for two new large buildings, one for CIT and one for CIW, foundered, unfortunately, when CIW decided to build (with private funds) the Las Campanas Observatory in Chile, which has proved to be an outstanding success.

On arrival, I taught the graduate (full-year) courses on stellar atmospheres and on stellar interiors; some appealed to graduate students in physics, an advantage of a divisional over a departmental system. These classes were small; later I taught a one-term introductory course for sophomores, usually 50 brilliant boys, from whom I recruited several future astronomers. The backbone of CIT graduate instruction was in mathematical physics, also required of astronomy students; the courses used problems of numbing difficulty to strain all minds and, it was hoped, to train the best. The survivors knew an order-of-magnitude more physics than my

generation had been required to learn. In the first ten years we were fortunate to have some of the best students in astronomy and to graduate many of its present leaders. I took more than paternal interest in them all, although few theses were actually done with me, since I was spread far too thinly. The department was rated highest in several evaluations by the Council on Graduate Education. The early faculty choices included several from Yerkes, largely those with theoretical abilities. I had found that a reformed theorist may become the most intelligent observer. Another factor was a change in the type of person appointed at CIW and to the joint MWP staff. Some were Caltech PhD's, others had substantial theoretical as well as instrumental abilities. The CIW staff had shrunk through retirements, and for financial reasons its earlier size could not be maintained. The size of the total CIT and CIW staff barely approached that of Mt. Wilson in the 1930s, in spite of the doubling of large telescopes available. One effect of the joint operation was that CIT astronomers were precluded from receiving government money for research, since CIW would not, on principle, do so for many years. The combined largest observatory in the world was fully privately supported and was always short of money for new instrumentation and for postdoctoral fellows. Beginning in 1957 (until 1970), after something of a crisis, I arranged substantial funding (by the Air Force Office of Scientific Research) of an "Abundance Project." Caltech is currently one of the largest university recipients of Federal funding for its radio and solar observatories, infrared experiments (including IRAS), the submillimeterwave interferometer, and Palomar advanced instrumentation. But Palomar is still mostly privately funded. (A recent example of such funding is the upgrading of the 48-inch Schmidt for the repetition of an improved Sky Survey.) I am now in principle retired (since 1979), after having resigned my management tasks in 1972. I cannot here describe the work of my younger colleagues, whom I admire, because of too close personal involvement. Many were students or postdoctoral fellows. For the same reason of closeness, I should not assess the contributions to astronomy by the Mount Wilson and Palomar Observatories. I believe them to have been very significant, and hope that they still are. They fulfill many of Hale's dreams, even if in fields he could not have foreseen. They combine development of the best possible instruments with extended programs of difficult observations and interpretation.

EARLY RESEARCH IN PASADENA

The emphasis in the 1940s on plasma and magnetic fields in space helped lead to an understanding of interstellar polarization and its implications for the magnetic field in the Galaxy. Knowing how to compute extinction by interstellar grains, I found a perfect collaborator in a younger physicist, L. Davis, Jr. He displayed the merits of the Caltech emphasis on classical mechanics and electromagnetism. His example persuaded me to force later generations of graduate students to take such courses. We enjoyed a lively controversy with L. Spitzer. In Davis & Greenstein (8), the alignment mechanism for rapidly spinning, elongated grains is thoroughly discussed. The fields deduced lay (correctly) along galactic spiral arms. Davis later studied the trajectories of cosmic rays in fields with small-scale deviations from parallelism and the morphology of the fields as deduced from maps of polarization. The Spitzer-Tukey suggestion of a ferromagnetic contribution to the relaxation process and, much later, Purcell's ideas may save the theory when the composition of the grains is better known. Our treatment seems to be one of the few in astrophysics relevant after 30 years.

The next learning experience was in nuclear physics, with W. A. Fowler as guide. The Kellogg Lab had been studying low-energy cross sections relevant to stellar energy production. My interest in anomalies of stellar composition meshed with the work in Kellogg. With various collaborators, using Mt. Wilson, Palomar, and solar spectra, I studied (1950-56) the abundances of ¹³C/¹²C, ³He/⁴He, ⁶Li, ⁷Li, Be, and Tc. The ¹³C/¹²C ratio in most stars, and even in a comet, seemed close to that in the Sun. The Li/H ratio in young stars was found elevated, and the ³He/⁴He ratio was high in some chemically peculiar stars. In much of this work, in retrospect, the hope of linking a surface abundance anomaly to specific, exoergic nuclear reactions in the interior seems naive. Early reviews (25, 26) describe the subject when hopeful and new. In a lecture (26) I noted that the exoergic ${}^{13}C(\alpha, n){}^{16}O$ reaction was a possible neutron source. A. G. W. Cameron explored chains of neutron captures in detail that explain the heavyelement stars and S-type giants containing technetium, which P. Merrill had found. I developed a somewhat philosophical, vintage-1952 plan for studying energy-generating reactions as the source of peculiar stellar composition. From 1957 to 1970 I published (with many collaborators) 60 papers in this field, largely with AFOSR support. Many US and foreign colleagues came to Pasadena, observed at Mt. Wilson and Palomar, or used my available spectra. There was also much theory, some agony, and much fun; in January 1970, a committee (G. Wallerstein, W. L. W. Sargent, and L. Searle) organized a surprise symposium on the "Chemical History of the Galaxy" for my 60th birthday. Pagel (60) has given a partial résumé, but unrecorded are glimpses of old friends convulsed with laughter at some particularly outrageous remark (usually by Geoff Burbidge, Willy Fowler, or Jerry Wasserburg). I have a pleasant letter from Unsöld (who really did not believe much in stellar nucleosynthesis), with his watercolor of Robinson Lab. There is an unpublished paper by P. Conti and A. Schadee

on "The Presence of Greenstones in White Dwarf Stars," with abstract as follows:

The fact that greenstones have a half life of 60 years suggests that nucleosynthesis from "teeny-tiny" bangs (Waggoner 1970) has occurred recently in white dwarfs.

I am proud to count the many postdocs who came through the Abundance Project, with the early graduate students at Caltech, as among my valued scientific foster-children. The quotation from Conti & Schadee correctly foreshadows the end of my work on stellar composition and the subsequent shift into white dwarfs, where, alas, greenstones have not yet been identified. The general theory of nucleosynthesis in stars (3) was based on what is really quite early knowledge. Abundances have since been determined from steadily improved models (made possible by the computer) and spectral synthesis (e.g. by R. A. Bell) and depend on electrooptical, higher-resolution data (e.g. by D. Lambert). Composition analyses of stranger and fainter stars and abundance gradients in the composite spectra of galaxies completely depend on new technology.

The metal-poor subdwarf G stars studied by L. H. Aller and myself in 1960 were of 9th magnitude; the 14th magnitude globular-cluster giants studied at 18 A mm⁻¹ dispersion by L. Helfer, Wallerstein and myself in 1959 took me longer than a full night to obtain exposures at the coudé, which illustrates the limit of photographic spectroscopy. The 200-inch coudé was designed by Bowen, with a series of Schmidt cameras made by D. Hendrix and a mosaic of original gratings by H. W. Babcock. When it began operation in 1952, it was a superb, ultimate instrument made inefficient by photographic plates and by slit-losses when used at high resolution. Work on faint stars that no one else could observe seemed a proper use of the 200-inch. The prime-focus spectrograph used dark-ofthe-moon time, competing with the high-priority programs of Baade, Humason, Minkowski, and Sandage on galaxies and clusters. But it opened up work on faint stars of low luminosity and on quasars. This change in subject matter reflected a pattern in my research. I enjoyed exploring a new speciality in order to learn a new area of astronomy and its related physics; I tended to leave a field once it was well established as a result of my low threshold for boredom and my inability to resist use of newly available equipment on a new type of object. In 1957, for example, I took the first high-resolution spectra of comets at the Palomar coudé. I wrote a halfdozen papers with collaborators and found the rotation of a comet by a generalization of the Swings fluorescence mechanism. I built a highresolution image-tube camera (0.2 Å) for Comet Kohoutek, but the latter's brightness failed to justify early expectations and I also encountered foul weather. I never returned to comets. Such incursions and retreats from fields in which I was an early pioneer are not whimsical. They seem proper if one has one-of-a-kind resources, and they have salutary effects on selfeducation. This pattern is made possible by provision of ample observing time and an environment that encourages long-term programs and individuality. Certainly, I do not imply that my colleagues were noncompetitive; the system was, however, tolerant.

I was among the pioneers, briefly and uncomfortably, in studying quasars. Their deep significance for relativistic astrophysics, cosmology, and the activity in galactic nuclei has now been well explored by others. For me, the events took added significance in that they illuminated the psychology of a scientist and the resistance to forward leaps that may be ingrained in the scientific process. A new start makes genuine novelty appear too easy and permits us to forget the obstacles our predecessors faced. The story has already been well told by M. Schmidt. In 1961, I had studied the redshift of a few radio galaxies, ellipticals with weak emission lines. The radio sources, which appeared stellar to Matthews & Sandage (54), showed unusual photoelectric colors. These were called quasi-stellar radio sources (QSRS), or blue stellar objects (BSO) if radio-quiet. The unidentifiable, broad, weak emission lines were a subject of lunch conversations at the Athenaeum. I inherited the astrophysical study of 3C48 from Sandage, who first observed its spectrum in 1960. For this spectroscopist's jigsaw-puzzle, I obtained many excellent prime-focus spectra. An account of early results from the MWP observers was given at an 1960 AAS meeting. The trap for me was that two broad emission lines in 3C48 were at the rest wavelengths of O VI and He II. These lines were not seen in the few other known QSRS observed by Schmidt. By 1962, it had become a joke: emission lines in different QSRS had always to be at different wavelengths. Immersed in nucleosynthesis, my obvious solution was that QSRS were collapsed supernova remnants, each with a different surface composition, and that their luminosity derived from different radioactive decays. The differences in spectra depended on their age. I gave a "brilliant" paper to that effect at the NASA Goddard Institute in New York. I computed frequencies of dead supernovae of different ages and luminosities, and predicted counts as a function of apparent magnitude. It was all correct and might be useful if pulsars shone by radioactive decay. But optical pulsars are rare and faint compared with quasars, in spite of their added energy source (rotation). The paper was submitted to the Astrophysical Journal (and fortunately could still be withdrawn when Schmidt showed me his 3C273 spectra). To reread it is a salutary lesson in breast-beating. I spend five pages on "unidentified spectra in other stars" (magnetic white dwarfs, supernovae) and consider carefully the possibility that 3C48 is a radio galaxy with an enormous redshift. Twenty ultraviolet forbidden lines from [NI] to [Ti VIII] are compared with the emission lines in 3C48. I find four acceptable redshifts z, each accounting for three or more observed lines. They included z = 0.368 (from [O I], [Ne III], and [NeV]) and z = 0.702 (from [Ne IV], [O II], [Mg V], and [Ar III]). The finding list omitted the resonance doublet of Mg II at 2800 Å, because it was permitted; but Mg II is, in fact, the strongest line in 3C48. It also omitted [O II] at 3727 Å (for no good reason except that it was not in the far UV), but included [OII] at 2470 Å. In fact, the 3727 Å line accounts for yet another strong 3C48 emission. When Schmidt showed me the spectrum of 3C273 and said its redshift was z = 0.16, after a few moment's struggle I dredged z = 0.37 from my subconscious, based on the computation for 3C48. The 2800 Å line was visible at the ultraviolet edge of Schmidt's 3C273 spectrum (64). With acceptance of the idea of a redshift, the psychological logjam was broken. Publications reflected the speed of subsequent developments after the lengthy period when QSRS spectra were barren (28, 39, 58, 64). Matthews & Sandage (54) had broadband photometry; J. B. Oke (58) found H α with this scanner. Exploration of a variety of physical effects in the emission-line regions [Greenstein & Schmidt (42)] was rapid because of the long incubation period waiting for a physical model. We convinced ourselves that a gravitational redshift model was implausible, faced difficulties in the source of energy, and discussed collisional versus photon heating, density fluctuations, self-absorption in radio and optical frequencies, and electron scattering. The Report of the First Texas Symposium (63) reprints these and other pioneering papers and illustrates how rapidly theoretical possibilities can be exploited, once a basic new set of facts is recognized. Total exploration of a new subject requires an incredibly open mind and abundant good data. None of the modern electro-optical technologies (except the 1P 21 photomultiplier) was in use in 1960-63. It was the 200-inch telescope and the amount of observing time available to the staff that made success possible. As for me, after a few parting shots on multiple-absorption-line systems in quasars, I abandoned the field in 1967 without regret. A further change of subject matter and style was required by my deepened involvement in the outside world.

A PUBLIC LIFE

Much have I seen and known; cities of men And manners, climates, councils, governments, Myself not least, but honour'd of them all And drank delight of battle with my peers. Alfred Lord Tennyson, Ulysses

Scientists budget outside activities in various ways: textbooks, society presidencies, international organizations, directorships, new instruments,

future projects from which they will never benefit. Others choose wholehearted devotion to their own scientific work, knowing that they will face frustrations with time's passage. I enjoyed running my department and doing battle in committee rooms, while still working with the telescope. I tried to stay in competition for scientific novelty and insight. Several occurrences drove me further into the nonresearch world, even as available facilities were made more tempting by technological innovation. A personal bias preexisted: Hitler, WWII, and Korea had frightened me. From 1950 onward the international scene appeared ominous, and it has not changed. The reputation of Caltech and the Observatory made me an easily targeted and persuadable spokesman. I found that some national advisory activities required technical knowledge, while others needed mainly general "wisdom." I learned some of the limitations of a purely rational scientific approach. I am grateful that some officials and industrialists accepted my limited horizons of advice when for ten years during and after the Korean War I worked in major, nonscience areas.

It was a very hectic life. Some committees of which I was a member carry only initials that I cannot recognize. In one year I gave three dedicatory addresses for new buildings. I was on the National Academy of Sciences' Committee on Science and Public Policy (COSPUP) and then the NAS Council, which led to my chairing the study Astronomy and Astrophysics for the 1970's. I served as a special advisor to NASA under N. Ramsey, and in a smaller group directly advisory to the Administrator, J. Webb. The Ramsey committee was an early link in the planning of the Large Space Telescope. Several ad hoc NASA committees helped bridge gaps between scientists and NASA management. One led to the decision to locate the NASA Infrared Telescope Facility on Mauna Kea; my last service for NASA was on the Source Evaluation Board for the Science Institute for the Space Telescope. I received the title of Lee A. DuBridge Professor of Astrophysics (1970 till retirement), which honored me and a man I both liked and admired. I was particularly honored to serve as Chairman of Caltech's Faculty Board at a time when important changes (1965-67) were occurring in university life. In spite of committee and administrative distractions lasting many years, I was honored for scientific work: the California Scientist of the Year Award for 1964, the Russell Lectureship of the American Astronomical Society (1970), the Bruce Medal of the Astronomical Society of the Pacific (1971), the NASA Distinguished Public Service Medal (1974), and the Gold Medal of the Royal Astronomical Society (1975).

I returned to Harvard as a member of its Board of Overseers (1965–71; successive chairmen, D. Dillon and D. Rockefeller). After not having visited Harvard since 1939, I attended 40 meetings of the Overseers and related committees. It was a time of student unrest and violence. The contrast was

dramatic between young near-revolutionaries and the devoted judges, publishers, lawyers, industrialists, and academics. Harvard's long survival as a national resource required vigorous money raising, as it does at all universities. Financially generous Overseers and their friends really enjoyed the opportunity of participating in novel academic adventures. The challenge for me was to explain what academic life was for; it was not easy with such intelligent people, since academic goals were abstruse and diverse. The renewed contacts with old friends still at Harvard were stimulating. At the same time, I remained active in fund raising and planning for Caltech.

My central activity from 1947 to 1981 was in work with Federal agencies involved in funding science, especially astronomy. I came slightly late; many leaders of WWII science had tried to slow the dismantling of some of the applied-science organizations created for WWII, or to transfer to universities some support for basic research. Active on the larger scene were W. Baker, D. Bronk, V. Bush, J. Conant, L. DuBridge, J. Killian, etc. Others had special enthusiasms, like L. Berkner (of Associated Universities, Inc.) for radio astronomy, R. McMath (of the University of Michigan) for astronomy in the about-to-be-born National Science Foundation (NSF), and C. C. Lauritsen (Caltech), who was instrumental in establishing the Office of Naval Research (ONR). I served (1947) on the first ONR grants committee for astronomy. It was ONR that funded Caltech's radio observatory beginning in 1954. My involvement with the NSF was long; I was member and chairman of its first astronomy advisory committee (1952-55) when it considered its first grants, including the early approaches for the national optical observatory (2). In 1954, I was secretary of the group organizing a conference on radio astronomy (sponsored by CIT, CIW, and NSF). The conference led to planning for the National Radio Astronomy Observatory. It seems that committee memberships or leadership positions are addictive. This brings to mind a New Yorker cartoon that shows a father and child viewing a statue of a group of business-suited figures, with the caption "There are no great men, only great committees." Oral history programs at the American Institute of Physics attempt to reconstruct these hectic years when astronomy made its quantum jump in size. Our first, privately supported, big-science became one of the most successful in (per capita) Federal funding and, we believe, in achievement.

Most circumstances are irresistible, and no individual plays a decisive role. The cost effectiveness of being an advisor cannot be objectively measured. The tasks that had to be done were well done by US astronomers, effectively and honestly. A worthwhile historical background is a study of the President's Science Advisory Committee, edited by my good friend W. T. Golden (14), who helped found this committee. The study illuminates developments at the highest Federal level. The seamy side may be found in S. Hersh or H. Kissinger. I emphasized a technique that suited my times and personality, namely, to avoid confrontation and to stress rational compromise. Initiating a new program is a long task, and many people of diverse talents and styles are involved. After the scientific need and a consensus are established, scientific leaders must be matched with responsive individuals in Federal agencies, in a creative, symbiotic relation. Lobbying is not enough, and I am unsure of the benefits of letter-writing pressure. Scientists tend to get bored too soon. Someone within the bureaucracy must carry the burden of internal persuasion, of interminable briefings and hearings, with little personal reward.

My personal copy of the Greenstein Report (56) is inscribed by G. Kistiakowsky (science advisor to the President for the years 1959-61), on whom I relied heavily. It reads, "Sorry that in my ignorance I started all of this." But I am not sorry. In 1964, the NAS had published the Whitford report on ground-based astronomy; there were two studies of radioastronomy facilities by panels headed by R. Dicke; there were also ongoing studies by the Space Science Board for NASA. Attitudes within the government had changed by 1965. The leveling off (and therefore real decline) in funding went with an impatience with "shopping lists." Thus, H. Brooks (in COSPUP) emphasized that the scientists must set the priorities, recommend that the obsolete facilities be closed, and even reject some new proposals. The Greenstein Report (56) had to include all astronomy (except planetary missions), to establish priorities, and to give reasonably accurate prices. Ten years later, the Field Report (57) was to function in an even more difficult environment; the astronomical community had become less cohesive, and major space projects more costly. A large portion of the Federal funds available were required to operate the national observatories. Costs in astronomy rival, per capita, those in high-energy physics. The Field Report recommends programs costing \$1.9 billion (1980 dollars); it states that the Greenstein Report recommended programs costing \$844 million (in 1970 dollars, equivalent to \$1.7 billion in 1980), of which most have been implemented.

The growth of knowledge and understanding in the last few decades are, for me, sufficient moral, aesthetic, and scientific justifications for such levels of expenditure. Human illness and poverty cannot be cured merely by spreading money, but the human condition can be ennobled by spending money wisely. Rising levels of health, education, and prosperity within the US are based on our technological revolution, which affords us the luxury of basic research from which technology springs. The growth of particle physics, with its ever more expensive, rapidly obsolescent facilities, parallels ours. After decades of leadership in that field, the US has fallen behind the

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orderly developments in Europe, at CERN and in Germany. I hope I am not being merely nationalistic. The competitive spirit in science seems to me to be necessary. Astronomy suited the American genius in its mixture of romantic subject matter and sophisticated tinkering. Observations, data, suit American empiricism. In Pasadena, a concern had existed in 1948 that I would bring in too much "theory" with the new staff and disregard the traditions of obtaining good data. But I feel that there is never enough good data. What if Grand Unified Theory does link cosmology and particle physics; what if the Universe is closed by invisible matter? Is astronomy to reach a dead end? Or will its practitioners readapt once more to produce ideas and new instruments to make visible the invisible? I hope so, and there are many past examples. The magnitude limit for photography was near 22 in 1952; it is now near 26 with the CCD at the 200-inch reflector. An excellent improvement, for half a percent the cost of a space experiment. Prolonged involvement in plans gave me opinions (but not conclusions) about the best strategy in the balance between the private and public sectors, on the role of the national observatories, and on the importance of the original, talented individual. The unique instrument may grow from the ideas of an individual, in response to either a scientific goal or to an irresistible urge for the ultimate technology. Groups or individuals plan and use experiments on the Space Telescope, the Advanced X-Ray Astronomy Facility, or the Space Shuttle; no individual can take full credit. But a ten-year delay makes success feel humanly remote. An excellent history of government and of external advisory committees preceding the 1972 funding of the Very Large Array was prepared by G. Lubkin (51). A Nieman Fellow (journalism) at Harvard, Lubkin bases her report on correspondence in the NAS (Brooks, COSPUP, my NASSurvey) and in the National Science Foundation, and supplements it by interviews with the protagonists. The National Radio Astronomy Observatory planning for a high-resolution system started in 1962, in many studies. External groups, the Whitford Committee, two Dicke Panels, and my NAS Survey gave it highest priority. Over 60 individuals are mentioned (51). The Bureau of the Budget and the NSF were convinced by 1969; so were the Office of Science and Technology (OST), the PSAC, and the congressional committee staffs involved. The procedure was slow and incredibly complex. It must be studied to be understood. It succeeded, and the VLA is successful; its story is a useful one for aspiring promoters of further large projects.

The issue of balance between individual and national goals is indirectly addressed in the Academy study (56); during our final discussions, it caused me intense discomfort. I resigned (51) for a brief time as chairman, since I was uncertain that I could fully support all the recommendations. That survey report is schizoid as published—it says build large, new national instruments (but please do not neglect to support university scientists and their new instruments). The parenthetical phrase may be intellectually correct but it is impotent, with no political or budgetary clout. The individualistic style of my own research was possible at institutions founded to pursue new, unplanned, and often changing goals. That system was good; I remain skeptical that it is completely outmoded.

LATER RESEARCH ON FAINT OBJECTS

Life has a limit, knowledge none. Chung-Tzu

The decisions on such arguments lie in the future; I wish good fortune to those who must answer such questions. I did not disappear from the national scene in 1972, but I had lost my optimism about large further contributions and resigned from many outside activities. Yet the busy, double life continued; I was on the Associated Universities for Research in Astronomy (AURA) Board (serving as its chairman from 1974 to 1977); I was a visiting professor at the Princeton Institute for Advanced Study, at NORDITA, at the Bohr Institute in Copenhagen, and at the Institute for Astronomy in Hawaii. I resigned from heading astronomy at Caltech in 1972 after 24 years. I had been assigned 20 to 30 nights a year (1952-79) at the 200-inch, and I could profitably use new instrumentation as it was developed. The prime-focus nebular spectrograph, designed for galaxy redshifts, was an exciting instrument and permitted work on faint objects, quasars, and white dwarfs. The first papers by Eggen & Greenstein (11-13) contain photographic spectral classifications, photoelectric colors, luminosities, and space motions of over 200 white dwarfs. One of these, Tonantzintla 202, has an interesting story. Called a white dwarf (40 pc distant), its prime-focus spectrum is probably the first taken (1960) of a quasar. Ton 202 appeared to be a nearly featureless, DC star, with flat energy distribution and possible emission lines; with electronic detectors, these would have been obvious. In a paper by Greenstein & Oke (40) it is shown to have a redshift of 37%, i.e. 2×10^9 pc distant. If one must be wrong, be so on a grand scale!

My general plan was to explore the lower left-hand quadrant of the HR diagram containing the hot subluminous stars, a complex and then nearly unknown group. Only a few, brighter members were accessible to detailed analysis at the coudé. The nebular spectrograph permitted classification of horizontal-branch and subdwarf stars (sdB, sdO) and some quantitative work. I first categorized the subluminous stars in two earlier papers (27, 30); the end results are in Greenstein & Sargent (41). Only recently have sdO's

received the full attention they deserve, by D. Schoenberner, D. Koester, and R. P. Kudritzki, in work at ESO, and with Kiel models. They range from nuclei of planetary nebulae, above 150,000 K, down to the predecessors of hot white dwarfs. Humason & Zwicky (48) had found faint blue stars at the galactic pole, and Zwicky believed them to be intergalactic wanderers. While some apparently normal B stars do exist at surprisingly large distances from the galactic plane, most faint blue stars prove to be highly evolved, low-mass remnants. They outline a horizontal branch extending to higher temperatures than found in globular clusters. Their low luminosity was established from line profiles and models, as well as from proper motions. They provided a new, broad insight into a wide variety of the nearly terminal stages of evolution. The *Strasbourg Conference Proceedings* (52) reviews the early stages of our recognition of questions raised by these stars.

The luminosity of a normal B star is 100,000 times that of a hot white dwarf; and it is another step of nearly 100,000 from the hottest to the coolest known degenerate. A broad range of phenomena awaited exploration. At first, white dwarfs seemed attractive since they were supposed to form a simple group; this proved far from the truth. Lacking an energy source other than cooling of the nucleons in their cores, they travel down a straight line in the log L, log T plane at a radius fixed by their mass. Chandrasekhar (5) and Hamada & Salpeter (43) had given the zero-temperature massradius diagram, so that, unique in astrophysics, a structure existed, depending upon a single variable. An IAU Symposium (53) documents the appearance of a variety of new problems. The observational data available in 1965 was severely limited; eventually I gave 12 lists of white dwarfs observed spectroscopically or spectrophotometrically (over 550 stars). The last list (34) was published in 1980. The number of presently well-observed degenerates exceeds 2000, many from the work of R. F. Green, Schmidt and Liebert (in preparation), G. Wegner, and others. A computer-stored list, with tables of most data, is being prepared by E. M. Sion and collaborators at Villanova. In 1965, only 17 white dwarfs had parallaxes; now over 100 precise values exist, coming largely from the US Naval Observatory (USNO) program initiated by K. Aa. Strand. The subject now seems inexhaustible. The IUE added ultraviolet fluxes for hot stars; HZ 43 is one of the hottest, and also is detected in the EUV. The IUE data checked the effective-temperature scale for hot degenerates. It provided the exciting detection of C I and permitted composition determinations of trace elements, like metals, in a few yellow degenerates (7, 68).

My collaboration with Eggen (a mine of knowledge on binaries, clusters, motions, and photoelectric photometry) was exciting. On the 200-inch, I graduated from photography to the image-tube spectrograph, and then to

Oke's remarkable multichannel spectrophotometer (59). This instrument provides absolute fluxes for comparison with computed models. Shipman (65-67) and Greenstein (32, 33, 35) established the relation between theoretical fluxes and temperature using Oke's (59) and my own multichannel data (32, 37). The Oke-Gunn double CCD spectrograph has now become the ultimate instrument. In 1982, long after retirement, I observed 200 spectra in 8 nights at 5 Å resolution, which approaches that of primefocus spectra. Spectra with signal/noise over 100 were obtainable in 10 minutes at 16th magnitude. I am grateful to Oke for his unselfish development of this advanced instrumentation for the 200-inch. My results spread over 60 papers, exploring such topics as mass, luminosity, temperature, surface composition, energy distribution, line profiles, magnetic field, the lack of rotation, gravitational redshift, motions, gravitational diffusion, and cooling theory. Some of the papers give general treatments of properties of white dwarfs with hydrogen-dominated atmospheres (33, 37) and with helium atmospheres (31, 37).

It was a pleasure to have to learn a new part of physics (solid state) for application within the theory of cooling and internal structure. The termination of nuclear burning in the parent red giant leaves a core of carbon (plus helium), and possibly a skin of an unknown, but small, mass of hydrogen. Interstellar matter may add more hydrogen by accretion. Thus, the internal composition of a low-mass white dwarf is almost certainly carbon, the envelope helium, and a skin of hydrogen may or may not exist. Observations probe only the thin atmosphere, which for 70% of the degenerates is hydrogen (H/He > 100), and for the balance, helium plus trace elements (He/H > 10,000). The white dwarf spectra divide observationally into the DA's and non-DA's, with the latter commonly having carbon and metals in concentrations far below their solar values. Only about 20 stars have had atmospheric composition analyses performed for carbon/helium or metal/helium ratios. The spectral class of a white dwarf should now include both a composition parameter and a temperature estimate. The latter is easy, since modern detectors give colors quantitatively. All white dwarfs have nearly the same radius; a new spectral classification system has been proposed by a group of workers active in the field, in which the type symbol indicates the dominant composition, the temperature, and thus the luminosity (69). This replaces my classification system (27) of 1960.

In their enormous gravitational fields, the composition of white dwarf atmospheres will be altered by differential diffusion of the heavy elements. They are also subject to accretion from interstellar space (1, 35), a process providing hydrogen, helium, and heavy elements. Accretion would contaminate the non-DA's with hydrogen; only rarely would He and metals

complicate the DA's, in which rapid diffusion of heavy elements purifies very thin atmospheres. Composition-stratification clearly exists (from the theory of nonradial vibrations of ZZ Ceti stars). A competition exists between convective instability and radiation pressure on trace elements (75, 76). The known complexity in composition of the thin surface layers of white dwarfs grows with each new quantitative analysis. Accretion theory is not certain; the observations currently suggest that some new mechanism is needed to inhibit hydrogen accretion. An interesting fact is that no white dwarf is known with high O/He (in fact O has never been detected), while relatively high C/He is common. If dredge-up mechanisms are involved in the appearance of C and metals in some He-atmosphere white dwarfs, it seems likely that nuclear burning ends at C, rather than at O in red giants that survive to become white dwarfs. Alternatively, gravitational diffusion may have buried the O so that the cores are also compositionally stratified. Burnt-out cores of red giants of initial masses up to $5M_{\odot}$ or even higher become white dwarfs without explosion, indicating how high is the fraction of mass lost.

If we know the core composition, cooling theory predicts the luminosity function. An excellent review is given by Liebert (50), who is now pressing forward the search for cool, red degenerates of very low luminosity. The first cooling stages, to luminosity L, predict a lifetime of $t_0 L^{-5/7}$; the constant t_0 depends on the mean atomic weight $\langle A \rangle$ in the core. Then, as new degenerates appear, in a steady-streaming model, the number of degenerates per unit volume should vary as $n_0 L^{-5/7}$, i.e. as $n_0 T^{-20/7}$. Below a certain core temperature $T_{c}(Z, A)$, the core solidifies; below the Debye temperature $T_{\rm D}(Z, A)$, the available heat content is low from quantum effects. If there is a simple dependence of effective temperature on core temperature, the number of white dwarfs above $T_{\rm D}$ should and does increase steeply with increasing bolometric magnitude. Below $T_{\rm D}$, with plausible simplifications, the number per bolometric magnitude interval is constant [see Greenstein (29)]. The increasing frequency of cool degenerates is established down to $M_v = +15$, but no very low-luminosity degenerates have yet been found. I was convinced in 1969 that cool degenerates were rare; the maximum cooling age found was near 7×10^9 yr. By 1979 I had spectrophotometry of 25 red degenerates with $\langle M_{\rm V} \rangle =$ +15.3. There are no known red degenerates fainter than +16.1. Liebert, and workers at USNO (notably C. Dahn), are pressing this search. In the oldest globular clusters, such red degenerates would be fainter than 29th magnitude. The search for new, low-luminosity red degenerates is a challenging technical problem; the total mass contributed by white dwarfs in our neighborhood depends critically on whether the observed luminosity function should be extrapolated as $L^{-5/7}$ or as L^{0} . The rapid increase of

bolometric correction below 4000 K suggests proper-motion surveys in the red for stars that are not M dwarfs. Binaries containing faint old dM stars have been searched for even fainter red degenerate companions, without success. A whole-sky search to 20th magnitude would probably reveal only 30 red degenerates of $M_V = +17.5$. None are now known. Liebert (private communication) has discussed the detailed prospects for such a search.

Other physical studies concern the properties of individual degenerates and the statistical value of the mean gravitational redshift. The latter is +50 km s⁻¹, from photographic low-resolution spectra, giving a reasonable value of $\langle M/R \rangle$ and a reasonable mean mass near 0.7 M_{\odot} . The detailed analysis by V. Weidemann and collaborators has shown a remarkably narrow spread of masses, as deduced from accurate surface gravities and temperatures, near $0.55 \pm 0.10 M_{\odot}$ for H- and He-atmosphere degenerates and ZZ Ceti variables. Another aspect of stellar evolution is the remarkably slow rotation of single white dwarfs, as illuminated by the discovery of sharp cores at the center of H α and H β in a few bright objects. This discovery was made possible by visits of A. Boksenberg's IPCS to Palomar. The resolution, at the coudé, was 25 km s^{-1} ; the deduced rotational velocities were from 35 to 100 km s^{-1} . This would be unexpectedly low for a star that has shrunk by a factor of 100 in radius, if angular momentum were conserved. Like the pulsars, single white dwarfs are, in fact, slow rotators. Both the specific angular momentum and magnetic field must be drastically reduced during their prior evolution, presumably by the stellar winds that are reducing their mass by about 80%.

For many, the white dwarfs in close binaries are fascinating, and their accretion disks are more easily studied than those near neutron stars. For me, there are still mysteries in the single white dwarfs; as a spectroscopist I find it exciting that we have not yet identified broad absorption features in some magnetic white dwarfs. White-dwarf fields lie halfway between normal and neutron stars, on a logarithmic scale. The cataclysmic variables have matter falling into relatively shallow potential wells, barely deep enough to produce X rays. In AM Her stars a single magnetic funnel guides matter from the main-sequence star to a surface where the white-dwarf magnetic field can sometimes be directly observed. The binaries containing white dwarfs are useful guides, on a smaller scale, to some of the phenomena in X-ray binaries and X-ray pulsars.

If one likes puzzles, take the example of an enigmatic white dwarf GD 356. This star had intrigued me for years because its continuum appeared rough with the low resolution of the multichannel. With the high signal/ noise of the CCD, I discovered triple, Zeeman-resolved emission lines of H α and H β split by ± 400 Å, at a contrast of only a few percent with the continuum (36). The splitting suggests a 10–20 MG field, presumably in a

thin chromosphere. More detailed study provides new problems; $H\beta$ is stronger than $H\alpha$, which is possible when Lyman lines and continuum are optically thick. In a dipole spread over an appreciable volume, the lines become too broad. The magnetic energy density controls the emitting region. No trace of a companion has been found, and GD 356 is not an X-ray source nor is it polarized or variable. It may be demonstrating hydromagnetic heating.

Since white dwarfs provide such a cornucopia of new and unexpected phenomena, I am delighted at the continued observations at Arizona, Kitt Peak, Texas, and Australia and at the theoretical studies at Delaware, Kiel, Montreal, Rochester, and elsewhere. The field is healthy and in good hands. I was indeed honored that colleagues working in the field dedicated to me the volume resulting from the IAU Colloquium No. 53 (74). The rapid intrinsic variability of the ZZ Ceti white dwarfs (not binaries) provides a seismic probe of the stratification of composition in their atmospheres and envelopes; the driving-pump mechanism is a hydrogen-ionization zone for the DA's. This was generalized, and rapid variability predicted (79, 80) and found, in a hot DB, GD 358, where it is driven by the helium-ionization zone. The computer is as necessary a tool for study of white dwarfs as a large telescope. Even I have a graphics terminal in my office.

SPECULATIONS ON THE FUTURE

It is not for you to finish the work, But neither may you exempt yourself from it. Ethics of the Fathers (1st c. B.C.)

This prefatory chapter has oscillated between my views on trends in the organization and funding of astronomy and a personal account of research interests. Astronomy is now too complex for even a well-informed generalist to make significant comments about its future. One obvious remark is that overspecialization is easy and common. In the 1950s an attempt was made to keep meetings of the American Astronomical Society to single sessions so as to induce all members to hear about all subdisciplines. While astronomy remains a romantic, all-embracing view of the Universe, it now employs too many types of eyes for easy communication. Struve's (70) rather pessimistic thoughts about the future of astronomy were written in the face of a war following a depression. His concern was whether his staff would remain interested in astronomy as it had been constituted. We now face financial crises, for example, in the undersupport of planetary missions and X-ray astronomy, but a new concern is that the nation's astronomical staff comprises such an enormous

diversity of scientists, working with many different tools. The adaptability of our institutions and publications has made the transition apparently painless, but there are still problems. I have difficulties in reading, let alone understanding, five pounds of *Astrophysical Journal* each month. Scientists with training as chemists, computer engineers, electronics engineers, geochemists, and physicists are now found under our broadened umbrella. While they often rediscover what was obvious to earlier generations, they also find new things that an older generation could only have imagined. Can the new, broadened family learn to speak a mutually intelligible language? Can we retain a sense of daily excitement and of pride and interest in each other's achievements?

Historically, optical astronomers have needed more detected photons. We have done well along one road, where electro-optical sensors approach limiting quantum efficiency. Availability of large collecting areas is more general, with the provision of telescopes of 4-m aperture for the astronomical community at the national observatories in both hemispheres. Much larger telescopes are planned with radically new designs. While radio astronomy long suffered an enormous deficit of capital expenditures, in the VLA it now has an extraordinarily successful instrument with millisecond resolution on galaxies and stars, better than any optical instrument. The very-long-baseline array should follow (57) the very-long-baseline-interferometry, working to microsecond resolution. The Space Telescope will provide many more photons for the ultraviolet (since the IUE had only an 18-inch mirror) and much higher resolution against a darker sky in the optical wavelengths. Infrared progress, including work done using the IRTF on Mauna Kea and the IRAS in space, has been remarkable. There seem to be no more wavelength regions to be opened. Easily crossed frontiers have vanished.

Gravitational-wave detection remains open ended in that it now gives information only on upper limits. Particle detection has remained peripheral to astronomy, even though some of the data obtained by cosmic-ray physicists are published in the *Astrophysical Journal*. But consider what happens if the "missing-mass" (which I hope does not exist) consists of magnetic monopoles, gravitinos, or neutrinos. (For a brief period, neutrinos were supposed to oscillate and have mass, but they now don't.) In such problems, how will astronomers (broadly defined) be involved, except in a speculative way? In particle physics our education and skills seem irrelevant. But there is a lesson to be learned from our past—that of resourcefulness. Many discoveries are part of what now seems obvious, but they cost a great deal to obtain. Some new results could have been guessed at or predicted. Astronomers have always had to be resourceful in deductions concerning unobservable wavelength regions. The Zanstra

mechanism (81), where far-ultraviolet photons are counted from the visible line emission of gaseous nebulae, is an example of such a resourceful technique. The X-ray emission from solar flares was deduced from ionospheric phenomena. In the 1960s, the high luminosity of quasars called for an unknown energy source, magnetic fields, and relativistic electrons. That energy source, now known to be gravitational collapse, is merely an extension of the idea of disks in deep potential wells. The nearly universal existence of magnetic fields is plausible from interstellar polarization and the isotropy of cosmic rays. It was noted that relativistic electrons would suffer inverse Compton collisions, turning radio-frequency photons into hard photons. A first response to that prediction might be "implausible." But the X-ray sky is now full of quasars. Had we been sufficiently brave, we would have predicted the X-ray sky with its binaries and quasars. Is it possible that we were forced to be more clever when severely limited in what we could hope to observe? I doubt that, and believe that this cleverness will reappear as scientists interpret the dramatic, unexpected revelations of the future, using typical astronomical ingenuity. I applaud their success in advance.

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