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JOSEPH W. CHAMBERLAIN
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A Biographical Memoir by

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Kitt Peak National Observatory photo

J W Chamberlain

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JOSEPH W. CHAMBERLAIN was the son of a country doctor, and it was assumed that he, as well as his older brother Gilbert, would also become doctors. His first laboratory experience in comparative anatomy as a college freshman convinced him to switch to physics and then astronomy. His first job after obtaining his doctorate was at the Air Force Cambridge Research center and involved the study of the Earth's upper atmosphere through spectroscopic observations of the aurora and the faint emissions from the night sky, called airglow. Interpretations of these data led to studies of the upper atmosphere from which they came, and then led to work on the interplanetary medium and the atmospheres of the other planets. Shortly after the dawn of the space age, he left the University of Chicago for the recently formed Kitt Peak National Observatory, taking the opportunity to initiate a program of sounding rockets and to assemble a group of observers and theorists who could, and did, become interested in studies of the upper atmosphere and atmospheres of other planets. Although Chamberlain really preferred to continue his own scientific work, his leadership helped this group (of which the present writer was a member) to expand into observations with telescopes and rockets and then to become involved in NASA's planetary program. After a dozen years of administrative work, including two years at the Lunar Science Institute, he returned to the academic life and later retired back to Tucson. In his last seven years he was crippled by the effects of a stroke. Former members of his group continued to have the weekly lunches that he had organized, and he participated until his last year, when he became too weak. He and his wife, Marilyn, were enthusiastic golfers and connoisseurs of opera and orchestral music; they raised three children.

Joseph Chamberlain was born in 1928 in Boonville, Missouri, and lived his entire childhood in New Franklin, Missouri, a town of about 1,200 people located three miles from Boonville. In high school he was active in several extra-curricular pursuits, especially basketball, band (where he played first-chair cornet), and the high school newspaper, where he wrote a weekly sports column. It was here that he first developed an interest in writing as a creative activity in itself. He attended the University of Missouri, quickly switching (as mentioned above) from pre-medicine to physics, finishing with an A.M. in June 1949, and then transferring to the Astronomy Department at the University of Michigan. He was married just before entering that university, on September 10, 1949, to Marilyn Jean Roesler, daughter of a Milwaukee dentist, whom he had met two years previously, when she attended Stephens College in Columbia, Missouri. His research professor and thesis advisor was Lawrence Aller, and the chairman of the department, Leo Goldberg, was also an exemplary instructor and advisor.

Chamberlain's first publication dealt with the atmospheres of certain stars that exhibited peculiar A-type spectra and that had been previously interpreted qualitatively as being deficient (compared with "normal" stars) in their hydrogen abundance. The quantitative spectral analysis that he did with Professor Aller indicated that the opposite was true;

the stars were cooler than implied by their incorrect A-type classification and actually were F-type. This paper turned out to be the first one clearly exhibiting large metallic deficiencies in certain Population II stars, and it helped open up a rather major field of investigation of cosmic abundances and their relationship to stellar evolution. Of course, the real interpretation was done by Lawrence Aller; at that time Chamberlain was merely a graduate student, learning as he assisted Aller with the technical details.

Chamberlain went to work with the Air Force Cambridge Research Center, Geophysics Research Directorate, in the Boston area, in December 1951, about six months before actually receiving his Ph.D. Much of the writing of the thesis and final analysis of results were done while he lived in Brookline. There he developed an interest in aurora and airglow, through his affiliation with Norman J. Oliver. Under Oliver's supervision he made two brief wintertime expeditions to Thule, Greenland, which is very near the north geomagnetic pole, where he obtained spectra of the airglow over the polar cap (i.e., inside the auroral zone). Their most significant result was that the spectrum emitted by the OH radical at high latitudes exhibited a considerably higher spectroscopic temperature than it did at middle latitudes. While they made no attempt to explain the effect at the time, this was apparently the first indication that the mesopause region (at a height of about 90 km) over the polar cap was warmer in winter than the same region at middle latitudes, a result that has since been verified by other means and explained as a consequence of the dynamical circulation of the upper atmosphere.

Part of his job at the Air Force Research Center was the monitoring of three contracts at universities. This work introduced him to Ralph Nicholls at the University of Western Ontario; the present writer; A. Vallance Jones at the University of Saskatchewan; and Aden B. Meinel at Chicago's Yerkes Observatory. When he later expressed a desire to work with Meinel, it was arranged by Oliver that, starting in July 1953, he would spend some six months with him at Yerkes Observatory in Williams Bay, Wisconsin. At the end of the six months, Meinel offered him a job working on the contract that he had previously monitored, and he accepted with alacrity, remaining then at Yerkes for nine productive and enjoyable years.

The Chamberlains's first born, Joy Anne, arrived in February 1953, a few months before they moved to Wisconsin. Two sons, David Wyan and Jeffrey Scott, were born in Wisconsin in 1956 and 1957, respectively.

At Yerkes Observatory he was influenced not only by Meinel, before he left for Arizona to take charge of the development of the Kitt Peak National Observatory, but also by Professors Bengt Strömgren, then the director of Yerkes; G. P. Kuiper, who later became the director; and perhaps most of all by S. Chandrasekhar.

When Meinel left Yerkes Observatory, Chamberlain had been appointed assistant professor, and he took charge of the Air Force contract. Working with him on the program in laboratory work was C. Y. Fan. And in 1957 Lloyd B. Wallace came to Yerkes to do aurora-airglow spectroscopy on the contract, having just completed a doctorate in astronomy at the University of Michigan. Thus began many years of a most enjoyable collaboration and friendship.

In March 1960, upon Kuiper's departure from Yerkes Observatory, W. W. Morgan was appointed director, and he selected Chamberlain to work with him as associate director. The arrangement, which worked surprisingly well for more than two years, was that Chamberlain should handle day-to-day routine administrative matters (which were not too burdensome at Yerkes) and Morgan would deal with the larger problems. Chamberlain at that period thought of himself as associate everything: associate director, associate professor, associate editor of the *Astrophysical Journal* under Chandrasekhar's editorship. In January 1961 he was appointed professor and shortly thereafter was given a joint appointment in the Departments of Astronomy and Geophysical Sciences at the Chicago Quadrangle campus of the university.

His first book, "PAA" (*Physics of the Aurora and Airglow* [1961]) was written between July 1957 and April 1960. This classic book contains an enormous range of material and was in large part responsible for the intense respect in which the community held him rather early in his career.

Shortly thereafter he received a phone call from N. U. Mayall inviting him to head an operation in space astronomy at the Kitt Peak National Observatory (KPNO) in Tucson, Arizona. He moved to Tucson in early October 1962, and during the next two years assembled a staff of approximately 10 astronomers and an engineering organization under the direction of Russell A. Nidey, the systems manager of the Space Division at KPNO. The principal staff members who joined him in Tucson were Lloyd Wallace, from Yerkes Observatory, who became the principal experimenter in the development of the rocket astronomy program and the author of this memoir, who came from the University of Saskatchewan to take charge of the program in laboratory astrophysics. In the fall of 1964 Michael B. McElroy joined the group for a one-year postdoctoral "visit"; he had studied with Alex Dalgarno and David Bates at Queens in Belfast, and then spent a year with Joseph Hirschfelder's group in Madison. McElroy fitted in so well that he remained for six years and eventually left to become a Harvard professor.

The administrative side of science was something that Chamberlain entered with considerable trepidation, and he felt the increasing burden of it and its competition with the amount of research that he would like to do. On the other hand, the administrative accomplishment itself of assembling and keeping a fine research organization in planetary and space science was as gratifying and as enjoyable as any of his significant research. It was a close group that worked actively together and that assembled daily around the coffee pot for off-the-cuff discussions. The youthful spirit and rapport within the Space Division (later called the Planetary Sciences Division) was infectious.

At that time KPNO was organized into three scientific divisions. The functions of the other two, the Stellar Division and the Solar Division, were to build telescopes and other equipment and to provide observing time for university astronomers. The Space Division existed because Aden Meinel, the first director of KPNO, began the development of a remotely controlled telescope with the objective of learning how to control a telescope in Earth orbit. Under Nidey's management a rocket astronomy program was also initiated, with emphasis on fine pointing, which allowed the observation of preselected objects. This program had two major continuing problems: (1) Each flight was very expensive, even by rocket standards, because of the pointing controls, and (2) few guest observers could be accommodated on a budget that allowed only about three flights per year. The program and the division were in a constant battle for survival—hence the shift in title in 1967 to Planetary Sciences Division. Early flights, mostly with Wallace and Hunten as investigators, obtained important results on the day airglow from the upper atmosphere; they still required pointing of the instruments but were not as demanding as the task of orienting a telescope at a planetary or astronomical object.

There was also a project to design a small astronomical satellite, which attracted attention from NASA, and at one time it seemed that NASA would enter a cooperative program with KPNO to fund it. Eventually these plans fell through, but NASA a few years later took over the concept and did fly the satellites. The development of the remotely controlled telescope, a 50-inch aluminum mirror on Kitt Peak controlled by a computer in Tucson, was also continued. Around 1968 that project was taken away and the telescope turned over to the Stellar Division and converted for ordinary use by astronomers in the observing dome. A couple of years later the expensive rocket program was terminated.

Chamberlain was elected to the National Academy of Sciences in 1965. The next year he was invited through Gordon MacDonald's recommendation to join JASON, a group of 40 or so physicists who spend part of their summers working on applied physical problems for the government. Because much of the work was militarily oriented and classified, JASON became much maligned in the antiwar movement of the late 1960s. It was not widely known that almost all the JASONs were strongly against the war in Southeast Asia but retained their ties with the Pentagon in the hope of being able to do something useful within the system. Many of them also worked on environmental problems, including some associated with the effects of a massive exchange of nuclear weapons detonated in the upper atmosphere. Chamberlain's interest in modifications of the stratosphere and human-induced changes in the climate was largely stimulated by such work.

The major scientific interests in the division had become purely planetary, without justification of space technology. Hunten and Michael Belton increased their observing programs of the planets with the McMath solar telescope on Kitt Peak. Richard Goody of Harvard became a frequent visitor and helped convince the board of the Association of Universities for Research in Astronomy (AURA) of the quality of the research being done in the division. In February 1967 the first of what was to become an annual conference, generically called the Arizona Conference on Planetary Atmospheres, was organized. Each year a different topic was chosen, a different member of the staff served as organizer, and a somewhat different list of invitees drawn up. These meetings, held in the best season of the year for Tucson, were very successful and filled a need for planetary conferences that was not at that time being met by either the American Astronomical Society or the American Geophysical Union. In September 1970 McElroy left KPNO and in March 1971 Chamberlain left, and the division began to disintegrate. The last such conference was held in March 1971, but there was another reason for their cessation.

In 1967 a small group of planetary astronomers led by Carl Sagan petitioned the American Astronomical Society to form a division for planetary astronomy. There was a precedent in that solar astronomers had recently formed such a group. The council responded by asking Chamberlain to serve as chairman of the organizing committee, which he did. The principal task was to draft the bylaws in accord with the bylaws and constitution of the parent society, a task that was not made any easier by Martin Schwartzschild's having succeeded Al Whitford as president of the society.

In December 1968 at the American Astronomical Society meeting in Austin, a symposium on planetary atmospheres was convened, and the organizing committee met to modify the draft bylaws. After review by the council the bylaws were adopted by a mail ballot in May 1969. The first official meeting of the Division for Planetary Sciences was held at the Jack Tar Hotel in San Francisco in January 1970. Chamberlain served as chairman of the division the first year; its annual meetings are attended by a large fraction of the world's planetary astronomers.

In the spring of 1970 Chamberlain was rudely informed that the rocket engineering group was removed from his control; he immediately resigned as associate director and a few months later accepted an offer to become director of the NASA Lunar Science Institute, located beside the Johnson Space Center in Houston. Relations with its governing board were even rockier than they had been at KPNO, and after two years he took up a professorship at Rice University. He remained there until he retired as emeritus professor in 1990. He had several graduate students, and among his classes was a graduate-level one in planetary atmospheres, which was the basis of his second major text, *Theory of Planetary Atmospheres* (1978). A decade later he invited the writer of this memoir to join him in preparing a second edition, which was published in 1987.

In Houston they bought a house from Buzz Aldrin, who had been on the Apollo 11 lander with Neil Armstrong. The existing support staff at the Lunar Science Institute (LSI) was in a state of anarchy. Most of them did not have enough work to do, and office intrigue was rampant. Within a few months Chamberlain had removed the worst of the troublemakers, put out a brochure describing LSI and the resources it could provide university scientists, and organized half a dozen conferences in the manner of the Arizona conferences on different topics. The LSI also managed the review of NASA proposals for the analysis of lunar samples.

One of the sadder aspects of leaving LSI (in June 1973) was that he ceased working with Helene Thorson, who had been his assistant for more than 15 years at Yerkes Observatory, KPNO, and LSI. She remained at LSI, where she served as executive assistant to an assortment of directors that followed.

After Chamberlain arrived in Houston he was appointed adjunct professor (without salary) in the Space Physics and

Astronomy Department at Rice University. Alex Dessler had founded the department and had earlier wanted him to join it. The current chairman was Ronald Stebbings, whom Chamberlain had earlier tried to hire at KPNO to start a new scientific division in laboratory astrophysics. (Lacking enthusiastic support from the director and the AURA board he called Stebbings in London and told him the job was approved but the outlook for an expanded laboratory of the scale we both envisioned did not look good. Consequently he took an offer from Rice instead.)

One job Chamberlain undertook was the editorship of *Reviews of Geophysics and Space Physics.* He accepted because he had worked with Chandrasekhar for several years as associate editor of the *Astrophysical Journal* and because his lunar science experience gave him some familiarity with subjects and people in solid-earth geophysics.

RESEARCH ACCOMPLISHMENTS

Aurora and Airglow Spectra. In 1952 Chamberlain obtained, on an expedition to the U.S. Air Force base at Thule, Greenland, the first spectra of OH airglow from inside the auroral zone and first demonstrated substantial OH rotational temperature anomalies between polar and temperate latitudes.

In 1953-1954 he obtained at Yerkes the first airglow spectra in the near ultraviolet with sufficient dispersion (23Å/mm) to resolve the rotational structure of the bands. These spectra gave the first positive identification of these bands as the Herzberg forbidden system of O_2 and showed that the rotational temperature was less than about 200 K. The latter result placed the emitting level in the 80-100-km region, much lower than contemporary photometric studies had indicated. The exposure required for the best spectrum was 75 hours, most of the dark, moonless hours of one entire month. In 1958 he finally obtained rotationally resolved spectra in the weaker and more confused blue region. Some of the features in this region have never been satisfactorily identified. The spectrographs used in both these investigations were designed by Aden B. Meinel. The smaller one that he took to Greenland had a 13-cm-diameter Schmidt camera. The larger one, which was mounted in an observing shack on the roof of the Yerkes Observatory, had an F/0.8 flat-field Schmidt camera, 23 cm in diameter, with a two-piece mosaic grating 20 by 20 cm. There was no spectroscopic equipment for airglow work anywhere that could match the larger of Meinel's instruments except a "sister" Meinel spectrograph that was used on aurora by Alister Vallance Jones in Saskatoon. This Yerkes spectrograph was also used to obtain the profiles of H-alpha that are described below.

Theory of Hydrogen-Line Profiles in Auroral Spectra. In 1954 and 1957 Chamberlain wrote a series of three papers in which the profiles (intensity versus wavelength) of hydrogen lines were first computed for incident beams of bombarding protons. The broadened profiles, with a blueward Doppler shift indicating downward motion, had been discovered earlier by Meinel. The problem was to use the profiles seen in the magnetic zenith, the profile from the magnetic horizon, and the distribution of auroral luminosity with height to extract a self-consistent description of the energies and angular distribution of the primary auroral protons. The principal result was that the bombarding protons had to have a *wide* dispersion in their initial velocities. This was, at the time, a new concept, and it was extracted from the ground-based spectra about a year before spacecraft measurements found the Van Allen radiation with an energy dispersion. It was natural that he should have undertaken this study, because his thesis work on gaseous nebulae had considered the effect of electron collisions in producing radiation from hydrogen atoms. It was a straightforward extension to substitute collisions of H^+ and H with air molecules for collisions with free electrons.

Theory of Na "D"-Line Airglow Variations. In a series of four papers in the *Journal of Atmospheric and Terrestrial Physics* (1956, 1958) with various collaborators, he first considered the importance of multiple scattering on the twilight, night, and day airglow in the resonance transition of Na. The importance of self-absorption in the Na layer had been noted by Donahue and by Hunten. Like most astronomers with a theoretical background, Chamberlain was familiar with the theory of radiative transfer as developed by Chandrasekhar and, therefore, applied his formalism to the airglow problem.

Polarization of Resonance Radiation. In 1959 and later he published a number of analyses of polarization measurements that were unique to the geophysical literature. This work had its origins in writing his book, "PAA" (*Physics of the Aurora and Airglow* [1961]), in which he was attempting to give the theory for an observed polarization of the Na "D" emission in twilight. The standard reference was the text by Mitchell and Zemansky, which was based on Van Vleck's treatment with the old quantum theory, which can give incorrect results. Instead, he used the Weisskopf theory, which is based on Dirac's theory of radiation. He applied the theory, which gives not only polarizations but the anisotropy of the scattering phase function, to Na "D," H Ly-alpha scattering in the night sky (with J. C. Brandt), the forbidden oxygen red lines excited by electron impact, and also applied the theory to comets.

Energy Deposition by Bombarding Auroral Electrons. In 1961 in "PAA" he published the first theoretical analysis of the height distribution of energy deposited in the atmosphere by primary auroral electrons (with proper allowance for electron scattering). An electron-induced aurora is more complicated to handle than one produced by relatively heavy protons, because the electrons are readily deflected as well as slowed at each collision. The analysis indicated also the fraction of the incident electrons that would be reflected by the atmosphere and produce auroral emission in the opposite hemisphere, which was a new concept at the time. The basic equations, which are somewhat like radiative transfer equations with the added complication that the velocity of the particle varies, had been developed earlier by Harold Lewis, who was concerned with the bombardment of tissues by beta-decay electrons.

Plasma-Instability Theory of Aurora. In 1963 he published the first quantitative theory of auroral bombardment that was explicitly based on a particular plasma instability. By this time it seemed to him that the bombardment mechanism was to be found in the instabilities that were constantly plaguing plasma physicists working on the confinement of plasmas by magnetic fields. He simply watched the plasma physics literature until a likely candidate showed up—an instability developed by N. A. Krall and M. N. Rosenbluth. It was thought by them not to be important in the laboratory but seemed to him to hold promise on the geophysical scale. The key feature is an induction electric field parallel to the external magnetic field.

Solar Breeze. In two papers in 1960-1961 he challenged Eugene Parker's theoretical analysis of the solar wind, maintaining that the solutions he obtained from the basic equations were specious and that the only physical solutions were those that had decreasing velocity approaching zero at large distances. A considerable controversy raged for a few years, with neither of them allowing that the other's solution was physical. Part of the difficulty was emotional. However, another part was that in addition to the equations of motion and continuity, Parker handled the temperature with a polytropic relationship, which always yields a solar-wind (supersonic) solution at some distance (and beyond), whereas Chamberlain's second paper for the first time treated temperature with the equation of heat conduction. (Chapman had earlier used the heat conduction equation but only for a static corona.) The resolution was simply that either solution could be correct (see below). Parker's use of a temperature profile resembling the observed one had led him to the solution that is actually valid for the sun. Chamberlain's interest in this topic was due to the implications of Parker's results to the escape of planetary atmospheres.

He found that Parker's solution, if taken literally and applied to the Earth's upper atmosphere, would have produced a rate of escape of the terrestrial atmospheric gases far exceeding what was predicted by kinetic theory. Hence, he began a study of the hydrodynamic equations of motion and obtained solutions contrary to those of Parker. Much of Parker's theoretical analysis, as well as his own, were undoubtedly inspired largely by their dialogue, so that the net result was a genuine benefit to the subject. (Chamberlain's last contribution on this subject was a note in the *Astrophysical Journal* in January 1965, followed by a disagreeing note by Parker; but a subsequent paper by Parker in the same year shows that the physical conditions required for a slow "solar breeze" solution are the same as Chamberlain stated in his January 1965 note.)

Exospheres and Escape of Planetary Atmospheres. In 1963 he published a thorough exposition on the density distribution in a planetary exosphere. Elaboration of this major paper was to occupy him on and off for 15 years. The 1963 paper was a fairly complete discussion of an exosphere where atomic collisions could be ignored, except for the special case of satellite orbits. The theory had been treated most notably by Sir James Jeans, and recent contributions by Francis Johnson and Öpik and their collaborators set the scene for a treatment of densities and the velocity distribution by Liouville's theorem. Later papers in the late 1960s dealt with the correction to Jeans's escape rate due to departures from the truncated Maxwellian distribution usually assumed; in the 1970s with the radial distribution of velocities in the exosphere (H-line profiles); and with charge-exchange collisions between H^+ and H. The latter problem became important when it was realized that the background H^+ plasma was much hotter than the neutral gas. The solution was accomplished by extending the Liouville equation to a Boltzmann equation. The paper resolved a current dilemma in the escape rate of hydrogen as inferred from exospheric measurements and middle atmosphere diffusion results for hydrogen-containing compounds.

Radiative Transfer in Planetary Atmospheres. In 1956 he first showed that the variation in the strength of absorption lines in an optically thick, homogeneous planetary atmosphere increased from the crescent phase toward full phase, which is opposite to the trend for optically thin atmospheres, such as Mars's and Earth's. Observations by Kuiper analyzed in the same paper showed the absorptions by CO_2 in the Venus atmosphere to behave in just this fashion. (A footnote in that paper makes it clear that the observations were Kuiper's, the analysis Chamberlain's.) The only thing of significance to have appeared in this subject earlier was the demonstration by van de Hulst that weak absorptions in such an atmosphere would be proportional to the square root of the absorption coefficient, rather than to the first power. Nine years later Chamberlain wrote a much more thorough analysis of the Venus atmosphere, based on the earlier spectroscopic interpretation. Very soon after this, the field became alive. In 1970 he published a rather complete analytical study of spectroscopy of an optically thick, isotropically scattering atmosphere; later he extended the work to anisotropic scattering and a student (J. P. Lestrade) examined continuum scattering to identify properties of the absorber in the Venus clouds.

Influence of the Stratosphere on Climate. In 1977 he made the first proposal for a physical mechanism that could connect variations in the sun's magnetic field to terrestrial climate. On the one hand there had been observational evidence linking sunspot activity and cosmic ray bombardment to historic climatic changes. On the other hand, studies of the balance of ozone in the stratosphere due to the presence of various catalytic chemicals suggested that cosmic-ray bombardment (and hence the strength of either the geomagnetic or heliomagnetic field) could affect the concentration of one or more of these ozone-destroying catalysts. Hence, the proposed chain of events was (1) sunspot cycle governs cosmic-ray deposition in the stratosphere; (2) cosmic rays produce atomic N through ionizing and dissociating collisions; (3) N reacts to form NO and NO_2, which catalytically destroy O_3; (4) a change in O_3 affects the temperature of the surface by altering the greenhouse insulation affect; (5) the altered stratospheric temperature affects the stratospheric abundance of water vapor, which tends to be nearly saturated at the base of the stratosphere; and (6) the altered water vapor further changes the greenhouse effect in the same direction as the altered ozone abundance.

Joseph Chamberlain's colleagues and friends hold him in great respect and admiration for his creativity, integrity, and leadership.

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