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WHAT IS A SPACE SCIENTIST? AN AUTOBIOGRAPHICAL EXAMPLE

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INTRODUCTION

Space science is not a professional discipline in the usual sense of that term as exemplified by the traditional terms astronomy, geology, physics, chemistry, and biology. Rather, it is a loosely defined mixture of all of these fields plus an exotic and expensive operational style. The distinctive features of space science are the use of rocket vehicles for propelling scientific equipment through and beyond the appreciable atmosphere of the Earth; the rigorous mechanical, electrical, and thermal requirements of such equipment; and (usually) the remote control of the equipment and the radio transmission of data from distant points in space to an investigator at a ground laboratory. Space science is primarily observational and interpretative; it is directed toward the investigation of natural conditions and natural phenomena. But it can be and sometimes is experimental in the sense that artificial conditions are created and the consequences observed. Most space science has been and will continue to be conducted by unmanned, automated, commandable spacecraft. But some is conducted by human flight crews performing direct hands-on manipulation of equipment. The latter mode of operation is of dubious efficacy and, in any case, will probably be the technique of choice only in specialized subfields involving preliminary laboratory-type experiments under free-fall or low-g conditions.

The personal and professional backgrounds of space scientists are diverse, as is commonly the case in new and interdisciplinary fields. In accepting the invitation of Editor Wetherill of the *Annual Review of Earth*

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and Planetary Sciences to write an autobiographical account of my career as a space scientist, I did so with a full realization of the diversity and individualism of those who belong to the fraternal order of space scientists. My account is a personal one and does not include references to primary sources, as would a proper scholarly paper. Some of this account is abridged from my monograph *Origins of Magnetospheric Physics* (Smithsonian Institution Press, 1983), but most of it is not.

PARENTAGE, BOYHOOD, AND EARLY EDUCATION

I was born to Alfred Morris and Alma Olney Van Allen on the 7th of September 1914, the second of their four sons, in Mount Pleasant (population then about 3000), Iowa, the county seat of Henry County. My mother grew up near Eddyville, Iowa, on a small farm that her father had inherited from his father, who had moved from Ohio to Iowa in the mid-1840s. My paternal grandfather, George Clinton Van Allen, was one of 11 children of Cornelius and Lory Ann Van Allen, the former a shipbuilder in Pillar Point, New York, at the eastern end of Lake Ontario. He attended Wesleyan University in Connecticut for two years and later studied law and became proficient in land titles and surveying. He passed through Mount Pleasant in 1862 as a member of the survey party that was laying out the route of the Burlington and Missouri River Railroad, which later became part of the Chicago, Burlington and Quincy (now the Burlington Northern) Railroad. (Several of my prized possessions are the magnetic compass and the drafting instruments that he used.) In late 1862, with his wife of five years, Jennie, he settled in Mount Pleasant, built a small house, and established a law office. My father, an only child, was born in Mount Pleasant in 1869. He attended the local public schools and Iowa Wesleyan College, and then studied law at the University of Iowa in Iowa City, receiving an LLB degree in 1892. He joined his father as a practicing lawyer and continued to practice law for the remainder of his life.

My boyhood activities were centered within our closely knit family, which had a strong resemblance to earlier pioneer families.

The virtues of frugality, hard work, and devotion to education were enforced rigorously and on a daily basis, especially by my father. My mother exemplified the pioneer qualities of affection and nurture for her husband and their children and of comprehensive self-reliance: cooking all meals from scratch, baking delicious bread twice a week, washing clothes with a washboard and tub, maintaining a meticulous standard of household cleanliness, canning large quantities of fruits and vegetables, and, most important of all, ministering to her children through health and frequent sickness during the many epidemics of those days. Before her marriage, she had taught in one-room country schools near Eddyville and had attended the Iowa Wesleyan Academy for two years.

My first clear recollection is waving a tremulous farewell to her as I set off on foot to kindergarten a few days after my fourth birthday. Two months later, my older brother and I went with my father to the public square in Mount Pleasant to witness the celebration of the armistice of World War I by a horde of raucous and exuberant people of all ages. The culmination of this celebration was the burning of a huge straw-filled effigy of Kaiser Wilhelm.

I enjoyed school work greatly under the guidance of devoted teachers, most of whom were unmarried woman who had gone into teaching as a durable profession. Our father read to my brothers and me for about an hour after supper nearly every evening—from the *Book of Knowledge*, *An Illustrated History of the Civil War*, the *National Geographic* magazine, and, occasionally, from the *Atlantic Monthly*. Then he shooed us off to our respective corners to do our homework for two or three hours. Our chores varied with the seasons. We raised a large flock of chickens year-round. In the summer we planted and cultivated a one-acre vegetable garden and a large apple orchard, and in the winter we split wood for the cook stove, shoveled snow, ran errands, fired the furnace, and tried to keep warm. We had a car but seldom used it, even during the summer. During the winter, the car was set up on wooden blocks in the barn to "save the tires." For the most part, we walked everywhere.

I was intensely interested in mechanical and electrical devices. *Popular Mechanics* and *Popular Science* were my favorite magazines. I built elementary electrical motors, primitive (crystal) radios, and other devices described therein. Two highlights were the construction of a Tesla coil that produced, to my mother's horror, foot-long electrical discharges and caused my hair to stand on end; and the complete disassembly and reassembly of those mysterious "black boxes"—the engine and planetary transmission of an ancient Model T Ford that my older brother and I had bought for $25 (later recovered on resale).

In high school my favorite subjects were mathematics (including solid geometry), Latin, grammar, and manual training (woodworking). As a senior in 1930–31, I had my first course in physics, with many opportunities for laboratory work, a memorable experience. During the same year I edited the senior annual *The Target*. I graduated from Mount Pleasant High School in June 1931 as class valedictorian. My valedictory oration was entitled "Pax Romana—Pax Americana," based on my study of Roman history in school and on my father's tutelage. The thesis of this oration was that America, by virtue of its economic, cultural, and military stength, would dominate world affairs and enforce world peace for a limited period of history but would then lose its influence because of its preoccupation with "bread and the circus games."

COLLEGE AND GRADUATE WORK

Throughout my boyhood, there was never any doubt that my three brothers and I would go to college and have an opportunity to "amount to something." The matter was not subject to discussion. In the autumn of 1931, following in the footsteps of my father, mother, and older brother George, I entered Iowa Wesleyan College in Mount Pleasant. The tuition was $45 per semester, and I lived at home. The academic work was demanding, and I took all the courses offered there in physics, chemistry, and mathematics (four years of each), a summer field course in geology, and the one available course in astronomy (using Moulton's 1933 *Astronomy*, which I still have), the only formal course in astronomy that I ever took. Professor Thomas Poulter in physics and Professor Delbert Wobbe in chemistry were my principal inspirations. Each was the one-man faculty of his respective department. I wavered between choosing physics or chemistry as my major but decided on physics after Poulter offered me a part-time student assistantship. I worked in his high-pressure research laboratory and learned to blow glass, to run a metal-turning lathe and a milling machine, and to braze, silver-solder, and weld. More importantly, I came to have an almost worshipful regard for his mechanical ingenuity, his intuitive use of physics and chemistry as a way of life, and his devotion to experimental research. Poulter was in the process of preparing for his role as chief scientist of the Second Byrd Antarctic Expedition, a part of the Second International Polar Year. Following my freshman year I became a part of those preparations. I helped build a simple seismograph and was entrusted with checking out a field magnetometer on loan from the Department of Terrestrial Magnetism of the Carnegie Institution of Washington (DTM/CIW), one of the most beautiful instruments that I have ever seen. In the autumn of 1932 I used this instrument to make precision measurements of the geomagnetic field at three ad hoc locations in Henry County. The measurements involved also the determination of latitude and longitude by observation of the Sun with the theodolite on the magnetometer. All of this was done by carefully following the third edition of Daniel L. Hazard's *Directions for Magnetic Measurements* (US Department of Commerce, Serial Number 166, 1930). I copied my field notes onto clean forms and mailed them proudly to John A. Fleming, then director of DTM/CIW, as a modest contribution to the world survey that was underway. I received a prompt acknowledgment from him that concluded by making it clear that only raw field notes could be accepted as valid. I then sent him those, thereby learning a durable lesson about the sanctity of raw data.

My other introduction to geophysical research was serving as an observer of meteor trails during the Perseid shower of August 1932. Arrangements for the observations were worked out between Poulter and astronomy professor C. C. Wylie of the University of Iowa; sky "reticles" devised and built by Poulter from welding rods were used to carry out the observations. These 6-ft-long conical devices with an eye-ring at the vertex and a coordinate system of radial and circular rods at the other end were mounted on fixed stands. One was located in my backyard in Mount Pleasant and the other in Iowa City, 50 miles to the north. The conical fields of view were positioned so that they included a common volume of the atmosphere spanning the estimated altitude range of meteoric luminosity. During the early morning hours of 22 August, Raymond Crilley manned the Iowa City reticle, and I the Mount Pleasant one, using accurate watches for coordination. Each of us observed about 20 bright meteor trails. Of these, Wylie identified seven as identical cases. He later published the calculated altitudes of the beginning and end points of each of these trails. At the time, I had the impression that this was the first successful attempt to make such measurements, and the impression provided part of the thrill of making them. Later, I learned that my impression was not true. During the ensuing Antarctic expedition Poulter used this system to obtain one of the world's most comprehensive sets of observations of meteor trails. Also, he made extensive use of the DTM/CIW magnetometer and the seismograph that I had helped construct. The 1935 graduation ceremony at Iowa Wesleyan College included a public parade honoring Poulter and Admiral Richard E. Byrd. The latter gave the commencement address. I graduated summa cum laude and was the first student to walk across the platform. Poulter moved forward to congratulate me, but I was so flustered that I scurried past him, clutching my diploma.

During the summer of 1934, I went by automobile to California with my mother, father, and two of my brothers to visit prospective graduate schools in the west. Two of my most pleasurable recollections were visits to the laboratories of Jesse Du Mond at Caltech and Paul Kirkpatrick at Stanford. My eyes popped at the elegance and scope of their laboratories, and I was deeply grateful for the careful explanations of their research that they gave me, a young kid who had dropped in uninvited. But in the end I followed my family's tradition of attending the University of Iowa. In 1935, the faculty of its Department of Physics numbered five: George W. Stewart (head of the department since 1909), John A. Eldridge, Edward

P. T. Tyndall, Claude J. Lapp, and Alexander Ellett. The latest addition occurred in 1928 with Ellett's arrival. My assigned advisor was Tyndall, a warm-hearted and spirited individual with a PhD from Cornell University. My central preoccupation was with introductory graduate-level courses based on Slater and Frank's *Introduction to Theoretical Physics*, Abraham and Becker's *Classical Electricity and Magnetism*, and Pauling and Wilson's *Introduction to Quantum Mechanics*; on instructors' original lectures on classical mechanics, statistics, and partial differential equations; and on lectures and laboratory studies in atomic physics. I found the work to be rigorous and demanding.

I was eager to start research, and soon after my arrival Tyndall introduced me to the art of growing large single crystals of spectroscopically pure zinc and of measuring their physical properties. I completed an MS degree in June 1936 with an original experimental thesis, "A Sensitive Apparatus for Determining Young's Modulus at Small Tensional Strains." By that time Ellett, who formerly worked with atomic beams, was actively converting his research interests to the new field of experimental nuclear physics. I decided to join in this work. Together with Robert Huntoon, a more senior graduate student, and others, I helped build a copy of the famous Cockroft-Walton high-voltage power supply and accelerator. Our capacitors were made of plates of window glass on which we glued aluminum foil; the rectifiers and thc accelerator tube uscd glass cylinders from a local company that supplied them to service stations for the then prevalent model of gasoline pumps. Everything was improvisation. Central elements of the measuring equipment were an ionization chamber and a Dunning-type pulse amplifier with a voltage gain of about one million, built with vacuum tubes of course and a nightmare to shield adequately against pickup of AC ripple and coronal discharges, of which we had a plethora. Because of the absence of air conditioning or any effective humidity control, operation during the summer was impossible. But on a good day in the autumn of 1938, we finally got an ion beam of a few microamperes with an accelerating potential of 400 kV. My objective was to measure the absolute cross section of the reaction

$$H^2 + H^2 \rightarrow H^1 + H^3$$

over as great a range of bombarding energy as possible. The novel feature of my experiment was the use of a gaseous (i.e. infinitesimally thin) target, which involved the controlled flow of deuterium gas through the custom-built reaction chamber. After several months of fixing leaks in the vacuum system, replacing burnt-out filaments in the rectifiers, repairing damage from high-voltage spark-overs, etc, etc, I finally got everything to work at the same time. With the help of a fellow graduate student, I then made a continuous run of 40 hr, being unwilling to turn off anything because of the well-founded expectation that many weeks might be required to restore full operation. However, with good luck, I was able to make a confirmatory run two weeks later. These two runs provided the basis of my PhD dissertation, which, with Ellett's approval, I then wrote up under the title "Absolute Cross-Section for the Nuclear Disintegration $H^2+H^2 \rightarrow H^1+H^3$ and its Dependence on Bombarding Energy" (50–380 keV). I defended my work successfully before the examining committee and received the degree in June 1939.

Following an oral paper that I gave at the spring 1939 American Physical Society meeting, Hans Bethe expressed a keen interest in the results but found that the trend of my curve of cross section vs. bombarding energy was impossible to believe at the lower energies because of basic quantum-mechanical theory. This criticism was unsettling to put it mildly. Ellett and I went over the entire matter critically and eventually realized that my method of measuring the beam current through the reaction chamber was faulty. I had collected the ion beam in a Faraday cup *after* it had passed through the chamber and had measured the charge collected per unit time there. I failed to take account of the partial neutralization of the beam by charge exchange in the target gas, an effect of increasing importance at the lower energies. As a result, the measured current was too low and the calculated cross section was correspondingly too large. A follow-on experiment by Stanley Atkinson, using the same apparatus, established the magnitude of this effect and corrected my results.

Many years later, the cross section of the deuteron-deuteron reaction at much lower energies became a matter of importance in the development of equipment for the current major effort to achieve controlled fusion in the laboratory.

DEPARTMENT OF TERRESTRIAL MAGNETISM OF THE CARNEGIE INSTITUTION OF WASHINGTON

Concurrently with the early nuclear physics work at Iowa, Merle Tuve, Lawrence Hafstad, and Odd Dahl had built a Van de Graaff (electrostatic) power supply and an ion accelerator tube at DTM and had succeeded in getting a stable beam at bombarding energies up to 1 MeV. The principal emphasis of their early work, under the urging of theoretician Gregory Breit, was the careful measurement of the proton-proton scattering cross section, then regarded as one of the most fundamental problems in nuclear physics. Norman Heydenberg, also one of Ellett's former students at Iowa, was one of Tuve's principal collaborators. In the spring of

1939 Ellett recommended me to Tuve, and I received a Carnegie Research Fellowship to work at DTM.

Earlier, in late 1938, Otto Hahn and Fritz Strassman in Germany had discovered nuclear fission. The DTM laboratory was converted to confirmatory experiments, which were successful. More importantly, Richard Roberts discovered the delayed emission of neutrons from fission products. This discovery provided the basis for the control of nuclear fission in all subsequently developed nuclear power plants.

My own work at DTM during 1939–40 was the measurement of the absolute cross section for photodisintegration of the deuteron by 6.2-MeV gamma rays from protons on fluorine. This was done in collaboration with Nicholas Smith, another Carnegie fellow, formerly at the University of Chicago. In addition, Norman Ramsey, yet another Carnegie fellow, and I measured neutron-proton cross sections using a small proportional counter that I had devised for observing the recoil protons.

Of much greater importance to my future career was my crossing of the culture gap at DTM from nuclear physics to the department's traditional research in geomagnetism, cosmic rays, auroral physics, and ionospheric physics. I was impressed especially by the work of Scott Forbush and Harry Vestine. Also, there were occasional visits by Sydney Chapman and Julius Bartels, who were then completing their great two-volume treatise *Geomagnetism*. As a result, my interest in low-energy nuclear physics dwindled, and I resolved to make geomagnetism, cosmic rays, and solar-terrestrial physics my fields of research—at some unidentified future date.

PROXIMITY FUZES

By late 1939 the war in Europe was already several months old, and Tuve foresaw the inevitable involvement of the United States. He abandoned experimental work and turned his remarkable talents to the problem of what scientists in the United States should be doing to help remedy the desperately inadequate quality of our military establishment. He made intensive inquiries, especially among high-ranking naval officers, and returned to DTM with a vivid impression of the ineffectiveness of anti-aircraft guns and with full knowledge of the embryonic British work on proximity fuzes for eliminating the range error of time-fuzed projectiles. He seized on this as *the* matter to which he would devote his own staff and, by recruitment, other physicists and engineers of kindred inclination—including Ellett from Iowa and Charles Lauritsen, his son Thomas, and William Fowler from Caltech. As a Carnegie fellow, I was apart from these early efforts, but by the summer of 1940 I asked to become a part of this enterprise and was appointed to a staff position in Section T (for Tuve) of the National Defense Research Council (NDRC) of the newly created Office of Scientific Research and Development, headed by Vannevar Bush.

I worked first on a photoelectric proximity fuze and succeeded in solving the basic problem of making a circuit such that the fuze would have equal sensitivity over a large range of ambient light levels. My circuit gave an output approximately proportional to the logarithm of the current from a photoelectric cell by using a fundamental characteristic of a vacuum-tube diode. My demonstration of a breadboard of this circuit to Charlie Lauritsen and Willy Fowler showed that I got the same size pulse by waving my hand in front of a photocell when illuminated by full sunlight as I got in a darkened room. Their exuberant response not only made my day, but it also propelled the photoelectric fuze into the realm of serious consideration.

But soon thereafter, I was transferred to work on the radio proximity fuze. Dick Roberts had built a simple self-excited rf oscillator operating at about 70 MHz after the fashion of the one that the British called an autodyne circuit. In brief, the plate current of the one-tube oscillator with a short antenna was affected by the reflected signal from a nearby conductor. The basic scheme was that the transient pulse as a fuze passed an aircraft could be amplified so as to trigger a gaseous tube (thyratron) to fire the detonator of the projectile. This device became the focus of a truly huge development.

For the first time in my life I worked under conditions in which urgency was the motto, multiple approaches to a problem were fostered, money was no object, and the first approximation to a solution was the prime objective. As Tuve put it, " I don't want you to waste your time saving money."

THE APPLIED PHYSICS LABORATORY OF JOHNS HOPKINS UNIVERSITY

The radio proximity fuze group soon outgrew the capacity of DTM, and thus Tuve negotiated an arrangement with Johns Hopkins University (JHU) such that JHU would assume contractual oversight of the project. In early 1942 JHU rented a large Chevrolet garage in Silver Spring, Maryland, and established the Applied Physics Laboratory (APL). Along with other members of the group, I was transferred to APL/JHU in April 1942, thereby qualifying as a plank-owner, as that term is used in the navy for a member of the crew who places a new ship on commission.

My own work was principally on developing what was termed a rugged vacuum tube, i.e. one that would survive acceleration of some 20,000 g as it was propelled through the barrel of a 5″/38 navy gun. The starting point was the miniature vacuum tubes that had been developed for use in electronic hearing aids by the Raytheon and Sylvania companies. I worked principally with tube engineer Ross Wood of Raytheon in the trial-and-error process of remedying the numerous shortcomings of the early tubes. I conducted field tests of each batch of tubes by putting them in a small cylinder that was mounted in a projectile. These projectiles were then fired vertically by a converted 10-pounder gun at a test site in southern Maryland along the Potomac River. We recovered the projectiles with a posthole digger and returned the tubes to the laboratory for detailed scrutiny. (In July 1942, I was commissioned a deputy sheriff of Montgomery County in order to legally carry a loaded revolver for coping with hypothetical hijackers on our daily expeditions to and from the test site.) I would then report the results to Ross by phone or, if we had important conclusions, by personal visit by train to Newton, Massachusetts, where he operated a pilot line. On most of these trips I would return to Silver Spring with a batch of improved tubes. One of the most nagging problems was the breakage of the fine filaments. I reasoned that distortion of the structure that supported the filaments was the cause of the failure. In a moment of inspiration I sketched out a scheme for a minute coil spring (wrapped around a mandrel), to the free end of which one end of the filament would be welded. My hope was that the spring would maintain nearly constant tension on the filament during acceleration in the barrel of the gun, and also that the tension would be such as to tune microphonics outside of the frequency pass-band of the amplifier. Wood executed this idea using the skills of the women who built these tubes with the aid of microscopes. The scheme worked and became an essential feature of the millions of tubes that were manufactured during the three subsequent years of World War II.

By late autumn 1942, the first of the Section T radio proximity fuzes were coming off the production line. Realistic and extensive testing at the Dahlgren Proving Ground over the Potomac River ("airbursts" as the projectile approached the water) and past an aircraft suspended between two towers at Jack Workman's test facility near Socorro, New Mexico, had been conducted. Despite numerous duds and premature bursts, it was estimated that the effectiveness of naval antiaircraft fire would be increased by a factor of order five if the proximity fuzes were substituted for the time fuzes then in use throughout the fleet.

In early November 1942, the Naval Bureau of Ordnance (Bu Ord) determined that the fuzes were ready for issue to the Pacific Fleet. Neil Dilley, Robert Peterson, and I were given spot commissions as United States Naval Reserve line officers with the rank of lieutenant junior grade.

Our job was to assist Commander William S. ("Deke") Parsons, United States Navy (USN), principal liason officer from Bu Ord during the development work, in introducing this new fuze to gunnery officers of combatant ships in the South Pacific. Parsons (later the weaponier on the *Enola Gay*, which dropped the first atomic bomb at Hiroshima) flew ahead to an unrevealed location in the Pacific theater. Dilley, Peterson, and I oversaw the loading of the first secret issue of some 5000 carefully counted, proximity-fuzed (also called VT fuzes to disguise their nature) 5″/38 projectiles into the hold of a troop ship at Mare Island near San Francisco. Within a week of receiving our commissions, signed personally by Frank Knox, Secretary of the Navy, we were at sea en route to a secret destination. The ship traveled without escort. I was able to keep track of our progress in latitude by elementary celestial observations and in longitude by the progressive change in mean time between sunrise and sunset and the occasional one-hour changes in ship's time.

About two weeks later we arrived in Nouméa, New Caledonia, headquarters of the Commander of the South Pacific Fleet (COMSOPAC). Parsons had already laid the groundwork and assigned us to various segments of the fleet. I was assigned as assistant gunnery officer on the staff of Rear Admiral Willis A. Lee, a task group commander of Task Force 38 (commanded by Admiral William F. Halsey) and Task Force 58 (commanded by Admiral Raymond A. Spruance). Admiral Lee was also type commander of battleships in the Pacific Fleet (COMBATPAC) with headquarters on the USS *Washington*. I arrived on the *Washington* only about two weeks after her celebrated role in the major engagement with a Japanese task force in the strait between Tulagi and Guadalcanal, thereafter called iron-bottom bay. Lee was the informal president of the Navy "gun club" and was acknowledged to be one of the leading gunnery officers of the US Navy. He was thoroughly familiar, both theoretically and practically, with the fundamental ineffectiveness of antiaircraft weapons and of the often fatal fallacy of supposing that an attacking aircraft could be stopped by "filling the air with shrapnel." He was deeply impressed by my briefings on the VT fuzes and immediately recognized their potential in quantitative terms. I gave him a clear statement on the necessity of a clear field of fire (not over our own ships), of the expectation of at least 15% duds and premature bursts (which posed no hazard to the firing ship), and of the airbursts that occurred as projectiles approached the sea at the end of flight. Also, I informed him of the then prevailing doctrine that despite the potential effectiveness of proximity-fuzed projectiles for shore bombardment, such usage was forbidden on the security ground that duds might be recovered by the enemy and either duplicated by them or used as a basis for countermeasures, i.e. "jamming" by radio transmitters so as to cause premature bursts. He endorsed my written description of the properties of the new ammunition and immediately ordered a pro rata distribution of the available supply to all combatant ships of his task group. My job was to effect this distribution and to brief gunnery officers and commanding officers on their proper use. I encountered a wide range of understanding and lack of understanding of the range-error problem and varying degrees of acceptance. Thc toughest operational problem was the restriction on firing over other ships of the task group under the complex conditions of actual air attack.

After eight months of sea duty on the *Washington* and other ships I was ordered back to Bu Ord to serve as liasion officer with APL/JHU and to read and summarize combat reports from ships using the VT fuze against attacking aircraft. Finding such desk work onerous, I requested transfer back to the Pacific Fleet to help remedy the grave shortcomings of the fuzes—most notably, the large percentage of duds that were occurring as the useful shelf life of their batteries expired during their long period of transport, usually at elevated temperature conditions, by cargo ships from the states to combatant ships. I then made contact again with Admiral Lee on the *Washington* and with Commander Lloyd Muston, COMSOPAC staff gunnery officer, in Nouméa and engaged in setting up rebattering stations at ammunition depots at Nouméa, Espiritu Santo, Tulagi, Guadalcanal, and Manus Island, and on ammunition barges at Eniwetok Atoll, Kwajalein, and Ulithi. I also had temporary duty on a succession of destroyers to instruct gunnery officers and conduct tests of the fuzes. And I made frequent reports to Bu Ord on the status of the work and (usually urgent) requests for fresh batteries, tools, and equipment—by air transport, if possible, to try to maintain the fleet's supply of workable fuzes. During this period I was on the *Washington* as assistant staff gunnery officer during the Battle of the Philippines Sea, in which the ship successfully defended herself against kamikaze attack. In March 1945 I returned to duty at Bu Ord and as liaison officer at APL/JHU until my transfer to the inactive reserve as a lieutenant commander in March 1946, after the end of World War II hostilities.

The period 1940–45 was a part of my life totally foreign to my previous aspiration to become an academic physicist. But I lost no energy grieving over the turn of events. On the contrary, I plunged into "the war effort" with the patriotic fervor of those days and with the exhilaration of applying my knowledge of physics and mathematics and my laboratory skills to solving difficult problems of practical importance and national urgency. My service as a naval officer was, far and away, the most broadening experience of my lifetime. I had considerable responsibility in the real world of life-or-death, and for the first time, I dealt with a vertical cross section of the human race on a one-to-one basis, from apprentice seamen to admirals. I was deeply impressed by every such relationship, by the code of honor of the navy, and by the validity of military protocol. I gained a profound respect for the raw power and grandeur of the sea and a corresponding respect for seamen. Much of my boyhood reading was in that vein. As a high-school senior, I had hoped for an appointment to the US Naval Academy, and our US congressman, a close friend and former classmate of my father's in college and law school, nominated me subject to passing the academic and physical examinations. But I failed the latter. Eleven years later I received a spot commission as a lieutenant junior grade in the Naval Reserve under the relaxed wartime standards.

Among other things that I learned in the navy by close observation of my peers and superiors was how to make a sound decision when the basis for a decision was diffuse, inadequate, and bewildering. This lesson has served me well. Another strong and durable impression was the great gap between the life of a bureaucrat in Washington and the real situation on a combatant ship.

HIGH-ALTITUDE RESEARCH

While still on terminal leave from the navy, I was rehired as a physicist at APL and encouraged to organize a research group to engage in high-altitude research based on the prospectively available opportunity to conduct experiments with captured and refurbished German V-2 rockets. I had earned my spurs by my wartime work, and Tuve gave me a free hand and ample financing to develop this field as I saw fit. My interest in geophysics stemmed from Tom Poulter's work and from my association with the " old-line" geophysicists at DTM. This line of scientific interest and my laboratory experience in nuclear physics and with rugged electronic devices and high-performance ordnance combined to lay the groundwork for my future research career. I was eager to attack problems of the primary cosmic radiation, the ionosphere, and geomagnetism by rocket techniques, which promised direct observation of many phenomena that had been previously a matter of conjecture, albeit sophisticated conjecture.

I gathered together a spirited group of like-minded individuals—Robert Peterson, Lorence Fraser, Howard Tatel, Clyde Holliday, John Hopfield, and several others—and got to work. Parallel efforts were underway at several other military or quasi-military laboratories. Of these, the group at the Naval Research Laboratory (NRL), inspired by Ed Hulburt, their long-time leader in atmospheric and ionospheric physics, was the most noteworthy. We adopted NRL as our principal competitor and sometime collaborator.

The opportunity to use V-2's for scientific work was provided by the Army Ordnance Department by virtue of the foresight and broad vision of Colonel Holger N. Toftoy.

Under the leadership of Ernst Krause of NRL, a small and highly informal group of prospective participants in this effort was assembled to maximize the scientific work and to allocate flight opportunities in an equitable manner. I was a member of this group, which called itself the V-2 Rocket Panel. We had no formal organization, no official authority, and no budget. Nonetheless, we oversaw, in effect, the entire national effort in this field for over a decade. Krause was the original chairman, but he left the NRL in 1946 and I was chosen to succeed him, continuing thereafter as chairman until the effective termination of our functions in 1958.

In 1946, with the support of Merle Tuve, I initiated and supervised the development of a high-performance American sounding rocket, the Aerobee, to be used exclusively for scientific purposes. This rocket soon joined the V-2 as a basic vehicle for high-altitude research. During the period 1946–51, payloads of scientific instruments were carried by 48 V-2's and 30 Aerobees.

The emphasis of our APL work was in the fields of cosmic rays, solar ultraviolet, high-altitude photography, atmospheric ozone, and ionospheric current systems. The site for most of the launchings was the White Sands Proving Ground (later renamed White Sands Missile Range) near Las Cruces, New Mexico. But in 1949 and 1950, I organized successful Aerobee-firing expeditions on the USS *Norton Sound* to the equatorial Pacific and the Gulf of Alaska, respectively. (As of 17 January 1985, a total of 1037 Aerobees had been fired for a wide variety of investigations in atmospheric physics, cosmic rays, geomagnetism, astronomy, and other fields.)

The national effort in high-altitude research during those early free-wheeling and spirited days was characterized by many failures and many noteworthy successes. Substantial advances in knowledge were achieved in atmospheric structure, ionospheric physics, cosmic rays, high-altitude photography of large areas of the cloud cover and surface of the Earth, geomagnetism, and the ultraviolet and X-ray spectra of the Sun.

The V-2 Rocket Panel [later renamed the Upper Atmosphere Rocket Research Panel (UARRP) and, still later, the Rocket and Satellite Research Panel] presided over the entire effort. Beginning in the mid-1950s, the panel spawned one of the important components of our national participation in the 1957–58 International Geophysical Year (IGY). Its members became influential in the planning of the IGY, actively promoted the adoption of scientific satellites of the Earth as an element of the IGY program, and laid the foundations for the scientific program of the

National Aeronautics and Space Administration, a major agency of the federal government created in 1958.

RETURN TO THE UNIVERSITY OF IOWA

In 1950, and despite the flourishing of our high-altitude work, the new director of APL, R. C. Gibson, split my assignment so as to include supervision of the residual proximity fuze group. I was competent to provide such supervision but had no interest in pursuing further developmental work on fuzes. I did the job but interpreted the split assignment as foreshadowing the termination of academic-style research in geophysics at APL. A few months later I received a telephone call from Professor Tyndall, my former research mentor at the Universtity of Iowa. He informed me that Louis A. Turner had resigned as head of the Department of Physics after four years and that he (Tyndall) had suggested me as a possible successor. I was thrilled by this prospect and soon thereafter made a short visit to Iowa City for interviews and a departmental colloquium. Several weeks dragged on after I returned to Silver Spring, with no news. I finally received a letter from Tyndall advising me that they had offered the position to the individual who was their first choice and were awaiting his response. Another few weeks of suspense came to an end when Tyndall called to offer me the position, which would also carry the rank of full professor. At that time Abigail, my wife of five years, had been west of the Mississippi only once and considered Iowa to be terra incognito from the cultural point of view. Nonetheless, she agreed to support my decision whatever it might be. I then accepted the offer but told Tyndall that I would need six months to wind up my obligations at APL. This was agreed.

On a very cold first of January 1951, my wife and I with our two young daughters arrived in Iowa City in our old station wagon, pulling an even older trailer containing most of our earthly possessions. We plowed through the snow to move into a "barracks apartment," one of a cluster of small metal-sheathed buildings that had been erected during the war as temporary quarters for naval cadets and other personnel associated with the university. The sole source of heat was a cast-iron stove that was fed fuel oil from an external 55-gal drum by gravity flow through a small copper tube. The small living room could be made comfortably warm, but the remainder of the apartment presented a challenging problem in heat transfer. However, the monthly rent was only $35.

I entered my new duties with enthusiasm and dedication. I had no research budget, but the department had an excellent machine shop and two skilled instrument makers, as well as a large stock of more-or-less obsolete but still usable electrical instruments.

With the help of George W. Stewart I got a small but very important grant from the private Research Corporation as seed money and started research on cosmic rays using balloon-borne equipment; I also recruited several able graduate students as collaborators. Soon thereafter, I wrote a proposal to the US Office of Naval Research (ONR) for measuring the primary cosmic-ray intensity at high latitudes above the appreciable atmospherc, using small military-surplus rockets carried to an altitude of about 50,000 ft by a balloon and launched from that starting point to reach a summit altitude of some 250,000 ft. By this inexpensive technique, I hoped to resume high-altitude research on a low budget. Thc proposal was accepted. Support by the ONR has continued without a break for the subsequent 38 years and has provided the base for all of my research during this period.

In the summer of 1952, two of my students, Leslie Meredith and Gary Strein, our lab technician Lee Blodgett, and I made our first rockoon (rocket-balloon combination) expedition to measure the cosmic-ray intensity above the atmosphere in the Arctic. We traveled on the Coast Guard icebreaker USCGC *Eastwind*, whose primary mission was the resupply of the weather station at Alert at the shore of the Arctic Ocean on northeastern Ellesmere Island. We released balloon-borne Deacon rockets from the heliocopter deck whenever we could persuade the captain to steam downwind for an hour while we inflated and released the balloon under zero relative wind conditions. After several failures we diagnosed and cured the problem and got a succession of successful flights to altitudes of about 200,000 ft at locations off the coast of Greenland—the first research rocket flights ever made at such high geomagnetic latitudes. All of the instrumentation, including the telemetry transmitters and nose cones, were built in our own shop; a single Geiger-Mueller (G-M) tube was the radiation sensor.

We reported our results with pride to the UARRP and set to work to refine the instrumentation using ionization chambers and scintillation counters as well as G-M tubes.

During succeeding summers, Arctic rockoon expeditions on various ships were the heart of our work. These were led by Melvin Gottlieb and Frank McDonald. The 1953 expedition yielded a remarkable new finding—namely, the first direct detection of the electrons that, we surmised, were the primaries for producing auroral luminosity.

For a 15-month period in 1953–54, my family and I took a leave from the University of Iowa to join Lyman Spitzer at Princeton University in an experimental program to investigate the confinement of hot plasma by a magnetic field in a twisted figure-eight-shaped tube that he called a stellerator—so named with the hopeful prospect of providing a demonstration of controlled thermonuclear fusion in deuterium and cventually in a mixture of deuterium and tritium. All of this recalled my PhD thesis. I built and operated a crude model of such a machine, called the Model B-1 stellerator. (Model A was a previously built smaller device of table-top size.) I demonstrated the validity of Spitzer's rotational transform of magnetic fields in the twisted toroid with a miniature electron gun and fluorescent screen and got plasma confinement times of a few milliseconds in a hydrogen plasma. The difficulty of making a much larger machine of this nature and, as it appeared to me, the remote prospect for achieving self-sustained fusion on a reasonable time scale convinced me to return to Iowa and resume my high-altitude research, which was already yielding significant original results. This I did in August 1954.

Plans for the International Geophysical Year were by then being formulated, and my colleagues and I on the UARRP were eager to add investigations with rocket-borne equipment to these plans. I proposed to continue the rockoon program with further auroral, cosmic-ray, and magnetic-field measurements in the Arctic, equatorial latitudes, and the Antarctic. This program was funded by the National Science Foundation within its special IGY program. Our field work culminated in 1957 with two shipboard expeditions that I led. The first was aboard the USS *Plymouth Rock* from Norfolk to northwestern Greenland; the second, aboard the large icebreaker USS *Glacier* from Boston via the Panama Canal to the Central Pacific and thence to Antarctica. Of the 30 rockoon flights that Laurence Cahill and I attempted during these two expeditions, which ranged from 79°N to 75°S latitude within a four-month period, 20 were successful in yielding high-altitude cosmic-ray, auroral particle, and magnetic-field data.

EARLY SATELLITE WORK

In 1956 I made a formal proposal to the IGY directorate for a simple but globally comprehensive cosmic-ray investigation using one of the early US Earth satellites as a powerful follow-on to my previous and planned work with rockets. In addition, I proposed study of the auroral primary radiation, also on a global basis, if and when orbits of sufficiently high inclination were available.

The cosmic-ray proposal was given a favorable rating, placed in the pool of experiments to be conducted by one of the early IGY satellites, and funded by the National Science Foundation. The development of the instrument was in the capable hands of George H. Ludwig, a former Air Force pilot and a graduate student at Iowa. He introduced many novel features, including the use of then new transistors throughout the electronics and the design and construction of a miniature magnetic tape recorder.

Following the Soviet's successful flights of the first Earth satellite *Sputnik I* and then *Sputnik II*, the Army's rocket vehicle Jupiter C was adopted as a US alternative to the planned but faltering Vanguard vehicle for placing an early US payload into Earth orbit. By virtue of preparedness and good fortune, the Iowa cosmic-ray instrument was selected as the principal element of the payload of the first flight of a four-stage Jupiter C, launched on 31 January 1958 (1 February GMT).

Both the vehicle and our instrument worked. The data from the single Geiger-Mueller tube on *Explorer I* (as the payload was called) yielded the discovery of the radiation belt of the Earth—a huge region of space populated by energetic charged particles (principally electrons and protons), trapped within the external geomagnetic field. The attempted launch of *Explorer II* was a vehicular failure, but the launch of *Explorer III* in 26 March 1958, with an augmented version of the Iowa instrument, was successful. The *Explorer III* data provided massive confirmation of our earlier discovery and clarified many features of the earlier body of data.

Soon thereafter we were invited to provide radiation-detecting instruments for two satellites that were to observe the effects of several nuclear bombs to be detonated after delivery to high altitudes by rockets. On a time scale of less than three months, Carl McIlwain, George Ludwig, and I designed and built the radiation packages for these satellites—using much smaller and more discriminating detectors, chosen for the first time with knowledge of the existence of the natural radiation belts and the enormous intensity of charged particles therein.

Explorer IV was launched successfully on 26 July 1958. Our apparatus operated as planned and provided the principal body of observations of the artificial radiation belts that were produced by the three high-altitude nuclear bursts—called Argus I, II, and III. The back-up launch of our apparatus on *Explorer V* was a vehicular failure. Analysis of our *Explorer IV* data on the natural radiation belt as well as on the artificial radiation belts from the Argus bursts propelled the entire subject to a new level of understanding and broad scientific interest.

The first Soviet confirmation of the existence of natural radiation belts came from *Sputnik III*, launched in May 1958.

Late in 1958, the Iowa group supplied radiation detectors on two missions, *Pioneer I* and *Pioneer III*, that were intended to impact the Moon. The lunar objective was not achieved, but our data established the large-scale structure and radial dimensions of the region containing geomagnetically trapped radiation. Another lunar flight, *Pioneer IV* (also unsuccessful in reaching the Moon), carried our apparatus in early 1959 and provided a valuable body of confirmatory data.

SPACE RESEARCH UNDER THE AUSPICES OF THE NATIONAL AERONAUTICS AND SPACE ADMINISTRATION AND THE OFFICE OF NAVAL RESEARCH

Our early satellite work was done under the auspices of the IGY/National Science Foundation program, the Office of Naval Research, the Army Ballistic Missile Agency, the Jet Propulsion Laboratory, and the Air Force.

The creation of the National Aeronautics and Space Administration (NASA) as a new, civilian agency of the federal government was formalized by President Eisenhower's signature on the enabling legislation on 10 October 1958. As NASA got itself organized, it moved toward becoming the central national agency for the planning and support of space science and applications. Nonethless, a substantial effort in these areas continued under the auspices of the military departments of the Department of Defense, our principal relationship therein being with the Office of Naval Research.

The creation of NASA led to a dramatic change in space research in the United States. Whereas previously it had been performed by only a small cadre of individuals who might well be described as members of the UARRP and their immediate associates, principally in military and quasi-military laboratories, it then assumed national scope and became "civilianized." In anticipation of the NASA legislation, the National Academy of Sciences established the Space Science Board (SSB) in the early summer of 1958 to advise the federal government on the conduct of scientific research in space. This board was chaired during its important and most influential period by Lloyd Berkner. I was an original member of the SSB and served from 1958 to 1970 and again from 1980 to 1983. A major planning study was conducted under my chairmanship during a two-month period in the summer of 1962 at the University of Iowa. This study yielded a classical document and became the prototype for subsequent summer studies by the SSB. Space science in the United States benefited greatly from the close relationship and mutual respect between Berkner and James Wcbb, the second administrator of NASA and an especially effective one during the period of great growth of the agency. Indeed, members of the SSB had the heady, but only partially true, perccption that they were writing the national scientific program in space in the form of well-considered advice from the interested segment of the scientific community.

Space exploration was transformed from being an arcane field with only a handful of participants to an activity of high national visibility. Members of Congress vied for membership on freshly created space committees.

Senator Lyndon B. Johnson and Congressman John W. McCormack led the legislative drive to put the United States in space on a scale adequate to restore national pride and international prestige following what was perceived as national humiliation by the early successes of the Soviet Union, previously thought by most Americans to be a technologically backward nation. The political scene culminated in the hesitant but eventually dramatic decision by President John F. Kennedy to undertake the landing of a man on the Moon and his safe return to the Earth. His formal public announcement of the Apollo project was made on 25 May 1961. The politics of this decision has been discussed voluminously by many, many others, and I have nothing to add.

In parallel with the Apollo project, programs of space science and the numerous practical applications of space technology were also flourishing. These had much less public visibility but in the long run have proven to be of far greater importance and durability.

Our research at Iowa centered on expanding our knowledge of the energetic particle population of the Earths's external magnetic field and the multifold physics therof. In 1959 Thomas Gold suggested the term " magnetosphere" for the region around the Earth in which the geomagnetic field has a controlling influence on the motion of charged particles, and the term "magnetospheric physics" was widely adopted. Magnetosphere joined the already established list of "spheres"—atmosphere, ionosphere, mesosphere, thermosphere, etc—as a geophysical term.

Much earlier, the presence of thermal and quasi-thermal plasma (ionized gas) in the Earth's magnetic field had been established by Owen Storey and others in the interpretation of "whistlers," a low-frequency electromagnetic phenomenon resulting from lightning strokes.

In situ measurements of this plasma became a central objective of magnetospheric physics using Earth satellites. Another central objective was the investigation of the physics of the aurorae, geomagnetic storms, and the ring current of the Earth. Also, space technology opened up new fields of investigation of cosmic rays, energetic particles from the Sun, solar X-ray flares, and the detailed nature of the interplanetary medium.

The Iowa group played an imprtant role in these developments. Louis Frank made a marked advance in our understanding of the plasma physics of the magnetosphere by developing and flying, first on the *NASA/Orbiting Geophysical Observatory II* in 1965, a low-energy proton electron differential energy analyzer (LEPEDEA) that was sensitive to particles having energies as low as 1 keV.

With the support of the Office of Naval Research and later of NASA we developed and built complete satellites with full complements of scientific instrumentation, thus becoming the first university to succeed in this comprehensive undertaking. Indeed, at one point in time, we had built and flown more satellites than the combined number built by all foreign nations, excepting the Soviet Union. The Injun series of Iowa satellites comprised *Injun I* (launched 29 June 1961), *Injun II* (not placed in orbit because of a launch vehicle failure on 24 January 1962), *Injun III* (launched 12 December 1962), *Injun IV* (launched 21 November 1964), and *Injun V* (launched 8 August 1968). These were placed in low-altitude, high-inclination orbits and had investigation of the aurorae as one of their primary objectives. A notable advance was made by Donald Gurnett in devising and successfully flying a VLF (very low frequency) radio receiver on *Injun III*. A large variety of plasma/wave phenomena were observed, and this new field of investigation began to assume an important role in magnetospheric physics.

We also provided, under increasingly competitive circumstances, instruments as part of the scientific payload of NASA spacecraft: *OGO I, II, III*, and *IV*; *Explorers VII, XII*, and *XIV*; and *IMP*'s (Interplanetary Monitoring Platforms) *-D, -E*, and *-F* (*Explorers 33, 35*, and *34*). *IMP-D* was placed into a very eccentric orbit of Earth with apogee beyond the Moon's orbit, and *IMP-E* was injected into a durable orbit around the Moon. Both of these spacecraft were exceedingly fruitful in studying solar X rays and solar energetic particles and in exploring the outer fringes of the magnetosphere—especially the magnetotail, which had been discovered by other groups using *Explorer VI* and studied further by us and others with *Explorer XII* and *Explorer XIV* in very eccentric orbits.

The latest of the Universtiy of Iowa's small satellites was *Hawkeye I*, which was placed in an eccentric, 90° inclination orbit in 1974 and continued to operate properly until its reentry into the atmosphere nearly four years later. It yielded important results on the configuration of the bow shock and magnetopause and on the topology of the geomagnetic field at large radial distances over the northern polar cap and in the vicinity of the polar cusp—a special feature of central importance in the entry of solar plasma into the magnetosphere.

Other Earth satellite missions in which the University of Iowa has had an important role have included *IMP-G, IMP-I, IMP-H, IMP-J, UK-4* (*United Kingdom-4*), S^3 (*Small Standard Satellite*), *DE-1* (*Dynamics Explorer I*), and *ISEE-1, -2*, and *-3* (*International Sun Earth Explorers 1, 2*, and *3*). Frank's imaging camera on *DE-1* has provided a large and classical album of global images of the aurorae and other faint light features of the Earth under lighting conditions previously thought to make such images impossible. One of my great regrets has been that Sydney Chapman did not live to see these, inasmuch as he often expressed such an aspiration when he lectured on auroral physics at the University of Iowa in 1963. Meanwhile, the investigations of Gurnett and his colleagues with their VLF receivers on *ISEE-3* have been exceedingly fruitful, as have the plasma measurements on *ISEE-1* and *-2* by Frank et al. The imaginative scheme of Robert Farquhar at the Goddard Space Flight Center for diverting *ISEE-3* from its original station at the Ll Lagrangian libration point of the Earth-Sun system to an orbit enabling it to fly by Comet Giacobini-Zinncr, the spacecraft being then renamed *ICE* (*International Comet Explorer*), resulted in the first in situ observation of dust and plasma physical phenomena associated with a comet (September 1985).

PLANETARY EXPLORATION

As early as 1960 and in parallel with the activities just sketched, one of my driving aspirations was to push on with magnetospheric studies of the other planets.

The emphasis at NASA in the early 1960s was on manned lunar flights. But the Jet Propulsion Laboratory (JPL) and other groups had already made extensive general studies of the ballistics of flight to other planets—especially Venus and Mars. The interest in Mars was driven by the desire for geological studies of its surface and, perhaps more importantly, by the desire to search for any form of biological activity there. Also, Mars and Venus were ballistically much easier to reach than was Mercury or the outer planets. The first planetary target to be adopted by JPL/NASA was Venus. I proposed a simple radiation detector for the first mission with the purpose of searching for the existence of a Venusian radiation belt and the consequent inferences on the magnetization of this planet, then completely unknown. My instrument was selected and was incorporated into the payload of *Mariner I*, an early in-flight failure, and of *Mariner II*, launched successfully on 27 August 1962. The cruise phase was quite successful, yielding, most importantly, the first continuous measurements of the solar wind by Conway Snyder and Marcia Neugebauer and of the interplanetary magnetic field by Paul Coleman et al, as well as the detection of numerous solar energetic particle events by my apparatus and a companion instrument of Hugh Anderson and Victor Neher. *Mariner II* passed by Venus on 14 December 1962 at a radial miss distance of 41,000 km. In our measurements there was not the slightest indication of the presence of the planet, thereby implying an upper limit on its magnetic moment as 0.18 that of the Earth, its "sister" planet. A casual, and perhaps even correct, interpretation of this result is that Venus is simply rotating too slowly (period 243 days) to drive an internal self-excited dynamo

I had an improved model of the radiation instrument on *Mariner III* (launch failure) and *Mariner IV* (launched successfully of 28 November 1964), which made the first-ever encounter with Mars on 15 July 1965. The interplanetary data yielded a nearly continuous record of solar X-ray flares and of the presence of energetic solar particles, including the discovery of energetic solar electrons and, in stereoscopic combination with data from *Explorer 35* in lunar orbit, the first determination of the altitude in the Sun's atmosphere at which 2–10 Å X rays are emitted. At Mars, as at Venus, we got a null result and inferred an upper limit on Mars' magnetic moment as 0.001 that of the Earth.

Meanwhile, as a member of the Space Science Board and, from 1966–70, of the Lunar and Planetary Missions Board, I had adopted the role of being a special and unremitting advocate of missions to the outer planets—especially Jupiter. The first fruits of these efforts were the adoption by NASA of two missions to Jupiter—later called *Pioneer 10* and *Pioneer 11*—with emphasis on energetic particle and magnetic-field measurements. A special motivation for this emphasis was the radio-astronomical evidence that Jupiter has a huge radiation belt whose population of relativistic electrons emits the observed synchrotron radiation in the decimetric wavelength range. The *Pioneer 10/11* project was managed by the Ames Research Center of NASA with a keen concern for optimizing the scientific yield of the mission. The spacecraft were built by the TRW Company. My proposal for an energetic particle instrument was accepted, after some reduction in its scope, during the vigorous national competition for payload space. *Pioneer 10* was launched successfully on 3 March 1972 and *Pioneer 11* on 6 April 1973. For the subsequent 17 years, the inflight data from these two spacecraft have been a central part of my research life and that of several of my students and associates. *Pioneer 10* made the first-ever encounter (December 1973) with Jupiter and yielded a large body of new knowledge, most especially on its magnetosphere. *Pioneer 11* encountered Jupiter a year later along a different trajectory and confirmed and substantially expanded the earlier findings. *Pioneer 10* has continued on an escape trajectory out of the solar system and, at the date of writing, is about 47 AU from the Sun (nearly 7 billion km)—the most remote man-made object in the Universe. It is still working well and providing daily data on cosmic rays and the interplanetary medium in the outer heliosphere. After its close encounter with Jupiter, *Pioneer 11* made the first-ever encounter with Saturn in September 1979, discovering its magnetosphere and yielding a rich body of new information on the planet itself and its system of rings and satellites. This spacecraft is now also on a solar-system-escape trajectory at a current distance of over 28 AU, and it too is transmitting data on a daily basis.

In the late 1960s and early 1970s a Grand Tour of the Outer Planets was being advocated by the Jet Propulsion Laboratory, in particular, and by other planetary enthusiasts who were advising NASA on new programs. JPL had shown that the forthcoming configuration of the outer planets Jupiter, Saturn, Uranus, and Neptune (a once-in-179-year phenomenon) would make it ballistically feasible to have a single spacecraft fly by all four of these remote planets. The Grand Tour, as such, was a budgetary casualty of late 1970. Soon, thereafter, I was asked by JPL to chair a Science Working Group to develop a more modest-sounding mission, tentatively called MJS (Mariner/Jupiter Saturn). The two-spacecraft mission that we developed was eventually approved and came to life in 1974. It was later renamed Voyager. Although the term Grand Tour was now eschewed in polite conversation, it did not escape our attention that the configuration of the outer planets was independent of budgetary-political considerations in the White House and the Congress.

The successes of the two Pioneer missions produced a greatly enhanced interest in the Voyager missions, as well as in ground-based study of the outer planets. Competition for payload space brought forth a wealth of proposals of new and sophisticated instruments and eventual selection of an excellent complement.

Both Voyagers were launched successfully in the late summer of 1977. Each flew by Jupiter and Saturn and provided great advances, most notably in high-resolution imaging of the atmospheres of the planets, of their satellites, and of their rings and in our understanding of the plasma physics of their magnetospheres. Since its Saturn encounter, *Voyager 1* is on a solar-system-escape trajectory, but *Voyager 2* made the first-ever encounter with Uranus in early 1986 and is now approaching Neptune for a 25 August 1989 encounter—thus prospectively achieving the objectives of the Grand Tour as visualized at the outset of this program.

I have had no part in the execution of the Voyager program but have been a guilty bystander, so to speak, and one of its enthusiastic fans.

In 1976–77, I chaired still another JPL/Ames Research Center science working group, called JOP/SWG [Jupiter Orbiter with (Atmospheric) Probe/Science Working Group]. Our purpose was to develop a follow-on Jupiter mission of more advanced capability than the Pioneers and the Voyagers. This program would have a deep atmospheric entry probe and an orbiter having a useful lifetime of at least two years, in contrast to the limited period (days to weeks) of nearby observation available on a flyby.

The mission, renamed *Galileo*, has suffered a plethora of delays—financial, political, and technical—principally as the result of the inadequacies and defaults of the shuttle launching system, which had been adopted by NASA in the late 1970s and 1980s. The launch of *Galileo* is now scheduled for October 1989, but many uncertainties remain. Also, because of the less than originally planned capability of the shuttle, it has been necessary to adopt an ingenious but very long flight path to Jupiter, requiring over 6 years vs. the 20-month flights of *Pioneers 10/11*. I recognize that the probability of my own survival to 1995 is substantially less than unity.

Nonetheless, I still hope to function in my interpretative role as an Interdisciplinary Scientist beginning after *Galileo*'s scheduled entry into orbit around Jupiter in that year.

CONCLUDING COMMENTS

In the period 1951–85, I served as head of the Department of Physics (which became the Department of Physics and Astronomy in 1969) of the University of Iowa. My formal teaching involved a full gamut of courses: General Physics, General Astronomy, Electricity and Magnetism, Introduction to Modern Physics, Radio Astronomy, Intermediate Mechanics, and a specialized course in Solar-Terrestrial Physics. Perhaps my favorite was General Astronomy, an introductory but rigorous course on the solar system, with laboratory, which I taught for 17 years.

My closest working relationships with students involved ones at the graduate level. The following are those who finished advanced degrees under my guidance. The first date in the parentheses after each name is the date of an MS degree, the second of a PhD degree. Some students who did their MS work with me later earned a PhD elsewhere or under another advisor at Iowa. Others terminated their graduate work at the MS level. Every one was a collaborator in the fullest sense of the word, a fact that is amply represented in authorship or coauthorship of published work.

JOHARI BIN ADNAN	(1983, —)
SIXTEN INGVAR ÅKERSTEN	(1969, —)
HUGH RIDDELL ANDERSON	(1958, —)
THOMAS PEYTON ARMSTRONG	(1964, —)
DANIEL N. BAKER	(1973, 1974)
KENNETH E. BUTTREY	(1955, —)
LAURENCE JAMES CAHILL, JR.	(1956, 1959)
CHARLES P. CATALANO	(— , 1971)
JAMES R. CESSNA	(1965, —)
PHILLIP CHANG	(1962, —)
TSAN-FU CHEN	(1973, 1978)
JOHN D. CRAVEN	(1964, —)
JOSÉ M. da COSTA	(1971, —)
JERRY F. DRAKE	(1967, 1970)
ROBERT A. ELLIS, JR.	(— , 1954)
R. WALKER FILLIUS	(1963, 1965)
HERBERT R. FLINDT	(1968, —)
LOUIS A. FRANK	(1961, 1964)
JOHN W. FREEMAN	(1961, 1963)
THEODORE A. FRITZ	(1964, 1967)
SISTER JEAN GIBSON, O.S.B.	(— , 1969)

CYNTHIA LEE GROSSKREUTZ	(1982, —)
DONALD A. GURNETT	(— , 1965)
ROLLIN CHARLES HARDING	(1966, —)
H. KENT HILLS	(1964, —)
WILLIAM G. INNANEN	(— , 1972)
ROBERT CHANDLER JOHNSON	(1957, —)
JOSEPH E. KASPER	(1955, —)
STAMATIOS MIKE KRIMIGIS	(1963, 1965)
CURTIS D. LAUGHLIN	(1960, —)
WEI CHING LIN	(1961, 1963)
THOMAS A. LOFTUS	(1969, —)
GEORGE HARRY LUDWIG	(1959, 1960)
CARL E. McILWAIN, JR.	(1956, 1960)
LESLIE H. MEREDITH	(1952, 1954)
RAYMOND F. MISSERT	(1955, 1957)
STEVEN R. MOSIER	(1967, —)
MICHAEL O'CONNOR	(1968, —)
MELVIN N. OLIVEN	(1966, 1970)
MARK E. PESSES	(1976, 1979)
GUIDO PIZZELLA	(— , 1962)
ROBERT C. PLACIOUS	(1953, —)
RICHARD LOUIS RAIRDEN	(1981, —)
BRUCE A. RANDALL	(1969, 1972)
JOANNA M. RANKIN	(— , 1970)
ERNEST C. RAY	(1953, 1956)
NICOLAOS A. SAFLEKOS	(— , 1975)
EMMANUEL T. SARRIS	(— , 1973)
MELVIN SCHWARTZ	(— , 1958)
DAVIS D. SENTMAN	(— , 1976)
STANLEY D. SHAWHAN	(1965, —)
HAROLD E. TAYLOR	(— , 1966)
JAMES DENNIS THISSEL	(1963, —)
MICHELLE F. THOMSEN	(1974, 1977)
WILLIAM R. WEBBER	(1955, 1957)
JOEL M. WEISBERG	(1975, 1978)
CHARLES D. WENDE	(1966, 1968)
MICHAEL JAMES WIEMER	(1964, —)
SAIYED MASOOD ZAKI	(1964, —)

In addition, I have benefited from the highly competent efforts of an uncounted number of members of our technical staff, of whom the following are representative: William A. Whelpley, Roger F. Randall, Robert B. Brechwald, Evelyn D. Robison, John E. Rogers, Joseph G. Sentinella, Donald C. Enemark, W. Lee Shope, Edmund Freund, and Robert Markee.

Finally, and most importantly, I am indebted to my wife of 44 years, Abigail, and our five children, who have provided the circumstances under which sustained and intensive professional work has seemed worthwhile.