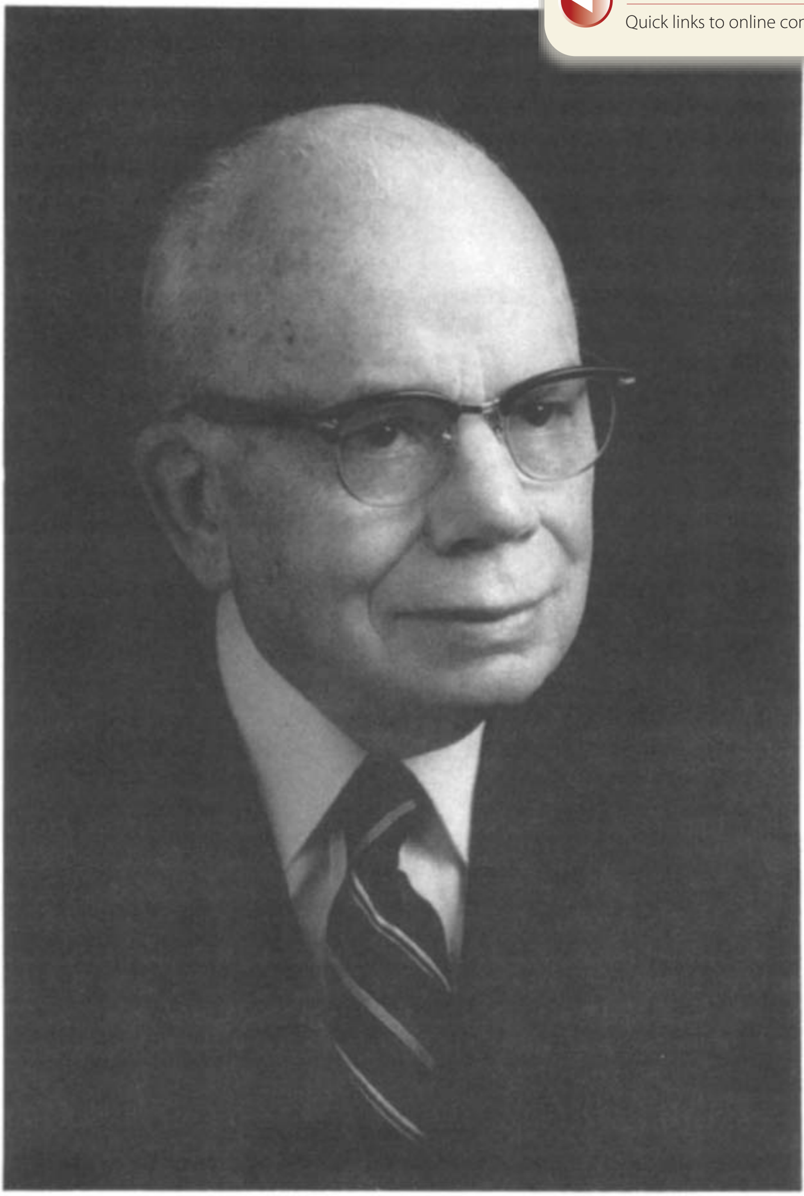

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A HALF-CENTURY OF ASTRONOMY

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BEGINNINGS

An adult lifetime devoted to astronomical pursuits did not in my case evolve naturally out of a youthful fascination with the stars. Any inclinations that stemmed from boyhood hobbies or early educational influences pointed in other directions. The circumstance that led to the path finally taken came rather late, and it was almost an accident. In the spring of 1931, I learned that a research assistantship at the Washburn Observatory at the University of Wisconsin in Madison was open to applications. Joel Stebbins, the Director, was attempting to develop a more sensitive method of measuring the extremely small electrical currents delivered by the photoelectric cells he was using to measure the light of the stars. Then a graduate student in physics at the University, I sought an interview with Stebbins. The job climate in the depression years following 1929 was such that I did not hesitate long after his offer of an appointment.

The route by which I became a graduate student in physics at Wisconsin was not unusual. In the period between the wars, a good proportion of the admissions to doctoral programs at major universities came from private liberal arts colleges. The dedicated teachers at many of these institutions succeeded in inspiring the intellectual interest that led their better students to undertake additional years of study leading to the PhD degree. The stipends and student aids then available were so limited that a rather frugal existence during the graduate years was the normal expectation.

My own undergraduate preparation was acquired at Milton College in southern Wisconsin. Ever since my grandfather's generation, members of

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the Whitford family had been deeply involved in the affairs of that institution. The family had descended from Yankee stock going back to the colonial era. The cultural heritage of the college had deep roots in the New England tradition, and the curriculum emphasized studies in the liberal arts.

The physics to which I was exposed in the late 1920s was in transition between the old quantum mechanics of Planck and Bohr and the just-emerging new quantum mechanics of Heisenberg, Schrödinger, and Pauli. Wisconsin students were introduced to the latter by John H. Van Vleck, a young member of the physics faculty. Visiting lecturers and colloquium visitors included H. A. Lorentz, Arnold Sommerfeld, Werner Heisenberg, and Gregor Wentzel.

Students who undertook thesis projects in experimental physics, then in the legendary era of sealing wax and string, came under the influence of Charles E. Mendenhall, the chairman of the department. He believed that students should as far as possible construct their apparatus with their own hands. He saw to it that they had ample opportunity to acquire the necessary laboratory skills through practical experience. Fundamentals included metal working, glass blowing, quartz-fiber drawing, and vacuum technology. Since many projects involved measuring very small electrical currents, the problem that Stebbins encountered in his photoelectric measurement of starlight was a familiar one.

Edwin P. Hubble's announcement in 1929 that the observed radial velocities of galaxies implied an expanding universe excited some interest in the Wisconsin physics group. A few years later, one of my fellow graduate students, Karl Jansky, made the important discovery of "cosmic static." Though he was able to show that the radio-frequency radiation came from an extraterrestrial source in the Milky Way, he did not live to see the emergence of radio astronomy.

The job interview with Stebbins gave me my first glimpse of the surroundings amid which astronomy was carried on in that period. The Observatory took its name from Cadwallader C. Washburn, a former governor of Wisconsin. He had donated the building and a 15.6-inch Clark refractor to the University in 1878. In the years up to 1931, the decor inside the observatory had not changed appreciably. Indeed, my first impression suggested a 50-year backward leap into the nineteenth century. The high-ceilinged rooms, the marble fireplaces, the Victorian walnut furniture, and the ticking pendulum clocks portrayed an atmosphere quite unlike the hurly-burly of the basement corridors of the physics building to which I was accustomed.

Amidst these reminders of the past, however, there was a research program in progress that was quite unique. Stebbins was using a photoelectric cell at the focus of the Observatory's modest telescope to measure the light and color of stars with unequaled precision. These observations had shown critical details of the light curves of eclipsing binaries missed by visual and photographic observers; the same was true of color differences arising from reddening by interstellar dust. Only relatively bright stars were within reach, however. The observing list was restricted to objects brighter than magnitude 7.5 owing to the limitations imposed by the quartz-fiber electrometer used to measure the current from the photoelectric cell.

My task as research assistant was to carry forward Stebbins' long campaign to find something better than the electrometer. Vacuum-tube amplification appeared to be a promising approach. Amplifier tubes designed to minimize unwanted internal sources of current to the control-grid lead had been developed in industrial laboratories. Yet for reasons then not understood, when the tube was connected to a photoelectric cell there were fluctuations in the output many times larger than the theoretical expectations. I found the source to be cosmic-ray-generated ions in the ambient air; some of these were being collected on the lead from the photoelectric cell to the amplifier tube. Mounting the cell and tube in a vacuum tank took away the air, and there was a marked reduction in the fluctuations. A test on the Washburn telescope in the spring of 1932 showed that stars of magnitude 9.0 were now measurable. The system soon went into regular use on the observing programs then in progress.

MOUNT WILSON OBSERVATORY

By June 1933 a version of the photometer and amplifier suitable for attachment to the 100-inch and 60-inch reflectors of the Mount Wilson Observatory was ready to be taken to California. Stebbins had already begun using the old photometer on them for objects too faint for the Washburn refractor. Now the magnitude limit could be extended still further.

There were no unforeseen complications. At the beginning of the first scheduled night on the 100-inch telescope, Stebbins opened the shutter that admitted light to the photoelectric cell at the Newtonian focus near the top of the dome. As he called down "Open," I saw the luminous spot on the galvanometer scale at the base of the telescope move upscale by the expected amount. This simple arrangement, primitive by modern standards, had been common in physics laboratories. It served with little change for many years of our observing programs at Mount Wilson.

The first observing session in 1933 came at the start of a two-year National Research Council fellowship in physics. I had completed my PhD thesis on a problem in atomic spectra under Mendenhall and Julian Mack, and undertook further work in this field at Caltech under Ira Bowen. I began the fellowship as a physicist spending part of his time as an instrumental specialist at Mount Wilson. By the end of it, the transition was complete; I had become a committed astronomer. The work with Stebbins had a strong influence. I could hardly avoid a deepening interest in the astronomical objectives of the observations we were making. Matters of technique, while still important, took second place.

The Mount Wilson Observatory of the early 1930s could not fail to excite and stimulate any young scientist working there. It was unquestionably the premier center of observational astronomy. All around me I could see and hear about a variety of research programs that covered most of contemporary astronomy. Enough rubbed off on me to provide a liberal education in a field to which I had not been exposed in my previous schooling.

The best opportunities for learning about the astronomy in progress came during the observing runs on Mount Wilson itself. Conversation flowed freely at the midnight lunch in the Galley, a low wooden structure between the domes. The same was true at meals in the dining hall at the Monastery, the dormitory complex at cliff edge on one of the spurs of the relatively level mountaintop. These were served with considerable style: a white tablecloth, a hierarchical seating plan, roasts carved at table by the observer assigned to the 100-inch, and a mandatory jacket-and-tie rule for all. These hallowed traditions had been established by George Ellery Hale.

Walter Adams, Director of the Observatory since Hale's poor health had forced him to give up that responsibility in 1922, never let his administrative duties interfere with his full quota of nights at the telescope. He carried on both activities with an economy of words that reflected his New England heritage. Just before the First World War, in the early years of stellar spectroscopy on the 60-inch telescope, he and Arnold Kohlschütter had noted differences in certain spectral lines that indicate whether a star is a giant or dwarf. This was at a time when the new Hertzsprung-Russell diagram was making a clear distinction between these classes of stars, long before there was an explanation of the line strengths in terms of ionization theory. Together with Alfred Joy, Adams developed the Mount Wilson system of spectroscopic parallaxes; this was a forerunner of the Morgan-Keenan classification system now in general use.

When Stebbins and I were studying absorption and reddening of stars at the north galactic pole, Joy gave us lessons on how to obtain spectra for classification purposes. Our manipulations with photoelectric cells held some mystique for "regular" astronomers; conversely, we as non-photographers needed some coaching in traditional techniques.

Hubble was an imposing figure, already assured of a distinguished place in astronomical history, first by his demonstration that the extragalactic nebulae (a term he preferred to "galaxies") are indeed stellar systems external to the Milky Way, and second by his discovery of the expansion of the Universe. In 1933 he was in the midst of his surveys of the number of countable galaxies per square degree as a function of limiting magnitude. Cosmological questions that had been in the realm of armchair speculations now seemed amenable to observational attack. He believed that his sampling of space to increasing depths could bring an answer. Although he underestimated the complications, the boldness and vigor with which he undertook the observations were characteristic of his approach to astronomy.

Walter Baade was perhaps the most stimulating person among those who influenced me in those initial years at Mount Wilson. He enjoyed explaining his ideas, full of insight gleaned from his encyclopedic knowledge and his current observations. His conversations sparkled, enlivened by fascinating anecdotes recalled from association with observatories and astronomers on both sides of the Atlantic. He could be irreverent and iconoclastic. He felt that the calibration of the distance indicators such as Cepheids and RR Lyrae variables had to be put on a firmer foundation before attempting cosmological investigations. He urged astronomers to "get out of the local swimming hole," i.e. to look at stars in our own Galaxy and other galaxies in environments unlike that of the immediate neighborhood of the Sun. A decade later this approach led to Baade's memorable contribution, the concept of the two stellar populations.

Baade had the foresight to recognize the considerable advance in astronomical optics that could follow from the application of the principle of the Schmidt camera. The inventor, Bernhard Schmidt, had been an optician friend during Baade's days at the Hamburg-Bergedorf Observatory. Well before the construction of the 48-inch "Big Schmidt" at Palomar, Sinclair Smith and Theodore Dunham introduced Schmidt optics into Mount Wilson spectrographs. Smith, a physicist trained at Caltech, had tested laboratory-type electrometers in photoelectric measurement of very faint starlight.

Although Hale was not seen in person in the Pasadena offices or on the mountain, there were constant reminders of his pivotal role in founding the Observatory. His name was mentioned with admiration and respect by senior astronomers who had worked with him during the formative years. In the period since relinquishing the directorship, he had returned to his interest in phenomena on the surface of the Sun and carried on research from his private Solar Laboratory in an orange grove not far from the Caltech campus.

I had a chance to meet Hale during my first summer in Pasadena. The occasion was a test of our photoelectric equipment as a possibly more sensitive detector of the influence of the Zeeman effect in solar spectrum lines. Hale had resumed his search for evidence of a general magnetic field on the Sun. Variations in the transparency of the Pasadena sky overwhelmed any effect we might have hoped to observe. It was not until Horace Babcock overcame this limitation in the 1950s by stepping up the alternation rate between the two states of polarization to 120 cycles per second that a successful solar magnetograph became possible.

I recall conversations with Hale in his office at the Solar Laboratory in San Marino. Around him were fine old books and objects from Egyptian tombs that he had collected in the pursuit of his hobbies. He spoke animatedly of the past and buoyantly of the future. During his years of semi-retirement and intermittent health problems, this office had been the center from which he had pursued his campaign to see the great 200-inch telescope pass from the realm of a dream to a developing reality.

There was a mood of anticipation in Pasadena as the early phases of the project took shape. The ribbed pyrex blank for the 120-inch test flat arrived and was unloaded at the optical shop on the Caltech campus. No one could then foresee that it would one day become the primary mirror of the Shane telescope at Lick Observatory.

WASHBURN OBSERVATORY

The research in which Stebbins and I were engaged went forward with little change following my return to Washburn Observatory in 1935. Stebbins found support for new ventures and for improving the laboratory workshop in which we built new instruments. Visiting observer privileges at the Mount Wilson Observatory continued to give us access to large telescopes and a good sky.

I found the familiar surroundings of the Wisconsin campus a congenial place to work. In 1937 I married Eleanor Whitelaw. An appointment to the faculty of the University of Wisconsin in 1938 meant that I began teaching formal courses in astronomy. Since I had never had any classroom instruction in the subject, this had the salutary effect of forcing on me a thorough review of the fundamentals of the subject. My introduction to astrophysics had come through Volume 2 of Russell, Dugan, and Stewart's *Astronomy*. It was a most influential textbook; its virtues included not only lucid exposition, but also a certain grace and felicity that pervaded the writing style of Henry Norris Russell.

I recall a meeting of the American Astronomical Society in Bloomington in 1937 at which Russell, the retiring President, commented in a most illuminating fashion on papers in many fields, showing the significance of each in its larger context. Though extemporaneous, these spoken comments had the same characteristic elegance of style. At a meeting with an attendance of about 70, all members could hear this discussion; there were no divided simultaneous sessions.

The strong emphasis on theoretical astrophysics that was developing at Yerkes Observatory in the late 1930s was felt at Washburn Observatory, 60 miles away. Under the leadership of Otto Struve, both Bengt Strömgren and Subrahmanyan Chandrasekhar were attracted to Williams Bay; occasionally one of them came to Madison to give a lecture or colloquium talk. Jesse Greenstein encouraged our studies of interstellar absorption. At the dedication of the McDonald Observatory in western Texas in 1939 (a project in which Otto Struve and the University of Chicago had a prominent part), I saw E. A. Milne, Jan Oort, and Albrecht Unsöld for the first time.

My astronomical pursuits were interrupted at the beginning of 1941 when I joined a "defense" project known as the Radiation Laboratory at the Massachusetts Institute of Technology. The task, then secret, was the development of microwave radar and its military applications. By the end of the war in 1945 the Laboratory had become a very large enterprise, yet not so compartmentalized by internal secrecy walls as to prevent individual staff members from keeping abreast of most of the technical advances and practical results. I was introduced to the optics and electronics of an unfamiliar part of the electromagnetic spectrum; later, this technology was put to use in the rapid growth of radio astronomy in the postwar years. I also acquired experience in several branches of more conventional vacuum-tube electronics. The transistor and the solid-state revolution were still in the future.

During the war years, Stebbins continued the observing programs that he and I had been pursuing during our annual visits to Mount Wilson through 1941. I resumed participation upon my return to Madison in 1946. My many years of collaboration with Joel Stebbins ended with his retirement in 1948. He had introduced me to astronomy. Working with him was not only good training, it was thoroughly enjoyable. More than anyone else I was in a position to appreciate his urbanity, his dry wit, and his inexhaustible fund of droll stories.

I succeeded Stebbins as Director of Washburn Observatory. My research program continued to be organized around an annual visit to Pasadena to work with the telescopes of the Mount Wilson and Palomar Observatories. I soon became involved in the movement toward direct government support of pure scientific research. The Office of Naval Research (ONR) was the first federal agency to undertake the financing of astronomical projects. Major grants to several universities, beginning in 1947, made possible the construction of the first round of radio telescopes in the United States. In 1948 I served on an ONR panel headed by Otto Struve to consider proposals from individuals for projects in optical astronomy. An indication of the modest scale of these early supplements to local support of astronomical research is given by the recommendations of our panel: 20 grants totaling $50,000.

The establishment of the National Science Foundation (NSF) in 1950 led to greatly expanded support of research at existing observatories and ultimately to the founding of the national observatories. My own involvement in these developments began in 1952 with membership on the Ad Hoc Panel on Astronomical Instrumentation, followed by chairmanship of the Organizing Committee for the Flagstaff Photoelectric Conference in 1953. The concept of a national observatory that would provide observing facilities for visiting optical astronomers from all parts of the country took definite form in the recommendations of that conference. I served on the advisory panel headed by Robert R. McMath that saw this development through the gestation period. The consortium of universities known as AURA (Association of Universities for Research in Astronomy) was formed in 1957. Leo Goldberg headed the organizing committee that drew up the plan by which AURA would build and operate the observatory under contract with the NSF. Kitt Peak, near Tucson, was selected as the site in 1958.

There is no need to emphasize here the enormous influence that the founding of the national observatorics has had on the strengthening of the observational side of astronomy departments at many US universities. The benefits of equal (though peer-reviewed) access to large telescopes and good skies have been widely used. I have profited from my own opportunities to observe, both at Kitt Peak and Cerro Tololo.

By 1955 the Washburn group was beginning to question its complete dependence on grant-supported expeditions to observatories with large telescopes in more favorable climates. A modern telescope of moderate aperture at a rural site near Madison would, we felt, provide an observing base from which these expeditions could be more effectively organized. Frequent commuting to distant telescopes had not yet acquired its present status as a normal part of research costs. We developed a plan, and a grant from the Wisconsin Alumni Research Foundation financed the construction of the Pine Bluff observing station, centered around a new 36-inch reflector. The station was dedicated on 30 June 1958, the last day I was associated with the University of Wisconsin. My Wisconsin colleagues in the decade 1948–1958 included C. Morse Huffer, Harold Johnson, Arthur Code, and Theodore Houck. Wisconsin graduate students of these postwar years who have gone on to astronomical careers include Olin Eggen, Burt Nelson, John Bahng, Kenneth Hallam, and John Neff.

LICK OBSERVATORY

I came to Lick Observatory as Director in 1958. My first task was to see the 120-inch telescope through the final stages of its construction and to bring it into operation. The telescope had its inception years before in a plan for a postwar University of California building program. In late 1944, Nicholas Mayall and Gerald Kron, Lick astronomers contemplating their return from leave for work at wartime scientific laboratories, made an appointment to see Robert Gordon Sproul, the University's President. When they expressed their strong feelings about the need for a large modern telescope at Lick Observatory, there was an immediate response: Sproul assured them that such a telescope could be financed from state appropriations to the University. He said he would do all he could to follow their advice to make the new reflector as large as possible.

Donald Shane, who became Director at the end of the war, guided the project through the planning phases and oversaw the erection of the dome and telescope during the 1950s. He supervised a number of additions to the original very simple design and negotiated the necessary supplementary appropriations. In recognition of his central role in bringing the telescope into being, it was named the Shane Reflector in 1978.

The construction of the telescope had taken much longer than originally scheduled. When I arrived in 1958, the all-important primary mirror had not yet been brought to its final precise figure. I gave a high priority to monitoring the laborious regime of optical testing on the telescope by evaluation of real star images, interspersed with carefully gauged local polishing. This brought the mirror within specifications in June 1959. The first astronomical observations were made in October of that year, and regularly scheduled every-night operation began in March 1960. Although observing programs in that first year were limited to direct photography at the prime focus and spectroscopy with one of the coudé cameras, it quickly became clear that the performance of the telescope was fully up to expectation.

There could be no relaxation. The resident technical support group on Mount Hamilton—designers, machinists, opticians—bent their efforts to the completion of the full complement of auxiliary instruments. The ultimate responsibility for accelerating this work fell on me as Director. Previous experience in building astronomical instruments inclined me toward personal attention to the details of each project. As an example, I had to review the optical design of the thick-mirror Schmidt camera for the prime-focus spectrograph and to work with the optician in devising tools and testing procedures.

Involvement in these instrumental problems consumed a large fraction of my energy in the first four years on Mount Hamilton. As the various auxiliary instruments for the 120-inch telescope came into operation, the scope of the research undertaken by the Lick staff naturally expanded. They were joined by astronomers commuting from the Berkeley, Los Angeles, and San Diego campuses of the University, a constituency that had not previously made extensive use of the Mount Hamilton telescopes.

At the end of 1962, I was again drawn into serving on an advisory body concerned with national problems. The Committee on Science and Public Policy (COSPUP) of the National Academy of Sciences proposed a survey of the national needs for astronomical facilities over the next decade as an aid to rational planning of funding by federal agencies. I agreed to be chairman of a Panel on Astronomical Facilities. The panel of eight members included both optical and radio astronomers. The field of inquiry as outlined by COSPUP did not encompass space astronomy.

The report of the panel was published in 1964 under the title *Ground-Based Astronomy: A Ten-Year Program*. It was frequently referred to as the *Whitford Report*. Before the decade was up, the very rapid growth of observational astronomy into fields opening up as a result of technological advances led the National Academy and COSPUP to commission a second and broader survey, this time including space astronomy. The conclusions, generally known as the *Greenstein Report*, were published in 1972. The third, and even more comprehensive, examination of the whole range of astronomical activity in the US led to the publication in 1982 of the *Field Report*. There can hardly be any doubt that these studies helped to channel federal support into facilities that were identified through the collective judgment of the astronomical community as the most imperative needs in each decade. Most of the recommendations of our first panel were finally implemented, but not in the originally projected 10-year period. In retrospect, those recommendations now seem rather modest.

By 1965 problems of organization and governance within the University of California diverted attention from the anticipated concentration on exploiting the capabilities of the fully instrumented 120-inch telescope. It became apparent that Lick Observatory could not remain an exception to the new University policy that research scientists with faculty status would henceforth be appointed to posts in special institutes only if the terms included membership in a teaching department. A second problem involved the role that faculty members in the existing astronomy departments in the University should have in controlling the operation of the

Observatory. Resolution of these ambiguities had much to do with the decision to transfer the headquarters of Lick Observatory to the new Santa Cruz campus of the University, where the staff would take up residence and become the nucleus of a new graduate-level teaching department. The actual move took place in 1966, and the first group of graduate students came in 1967.

Thus ended a way of life and a tradition for Lick astronomers that went back to 1888, when living on Mount Hamilton next to the telescopes had been a logistical necessity. Continuance of this mode for nearly 80 years permitted a programming flexibility not conditioned by commuting from a different place of residence. The feuds and personality clashes that had come all too easily to dwellers in a closely confined mountaintop community in the early years had not been a serious problem in recent times. The self-reliance imposed by this isolation created a bond and a strong sense of group identity that Mount Hamilton residents carried with them into their new sea-level center of operations.

Once the transition to a plan of operation nearly universal at other major observatories was completed, any lingering nostalgia gave way to recognition that the research potential of the Observatory was in fact considerably enhanced. There could be daily contact with theoretical astrophysicists. Laboratory experimentation looking toward the use of complex new radiation detectors could be greatly expanded.

In 1968 I laid aside administrative responsibilities to return to teaching and personal research. After many years of being concerned with the *means* through which astronomical objectives are pursued, I could now devote myself to the *ends* themselves. The classroom teaching during the five years remaining before retirement from active faculty status brought me into contact with some excellent students whose astronomical careers I have followed with interest in subsequent years; among these are Jack Baldwin, David Burstein, Alan Dressler, and David Soderblom.

I have been fortunate to be able to continue my research into the years after formal retirement in 1973, thanks to the generous privileges accorded emeritus faculty members by the University of California and the facilities provided by Lick Observatory.

RESEARCH TOPICS

Photoelectric Technique

When I became involved in the problems of measuring very small photocurrents at Washburn Observatory in 1931, the potassium hydride photoelectric cell had held sway as the light-sensitive detector since 1913. Its superior response had displaced the selenium photoconductive detector that Stebbins had been using on stars brighter than the third magnitude at the University of Illinois in the years 1907–1913; with it he had produced the classic precise light curve of the eclipsing binary Algol, often reproduced in textbooks. Prior to 1907 there had been a hiatus since 1895, the year in which G. M. Minchin had succeeded in detecting the brightest stars with a selenium cell at the focus of a 24-inch reflector near Dublin.

All of the potassium hydride cells used by Stebbins had been made by Jakob Kunz in the physics laboratory at the University of Illinois. I witnessed the process in the late 1930s; it was still an art, dependent on Kunz's instinct and skill. These cells were prized because of their extreme low dark current. Though considered very sensitive at the time, they were far short of the ultimate. Their quantum efficiency was about 10% at their peak response in the blue, and the bandwidth, down to 10% of peak, was 1300 Å.

The electrometer used to measure the photocurrents also fell considerably short of the ultimate limit set by the statistical shot-effect fluctuations in emission of electrons from the cathode of the cell; the required charge sensitivity had been achieved only in laboratory electrometers too delicate to mount on the end of a moving telescope.

The considerable improvement realized by the installation of the vacuum-tube amplifier (see the section on Beginnings) was not enough to reach the ultimate limit. The residual grid current was still about 6000 electrons s^{-1} and added a statistical fluctuation noise to that of the true photocurrent. Nevertheless, a rate-of-charge technique permitted us to reach sixteenth magnitude with a potassium hydride cell on the 100-inch telescope. This was about 25 times fainter than would have been possible with the electrometer in use before 1931. Stebbins and I continued to use the single-stage amplifier and galvanometer to measure photocurrents until 1946.

The introduction of amplification in 1932 was the first step in a succession of improvements over the next 25 years that permitted the measuring of objects at least 10^4 times (10 magnitudes) fainter than those possible in 1931 with a given telescope. From a technique applicable only to selected classes of relatively bright objects, the photoelectric process became in the end the basic standard for quantitative measurement of any astronomical radiation that could be recorded on a photographic plate. Quite a few persons had roles in exploiting the technological advances that contributed to this revolution in observing procedures. In my recollections of this period, certain crucial steps stand out. More details and full references are given in the historical review by David H. DeVorkin in *Proceedings of the Institute of Electrical and Electronic Engineers* 73: 1205 (1985).

The advances along the way were divided in nearly equal proportions between improvements in cathode yield and improvements in the minimum charge or current that could be reliably measured.

In the first category, John Hall's introduction of cells with cesium-oxide-on-silver cathodes at Yale University in 1932 brought the advantage of a useful spectral response from 3500 to 11,000 Å. Spectrophotometric observations could now be made over a much broader range of wavelengths than had been possible with the Kunz potassium hydride cell. There was no improvement in quantum efficiency; in the red and infrared region it was not over 0.5%. Hall was also the first to use refrigeration by dry ice to reduce to a very low value the thermal emission at room temperature from the cathode; for this new red-sensitive surface, it was many times that from fairly bright stars. In 1937 Stebbins and I began using a photometer built around one of these new cells in our Mount Wilson observing programs.

The most significant improvement in cathode response during the 1930s was the order-of-magnitude increase in quantum efficiency that came with the development in 1936 of the antimony-cesium surface by P. Görlich in Germany. Its useful spectral range was from 3400 to 6000 Å, and the peak quantum efficiency in the blue was over 10%. Trials of cells with this cathode made in US industrial laboratories, at Washburn Observatory, and at Lick Observatory (by Gerald Kron) were very favorable, but they came too near the wartime interruption of instrumental development for any immediate application to astronomical research.

The most important development on the measurement side was the introduction of multiplication by secondary electron emission as the method of amplification. A chain or cascade of nine or more of these secondary emission targets, all in the same glass envelope with the cathode, could produce a pulse of 10^6 electrons for each photoelectron released at the cathode. The amplification was virtually noise free.

In the early stages of this development, the electrons were focused from stage to stage by a magnetic field. A multiplier based on this principle was made available to Stebbins by Vladimir K. Zworykin of the RCA Electronics Research Laboratory. In 1937 Kron and I used it in a demonstration of the first automatic guider for correcting small errors in keeping a telescope pointed on a star.

A compact, electrostatically focused multiplier with an antimony-cesium cathode was developed at the RCA Laboratories in 1940. An improved version known as the 1P21 became generally available at the end of the war. Kron used it uncooled at Lick Observatory to demonstrate its great potential in astronomical photometry. In independent developments, Kron and I each put this multiplier into a refrigerated mounting; the cathode dark current fell to a few electrons per second. Each of us devised an amplifier package that brought the output to the level needed to feed a standard pen-and-ink chart recorder. The amplifier input was high enough to avoid the introduction of additional noise.

The advent of the 1P21 also broke a psychological barrier. Photoelectric photometry was no longer the province of a small band of specialists. Now relatively uncomplicated, the technique was taken up at quite a number of observatories and applied to problems such as the color-magnitude diagrams of open clusters and globular clusters. The growing body of US observers met at the Flagstaff Conference (see section on Washburn Observatory) in 1952.

The transition from dc amplification of the multiplier output current to pulse counting of individual photoelectrons began in the late 1940s. This was advantageous for the long exposures needed to detect and measure stars at the limit of photography, stars that could not be seen and centered by visual methods. These had to be put in the entrance aperture by an offset, measured on a photograph from a star bright enough to be seen visually at the telescope. In 1955 William Baum successfully measured a star of magnitude 23, at the photographic limit of the 200-inch telescope. Magnitude sequences going beyond the twentieth magnitude have become standard procedure in later applications.

I have been no more than a witness to the final part of the photoelectric revolution: the advent of the so-called panoramic detectors that have extended the advantages of high quantum efficiency and linear response to thousands (or as many as a hundred thousand) of picture elements simultaneously viewed in the focal-plane image delivered by the telescope or spectrograph. Observers no longer have to reckon with the long exposure times required by sequential viewing of one element at a time. I salute those responsible for devices such as image-intensifier tubes, including schemes for recording and storing in the proper specially labeled bins the flashes on their output screens; for silicon target vidicons; and finally for charge-controlled devices (CCDs). Though the number of simultaneously recorded channels will undoubtedly increase, contemporary techniques have come very close to the ultimate of capturing all the photon information in the radiation incident on each picture element.

Interstellar Absorption and Reddening

Stebbins had initiated an extensive survey of space reddening in the Galaxy well before I became a collaborator in 1933. Huffer had been involved from the first. Stebbins had chosen B stars and globular clusters as test objects. The investigation had its inception in Robert J. Trumpler's historic demonstration in 1930 that there is general interstellar absorption in the

Milky Way. Trumpler concluded that the angular diameters of galactic star clusters could be reconciled with the clusters' apparent photometric distances only if the distances were revised downward by allowing for 0.67 mag kpc^{-1} mean absorption in the photographic light; since the absorption in visual light was only about half as much, there was also an indication of reddening by the absorbing interstellar material. He warned, however, that many local irregularities were probably smoothed over in the average value.

In any case, the ghost that had haunted astronomers for many years had now been proven to be very real. Harlow Shapley's first estimate of the size of the Galaxy, based on his conclusion that there was no evidence for absorption in front of the distant globular clusters in the direction of Sagittarius, now appeared to be much too large.

When finally completed, our survey gave colors and color excesses (reddenings) for 1332 B stars and 68 globular clusters. In retrospect, it is difficult to understand how the large effects that we turned up could have been missed in previous photographic studies. The photoelectric method was ideally suited to precise determinations of colors of isolated and widely separated objects; transfers of photographic magnitude sequences from standard areas were not necessary. The photoelectric color came from the relative intensity through two filter glasses (called "yellow" and "blue") interposed in quick alternation. The ability of the photoelectric cell to integrate the total light of a globular cluster overcame a serious difficulty in measuring photographically the magnitude for a diffuse object.

Our surveys, published in 1939 and 1940, showed that the interstellar dust clouds are strongly concentrated toward the galactic plane, and that there is more reddening toward the center than toward the anticenter; these conclusions, now taken for granted, were not obvious before that time. We found the reddening to be very unevenly distributed, however, and it was clear that a mean coefficient of absorption could not be used in correcting photometric distances.

Our investigations of the dependence of interstellar absorption on wavelength began in 1937. The first results came from a photoelectric scanner based on an existing slitless spectrograph that had been designed for the Newtonian focus of the Mount Wilson telescopes. A more extensive study used six broad-band filters with band centers ranging from 3530 to 10,300 Å; this program was completed by Stebbins during the war years. In 1948 and 1949 I was able to extend the range of wavelengths to 2.2 μm with the aid of a lead sulfide cell obtained from R. J. Cashman. In 1952, a photoelectric scanning spectrometer, developed in collaboration with Arthur Code, gave quite accurate spectrophotometric measurements.

The spectrophotometric comparisons of reddened and unreddened O and B stars by these different methods were in satisfactory agreement. In 1958 I brought all of them together in the formulation of a "law of reddening" that has not required appreciable revision in the face of later observations of the part of the absorption curve accessible to ground-based telescopes.

The general shape of this relation showed a variation $A_\lambda \propto \lambda^{-1}$ over a considerable interval, as earlier photographic observations by others had suggested. The observed curvature in the infrared was in line with the theoretical predictions by Oort and H. C. van de Hulst. The same reddening curve fitted observations of absorption in many directions in the Milky Way; the properties of the dust cloud in front of the Trapezium stars in the Orion Nebula were, however, a conspicuous exception. Not until observations at wavelengths $\lambda < 3000$ Å from vehicles above the terrestrial atmosphere became possible (notably by the Wisconsin package aboard the Orbiting Astronomical Observatory OAO-2) did the considerable diversity in the nature of the dust particles in various interstellar clouds show up.

Magnitudes of Stars and Galaxies

In 1930, during his first season of photoelectric observations at Mount Wilson, Stebbins measured the brightness of a number of external galaxies. The advantage of the integration of any surface brightness distribution, whether concentrated in a star image or diffuse in a galaxy image, coupled with simple linear subtraction of the "sky" contribution as gauged in an adjacent blank area, avoided the serious complications in photographic evaluation of such quantities. Hubble and Baade, aware of these difficulties, had urged the initiation of Stebbins' photoelectric program. I joined in the work on the long-term project in 1933; our first published report on the measures with the potassium hydride cell appeared in 1937. Our postwar extension to fainter galaxies with the 1P21 photomultiplier, published in 1952, reached the eighteenth magnitude.

The importance of accurate comparison of the total light of nearby galaxies (hence more diffuse in apparent angular extent) with much fainter ones having significant redshifts was obvious at the outset. The nearest large galaxies containing resolved high-luminosity stars had too great an angular extent, however, to be contained in the focal-plane apertures of the large reflectors at Mount Wilson. I therefore undertook observations of 11 bright systems with lens-type objectives of 3.5- and 10-inch diameter. The results, published in 1936, were the subject matter of my first sole-author paper on an astronomical topic.

During the 1950s other workers undertook extensive programs of galaxy photometry, mostly photoelectric measures with the 1P21 photomultiplier.

The contributions of Gerard de Vaucouleurs stand out. The results of various observers were brought together and systematized in the *Reference Catalog of Bright Galaxies* (RCBG), compiled by Gerard and Antoinette de Vaucouleurs and published in 1964. In most cases the galaxies included in our two early series were later observed by others; the RCBG magnitude for such systems is the weighted mean of all measures.

Stebbins and I were also engaged in a long-term program to check the validity of the International Photographic and Photovisual magnitudes of stars then in general use as reference standards. These studies had received their initial impetus from Hubble and Baade, who wanted accurate magnitudes for stars in certain Selected Areas against which luminous resolved stars in external galaxies (e.g. Cepheid variables) were compared in order to get a distance modulus.

In 1947–1949 we extended with a 1P21 photomultiplier a calibration of the North Polar Sequence we had made with a potassium hydride cell. There were discordances around the sixth magnitude but no appreciable scale error from the seventh to the fifteenth magnitude. Beyond the fourteenth magnitude the stars in three Selected Areas showed a serious scale error. The 1949 measures, in which Harold Johnson had a major part, showed an error of as much as 0.7 mag at the nineteenth magnitude; the error was in the sense of making the stars fainter than the values listed in the *Mount Wilson Catalogue of Magnitudes in Selected Areas*. The discrepancy was considerably larger than Baade's photographic recalibration had suggested. It was one of the factors entering into his upward revision of the distances of galaxies of the Local Group, such as M31.

These corrections for scale errors in existing photographically determined magnitude standards were useful interim measures. Efforts of this kind also made clear the importance of a self-consistent, wholly photoelectric system of magnitudes, divorced from the North Polar Sequence; by this time the stars of this sequence were known to be slightly reddened.

The *UBV* magnitude system established by Johnson and W. W. Morgan in 1953 met this need and soon became generally adopted as the new standard. It depended on the response curve of the 1P21 cathode and three precisely defined filters; the *B* band (the blue band, which closely resembles that of the old International Photographic magnitudes) excluded any contribution from the luminosity-sensitive wavelengths near the Balmer discontinuity. The *U* (ultraviolet) magnitude added, among other things, a discriminant between temperature-reddened and space-reddened stars. Since photoelectric photometers built around the 1P21 photomultiplier had come into use at many observatories, magnitudes and colors of stars in open clusters, globular clusters, and Local Group galaxies could henceforth be determined by reference to the fundamental *UBV* standards set up by Johnson and Morgan. This transition to a uniform, clearly defined magnitude system was one of the more important steps along the way in the photoelectric revolution.

Spectrophotometry of Galaxies

Beginning in 1940, we used the same six-color filters with band centers ranging from 3530 to 10,300 Å that had served in a comparison of reddened and unreddened stars for a study of the spectral energy distributions of galaxies. The wide range of wavelengths provided the first unequivocal demonstration that the integrated radiation came, as expected, from a mixture of stars of different temperatures; there were excesses of both ultraviolet and infrared light over the amounts expected from an average star as judged from the midrange spectral features. The strong infrared radiation indicated that cool M stars formed an important component.

The effect of redshift on the color of galaxies could be calculated from the shape of this first crude energy curve. The predicted change in color of an elliptical galaxy [the type dominating the distant cluster with the largest redshift known in 1948 ($z = 0.13$)] was, however, less than half that actually observed. The discrepancy, which came to be known as the "Stebbins-Whitford effect," was tentatively ascribed to evolutionary changes in the intrinsic color during the light-travel time. The implausibly rapid evolution rate that this interpretation required immediately raised doubts. Gerard de Vaucouleurs put forward the suggestion that the smooth energy curve drawn through the points derived from six broad-band filters could not take account of a sharp decline in the energy curve of certain types of stars near 4000 Å; such a "guillotine" would make for a considerable increase in the calculated change in color of a redshifted galaxy.

During the mid-1950s, Code and I developed a photoelectric scanning spectrograph that was built in the Mount Wilson shops. With it, Code obtained the first detailed energy curve of M32, published in 1959. There was indeed a steep falloff near 4000 Å quite like that of giant stars. The calculated color of a redshifted galaxy no longer showed the embarrassing discrepancy. The "effect" could be laid to rest.

I later obtained well-calibrated scans of energy curves of nearby giant elliptical galaxies in the Virgo cluster with a scanner at Lick Observatory. They showed the dropoff near 4000 Å in greater detail. Comparable energy curves were observed by J. B. Oke and his collaborators with a similar scanner at Palomar. Allan Sandage's 1973 review of calculated and observed colors of giant elliptical galaxies with redshifts up to $z = 0.46$ found no evidence for evolutionary changes over the look-back time involved.

The Galactic Bulge

Shapley's much-quoted remark comparing the Milky Way with the external stellar systems serves as a reminder of how the cosmological scale of things has changed in the half-century since I came into astronomy. Regarding the island universe concept, Shapley said "If we call them islands, the Galaxy is a continent."

My first involvement in the issue of these relative sizes came in 1933, the first season that I worked with Stebbins at the Mount Wilson telescopes. We made photoelectric scans across the central region of M31 and found that the light of this galaxy could be detected out to about twice the angular extent seen on photographs. The eventual realization that M31 is in fact a larger spiral than the Galaxy came two decades later, when Baade showed that the distance to this member of the Local Group must be revised upward to nearly three times Hubble's first estimate.

Baade's discovery that the outer part of the great star cloud in Sagittarius is in fact a part of the central bulge of the Galaxy led to the speculation that if the absorption that blots out the bright nuclear region were reduced by observing in the infrared, some features of the inner bulge might be revealed. In 1945 and 1946 Stebbins and I therefore made strip scans of the Sagittarius region in a wavelength bandwidth at 10,300 Å. Reddening studies had shown the absorption in this infrared band of the six-color system was less than half that at visual wavelengths. At a resolution of 8 arcminutes, a broad maximum of infrared light showed up in the expected direction along the galactic equator, but we did not find a sharp central peak denoting the nucleus.

In 1947 I scanned the same region at a wavelength of 2.2 μm using a lead sulfide detector. The optics of the searchlight mirror system set up on Mount Hamilton for this purpose sacrificed resolution in favor of detection of faint surface brightness. The smooth intensity maximum along the galactic equator was now quite prominent. Any sharp nucleus would, however, have been blended into a large patch (about $1^\circ \times 1^\circ$) of surrounding bulge light and thereby rendered undetectable.

In 1966, with much higher resolution and a better chopper design for observing at 2.2 μm, Eric Becklin and Gerry Neugebauer found detection of the sharp nucleus at a Mount Wilson telescope quite straightforward. Their value of the total absorption was much higher than our early estimate; it indicated that detection of the nucleus at 10,300 Å had been a vain hope.

My next concern with the galactic bulge came years later and was motivated by the question, Do the resolved stars seen in the windows identified by Baade as providing lines of sight penetrating through the central region of our Galaxy constitute a typical sample of the stellar population in the type of galaxy represented by ellipticals and the bulges of spirals? In order to put this question to an observational test, I made a narrow-band spectrophotometric comparison of light from patches of the bulge in the Baade window at $b = -3^\circ\!.9$ with that from corresponding areas in other galaxies. The bulge observations were made in 1972 from Cerro Tololo with a sequential scanner similar to the one Joseph Wampler had developed in the mid-1960s at Lick Observatory; the latter instrument was used to observe several comparison galaxies. The bulge light proved, not unexpectedly, to be very similar to that of other galaxies. Subtraction of the light of the terrestrial airglow and of the foreground stars of the Milky Way permitted a quantitative comparison that had not been possible for the photographic spectra of bulge light obtained by Morgan in 1958.

This confirmation cleared the way for a star-by-star study of the properties of the giants of the bulge population for comparison with the properties postulated for the stars in the population synthesis models that had been constructed to give the best match to the spectral characteristics of the integrated light of unresolved galaxies of similar type.

The observations of bulge stars that have followed in the years sincc are incomplete and still in progress. Two results that have emerged illustrate how this approach can give more specific information about the population than can be determined by the model-fitting method.

The first has to do with the metal content of the bulge population. Spectra of bulge K giants in the Baade window that Michael Rich and I obtained at the Las Campanas Observatory in 1980 showed a major fraction of them to be super-metal-rich (SMR), with an upper extreme having metal abundance 5 to 10 times that of the Sun. This result confirmed the hypothesis that had been put forward to account for the very strong spectral features in the spectra of the more luminous galaxies. There were also a minor fraction of metal-poor stars among the bulge giants; this was not surprising, since the metal-poor RR Lyrae variables known since Baade's time must have giant progenitors. It is thus clear that the single "average metallicity" adopted in the synthesis models is an oversimplification, and that the different theoretical evolution paths for metal-poor and super-metal-rich stars must be taken into account in calculating the spectrum of the integrated light.

The second illustration concerns the evolutionary history of the late M giants in the bulge population, stars first identified in 1958 on objective-prism plates of bulge fields by J. J. Nassau and Victor Blanco. The more recent observations of Victor Blanco, Martin McCarthy, and Betty Blanco have provided a complete census of stars of this type in several windows along the minor axis of the Galaxy. These giants are the resolved examples in our own Galaxy of the cool stellar component responsible for the strong infrared radiation in the integrated light of external systems. This component stood out in the broad-band colors of galaxies by Stebbins and Whitford in 1948, by Johnson in 1966, and by Jay Frogel et al. in 1978.

Population models for external galaxies have generally adopted late M giants in the solar neighborhood as prototypes for the stars that contribute this infrared light. These very luminous stars are definitely on the asymptotic giant branch. It was not until evolution-track calculations for such stars became available (e.g. those of Icko Iben and Alvio Renzini) that it became clear that among stars of solar metallicity, only those with an initial mass of at least 1.5 $M_\odot$ are able to survive the high-mass-loss rates of the upper asymptotic branch and reach luminosities as high as 1.25×10^4 $L_\odot$; the age of these giants is not more than 2×10^9 yr. In the disk population, where star formation is a continuing process, rather young stars of this mass range are to be expected. But in the old population found to give the best fit to the spectral characteristics of the integrated light of elliptical galaxies and the bulges of spirals, M giants of this initial mass would have died long ago and would not have been replaced.

Recent deep color-magnitude diagrams of bulge fields derived by Donald Terndrup from CCD photometry are consistent with theoretical isochrones of a population at least 10^{10} yr old. The diagrams do not show a clump of stars at the theoretical main-sequence turnoff for younger stars of the mass range that could be progenitors of M giants like those in the solar neighborhood. Yet there are M giants in these fields that have TiO band strengths corresponding to those in spectra of the local standards that define types M6 and M7.

These results suggest that bulge M giants and local disk giants of the same spectral type (as judged by TiO band strength) differ in their physical characteristics, and that they differ in a way that allows the bulge giants to be old stars. Multicolor photometry that Frogel and I have accumulated in a joint program shows that this is indeed the case: Bulge M giants in each spectral subclass are hotter and less luminous than their counterparts in the disk population near the Sun.

The observed differences can be understood if the late M giants in the bulge are all SMR, having evolved from the fraction of K giants in the bulge population known to be SMR. Calculated evolution tracks show that stars of mass 1.0 $M_\odot$ in this SMR component can be $10\text{–}12 \times 10^9$ yr old at the start of their giant evolution. Compared with metal-poor stars, they therefore have more mass available as fuel as they ascend the asymptotic giant branch. Furthermore, they would show the spectral signature of (say) M6 giants at the higher temperature of disk giants of an earlier type (perhaps that of type M4), simply because the greater abundance of both Ti and O favors molecular association. At the higher temperature and lower luminosity of the disk M4 giants, the bulge M giants classified as type M6 would not be subjected to the high mass-loss rates of the upper asymptotic giant branch (a fate that would require them to be young); hence, they could be coeval with all the old stars of the bulge population.

This hypothesis needs to be checked by studies of bulge M giants at higher spectral resolution. It is most unlikely that any bright SMR prototype of this class of bulge stars will be discovered among the late M giants of the solar neighborhood. I am eager to see what observations of seventeenth magnitude bulge giants can show; I believe that existing ground-based telescopes can furnish decisive clues.

The stellar population of the galatic bulge is the nearest, best-resolved sample of a large class of galaxy populations. This class is heavily represented in spectral studies of galaxies to a given magnitude limit because of the high surface brightness. The resolved individual stars in the bulge provide a unique opportunity to test models that seek to explain the present-day proportions of stars on the upper part of the giant branch in these galaxies, and also the proportions at that earlier time in their spectral evolution that is now becoming observable in spectral studies of very distant systems at look-back times.

I await the outcome of the projected programs of a number of observers with high expectations.