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Pioneers of space physics: A career in the solar wind

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Abstract. This paper is s personal history of the author's experiences, starting with the earliest direct measurements of the solar wind and continuing through later experiments to investigate the physics of the solar wind and its interaction with comets.

Getting to the Right Place at the Right Time

It is difficult to think of myself as one of the "Pioneers of Space Physics." I certainly did not enter the field because of any strong pioneering spirit. It was simply a matter of being at the right place at the right time. So, I'll start by explaining how it was that I happened to be working at the Jet Propulsion Laboratory (JPL) at the very start of the space age.

I majored in physics as an undergraduate at Cornell. I am not sure why. Perhaps it was because my father had taught me to use a slide rule which made high school physics a lot easier and more interesting than it might have been. Or perhaps part of the attraction was getting to know one of my lab partners in sophomore physics at Cornell, named Gerry Neugebauer.

After finishing Cornell, I went to graduate school at the University of Illinois in Urbana, while Gerry went to Caltech. The first summer in Illinois. I had a job working in David Lazarus's lab studying diffusion of one metal into another. That was not one of my life's greatest successes. The metal I was assigned to study didn't dissolve in the solvent I was told to use, and in my clumsy enthusiasm, I broke a record number of Geiger tubes, which were fairly expensive at the time. Come fall, I decided I'd better work somewhere else and obtained a research job, under Robert Hill, studying anomalous scattering of energetic particles in nuclear emulsions. I got lots of data by measuring particle tracks seen through a microscope situated in a dark closet, but no one could make much sense of my results. Several years later, I learned from my successor that the anomalous scattering had been caused by the closet's air conditioner cycling on and off.

After receiving my Master's degree, I decided to move to California to marry Gerry. When I talked to Carl Anderson, whom Gerry was then working for, about the possibility of transferring to the all-male Caltech, he said "It might be fun to try to get a girl in here." I decided, however, to forego that challenge and make some money instead, and I accepted a job offer from JPL, which is part of Caltech, but situated 7 miles away. I'm not sure why JPL offered me a job working on the feasibility of building nuclear rockets by heating gas in a fission reactor. Perhaps they thought that I really understood diffusion in metals and that that knowledge might be transferable to modeling diffusion of neutrons in reactors.

When I started working at JPL in June 1956, it was sponsored by the Army. It was a time of great competition between the Army and the Air Force to develop missiles. Rooting for the Army's Jupiter missile, we were sometimes treated to movies of spectacular failures of the Air Force's Thor missile. As I remember, not long after I arrived the Secretary of Defense decided that the competition had gotten out of hand and that the Army could not work on anything that landed more than 100 miles from its launch site. That decree ended JPL's work on nuclear propulsion. At that point, our small group, led by Conway Snyder, started learning about ionized gases. Then came Sputnik (October 4, 1957) and Explorer I (the spacecraft was built by JPL and launched on January 31, 1958, by the Army's Jupiter rocket), and we were in the space age. On December 3, 1958, JPL became part of the newly formed NASA, while still being managed by Caltech.

Early Measurements of the Solar Wind

The history of the prediction of the existence of the solar wind and of the first attempts to measure it has been described in a 46-page chapter in a book by a professional historian named *Hofbauer* [1991]. He has given more detail, from Biermann and Chapman, through Parker and Chamberlain, to the measurements by Luniks, Explorer 10, and Mariner 2, than I can give here. Bruno Rossi has also written about the history of the early measurements of the solar wind through the light of Explorer 10 [Rossi, 1962; 1984]. In this paper I will therefore only cover some of the highlights, together with some description of the JPL effort that is not included in those other histories.

One of JPL's early responses to the space age was to assign Ray Newburn and me the task of writing a report about scientific questions that could be addressed by spacecraft, with the emphasis on interplanetary probes. High on our lists were studies of the solar corpuscular radiation, as the solar wind was then called, and of comets. We knew that there were other groups with

Figure 1. NASA press conference, October 10, 1962. (left to right) Ed Smith (magnetometer), the author, Jack James (Mariner 2 Project Manager), Homer Newell (NASA Office of Space Science), Merle Alexander (dust detector), and Hugh Anderson (energetic particle detector).

instruments for studying energetic particles and interplanetary dust, but we knew of no American group that had instrumentation suitable for studying interplanetary plasma. So Conway Snyder and I started to work on developing such an instrument. Soon thereafter we heard that Rossi's group at MIT had started to develop an instrument with the same objectives, and we nearly decided to give up as we thought we could not possibly compete, but we stuck with it.

Theoretical and ground-based observations left a broad range of possibilities for the properties of the interplanetary medium. There was Chapman's model of a hot, static corona, in which the particle fluxes would be almost isotropic [*Chapman,* 1957]. There was Parker's solar wind model, which predicted radial, supersonic flow with speeds of hundreds of kilometers per second, in agreement with the data obtained from the direction of cometary ion tails [*Parker,* 1958]. There was Chamberlain's solar breeze model, which also had radial fluxes, but with speeds of ~10 km/s consistent with exospheric escape from the Sun [*Chamberlain,* 1960]. The density was equally in doubt. Some interpretations of observations of the zodiacal light in which all the polarization was attributed to scattering by electrons led to densities as high as 1000 cm^{-3}. Such high densities were consistent with models of the orientation of comet tails if the interaction were due to Coulomb collisions between the interplanetary and cometary plasmas. Parker's model, on the other hand, required densities of only ~10 ion pairs/cm^3. Until some direct measurements were made, we felt our instrument had to be designed to cover all possibilities.

My first idea for an instrument was to use a fixed electric field to deflect a collimated beam of ions and electrons onto an array of Faraday cups, with each cup detecting particles in a specific energy range. Conway had been exposed to a lot more instrumentation than I had, and he quickly convinced me that it would be much better to use a curved plate analyzer to provide a variable electric field and have only a single detector that measured different energy ranges in temporal sequence. Some of the methods we used to design the analyzer now seem terribly quaint. To calculate the ion trajectories through the cylindrical analyzer, I took the equations and a set of instructions to a "computer," which was a lady (one of many in JPL's computer section) who integrated the equations and plotted the trajectories point by point using pencil and paper and an electromechanical desk calculator. We decided on the shape of the field-shaping entrance electrodes by painting candidate configurations onto special paper with metallic paint and then moving probes around to map the equipotentials. For the electronics we obtained the aid of a young electrical engineer named Conrad Josias. Connie had recently distinguished himself in the Navy by designing the first high-sensitivity electrometer that was small enough to fit down a submarine hatch. He thought he could use the same vibrating-reed capacitor technique to get an electrometer small enough and sensitive enough to meet our needs. Connie and I worked well together, despite the fact that he thought he could make everything clear to me by covering a blackboard with circuit diagrams. To be honest, I never understood Connie's vibrating-reed electrometers, but I think Conway did. Anyway, with the help of another young engineer named Jim Lawrence, and others, Connie made it work just fine. The instrument was officially known as the Solar Corpuscular Radiation Electrostatic Particle Analyzer, but, of course, no one ever said that; they just called it SCREPA.

By the time we had an instrument, we discovered we had plenty of competition and what we needed was a flight. Table I summarizes all the solar wind experiments launched through 1962. Not surprisingly, the Soviets went first. They launched "ion traps" on four deep space missions. These were Faraday cups with a -200 V inner grid to prevent the escape of photoelectrons from the collector and an outer grid at a positive potential which defined the minimum energy of the ions that could be detected. The measured signal was the sum of the fluxes of ions with energy above the outer grid potential, electrons above 200 eV, and photoelectrons produced at the electron suppression grid. Lunik II was by far the most successful of the four. It determined that there was indeed a flux of ~$2x10^8$ $cm^{-2}s^{-1}$ of positive ions, from some unknown direction, with energies >15 eV [*Gringauz et al*., 1960]. These results were consistent with, but certainly not proof of, Parker's theory, which had been published just the year before.

Decisions concerning what instruments would fly on which U.S. spacecraft were certainly above my pay grade, but on several occasions, Conway and I were called upon to defend our instrument against the competition on scientific grounds. The history of how instruments were selected in those days has been well documented by *Naugle* [1991]. Even though Conway and I had no reputation in the field (actually, neither did anyone else), we obtained opportunities to fly principally because JPL's management wanted to get into space science and vigorously supported our cause, and because NASA headquarters wanted to spread the opportunities among several institutions.

The first U.S. experiment was the modulated Faraday cup designed by Rossi's group at MIT and flown on Explorer 10. The design was a considerable improvement over the Soviet ion traps in that a square-wave voltage was placed en the positive grid which allowed measurement of an energy spectrum by shifting the upper and lower limits of the square wave, while by measuring only the ac signal, the signal from photoelectrons, which was almost entirely dc, could be greatly reduced. Because Explorer 10 was powered by batteries (no solar powered missions had yet flown), it lived only 2 days during which it reached a distance of ~34 Earth radii (R_E). Intermittent ion fluxes were observed between 17 R_E at a local time of ~2200 and 34 R_E at a local time of ~2250. When flow was detected, it came from somewhere within a 60° field of view that included the solar direction. It became clear that Explorer 10 had not reached the undisturbed solar wind but had probably skimmed along the magnetopause, alternating detections of magnetosheath plasma with submersion in the outer geomagnetic tail. The MIT scientists determined the speed, density, and temperature of the magnetosneath plasma and established that the flow was supersonic [*Bonetti et al.,* 1963].

Next it was the turn of a group at NASA Ames Research Center on Explorer 12. Their instrument was a quadrispherical curved plate analyzer, again with an electrometer as a detector. Explorer 12 was in an eccentric orbit with an apogee of 13 R_E which moved through the morning sector during the course of the 4-month mission. The Principle Investigator (PI),Michael Bader,

summarized the experiment's results as [*Bader*, 1962, p. 5009] "The most significant result of the experiment is actually the absence of any detectable flux at any altitude, either during quiet periods or at times of geomagnetic disturbances… We note in addition that directions as close as 15° from the Sun were sampled during the lifetime of the satellite with apparently equally negative results." Because of poor communication between the Project team and the PI, the instrument never looked directly into the Sun, and it was not sensitive enough to detect the hot plasmas in the magnetosheath and magnetosphere. Nonetheless, at scientific meetings, Bader said there was no solar wind.

Late in 1962, NASA Ames Research Center got a second chance to measure the solar wind, this time on Explorer 14 under the leadership of John Wolfe. That spacecraft had an apogee of 16.5 R_E at an initial local time near 0700. The instrument was similar to that on Explorer 12. Its principal problem was that it was blinded by solar UV whenever it looked within 3° of the Sun. The history of that flaw is an interesting story that Wolfe told on himself. John Wolfe and I had a reasonably friendly and cooperative relationship, and I had mentioned to him that we had gone to great lengths (coating the electrode surfaces with gold-black) to prevent UV from scattering down our curved-plate analyzer. He thanked me for the hint and instructed his technician to test their instrument for UV response. None was found, until the actual flight. It turned out that Wolfe had neglected to tell his technician that the UV test had to be done in a vacuum chamber. Despite that problem, *Wolfe and Silva* [1965] did obtain a small amount of solar wind data during a geomagnetic disturbance.

Between the two attempts by NASA Ames Research Center, we had our chances. The Ranger series of spacecraft was NASA's first attempt at lunar and planetary missions [*Hall*, 1977]. The first two missions, Rangers 1 and 2, were intended to be technology demonstrations; they would be the first spacecraft to have three-axis stabilization and the first to use parking orbits, in which the spacecraft would coast in low-Earth orbit before the final propulsion maneuver to send them on their way. Parking orbits would allow reasonably long launch windows for lunar and planetary targets. Since the guidance needed to hit the moon was not scheduled for testing until Ranger 3, the nominal trajectory for Ranger 1-2 was a highly elliptical Earth orbit with an apogee of about 10^6 km. The payload consisted largely of fields and particles experiments.

Our SCREPAs were scheduled to fly on Rangers 1 and 2. The experiments were approved in early 1960, when the reality of Parker's solar wind was still not proven. Just to cover our bets, in case Parker should be wrong, we flew six instruments on each spacecraft, one pointed at the Sun and the others in the remaining orthogonal directions. Ranger I was launched on August 22 1961, during its fifth countdown in four weeks. Unfortunately, the second burn of the Agena rocket did not work, and Ranger I was stuck in a low Earth orbit. The spacecraft was exercised and shown to perform well, but it reentered the Earth's atmosphere eight days after launch. Much to our dismay, the same thing happened to Ranger 2. We, of course, got no solar wind data, but a few engineers showed mild interest in the analysis of the tumbling motions that could be discerned as our six instruments took turns scooping up ionospheric plasma.

In parallel with the development of the Ranger missions to the Moon, JPL was designing a set of Mariner missions, with two Mariner A spacecraft planned to launch for Venus in the summer of 1962 on an Atlas-Centaur. The MIT plasma instrument had been scheduled to fly on those missions. In the summer of 1961, however, it became clear that the Centaur upper stage would not be ready in time. Consequently, in September 1961, NASA approved the development of a much smaller spacecraft (half the mass of Ranger 1-2 and one third the mass of Mariner A) to be launched by the Atlas-Agena the following summer. Because of the imminence of the launch date, the mission had to be designed in a week, and it happened that Herb Bridge, the leader of the MIT group, was in China at the time and could not submit a proposal. Hence our instrument was chosen. Largely because of the results of Explorer 10, we dared strip down our Ranger experiment from six sensors to one, and we deleted the power supply required to measure electrons and made other minor changes to meet the new mission's allocations for the solar plasma instrument of 1 w and 5 pounds (NASA had not yet converted to metric units).

This new mission was initially called Mariner R, because the spacecraft was assembled mostly from leftover parts from Ranger. The first Mariner R, Mariner 1, with our instrument on board, was launched in July 1962, but because of a missing hyphen in the program that guided the Atlas, the rocket headed for the North Atlantic shipping lanes and was destroyed by Range Safety. At that point our record was 12 curved-plate analyzers vaporized in the atmosphere and one at the bottom of the ocean.

The word "miraculous" keeps arising in most people's descriptions of Mariner 2. Shortly after the launch on August 27, 1962, the Atlas guidance system again misbehaved and the rocket rolled over, through a complete loop, and, miraculously, ended up pointing in just the right direction. After launch, telemetry from the Earth sensor indicated that the brightness of the Earth was much too weak, and kept getting weaker, approaching the level at which lock on the Earth would be lost. On September 8, the spacecraft lost its attitude control, but reoriented itself three minutes later with the Earth signal, miraculously, just what it should have been. At the beginning of November, one of the two solar panels stopped working, so all the scientific instruments were turned off; one week later, the solar panel, miraculously, started working again and the instruments were turned back on. Everything except the solar panels got much hotter than anticipated to the extent that seven temperature sensors hit the tops of their ranges. The overheated control system didn't issue the command to start the Venus encounter sequence, but the spacecraft did accept a command from the ground and, again rather miraculously, the superheated spacecraft carried out the observations of Venus as planned except for a decreased number of scans across the planet due to a failure within the microwave radiometer. After the Venus encounter on December 14, our data return gradually became sparser and finally, on January 3, Mariner 2 ran out of miracles and its radio signal was lost. A history of the Mariner 2 mission, from conception through completion, has been compiled by *Wheelock* [1963].

Table 1. The earliest attempts (1959—1962) to Observe the Solar Wind

Date	Spacecraft	Institution	Instrument	Result
Jan. 2, 1959	Lunik I	former USSR	4 ion traps -10 to +15 V	no publishable data
Sept.12, 1959	Lunik 2	former USSR	4 ion traps -10 to +15 V	39 - 60 R_E flux> 15eV=$2x10^8$ $cm^{-2}s^{-1}$
Oct. 4, 1959	Lunik 3	former USSR	4 ion traps -19 to + 25 V	one observation of flux > 20 eV=$4x10^8$ $cm^{-2}s^{-1}$ other data < threshold (~10^8 $cm^{-2}s^{-1}$)
Feb. 12, 1961	Venus Probe	former USSR	Ion traps 0 and 50 V	very intermittent data one observation of flux =$4x10^8$ $cm^{-2}s^{-1}$
March 25, 1961	Explorer 10	MIT	Modulated FC	skimmed magnetopause flank consistent with flow from Sun measured *n, V, T* supersonic & superAlfvénic
Aug. 16, 1961	Explorer 12	NASA Ames	CPA	dayside magnetosheath didn't detect any ions
Aug. 22, 1961	Ranger 1	JPL	6 CPAs	failed to get out of parking orbit
Nov. 18, 1961	Ranger 2	JPL	6 CPAs	failed to get out of parking orbit
July 22,1962	Mariner 1	JPL	CPA	destroyed by range safety
Aug. 27, 1962	Mariner 2	JPL	CPA	113 days of data continuous radial flow high, low speed streams *n, v, T* relations $v_\alpha = v_p$; n_α/n_p variable: $T_\alpha=4T_p$
Oct. 2,1962	Explorer 14	NASA Ames	CPA	mostly magnetosheath UV interference

FC is Faraday cup and CPA is curved plate analyzer.

We had data! Lots of it! There was no longer any uncertainty about the existence and general properties of the solar wind [*Snyder and Neugebauer,* 1962]. We had a spectrum of the solar wind almost every 3.7 mm for 113 days between Earth and the spacecraft perihelion at 0.7 AU. The solar wind blew continuously within our instrument's field of view (which dropped to zero at 10° from the Sun), energy range (231-8224 eV/charge), and dynamic range (currents between 10^{-13} and 10^{-6} amp). Parker had certainly been right on. NASA arranged a press conference (see Figure 1), and Mariner 2 was above the fold on the front page of the New York Times.

The Mariner 2 data were very primitive by today's standards of, for example, the 40 energy channels in each of 79 angular directions measured by Ulysses. We had to work with one-dimensional spectra (no angular information) containing measurable currents in only one to five energy/charge channels. We were nonetheless able to learn that the solar wind was organized into high- and low-speed streams, that the streams had steepened leading edges with higher densities due to pileup, and that the proton temperature varied directly with the speed [*Neugebauer and Snyder,* 1966]. We even learned something about the helium in the solar wind. Figure 2 is an example of our best spectra. In working with such five-point spectra I found that I could not fit the currents in the two highest channels unless I assumed that the alpha particles had the same thermal speeds as well as the same bulk speeds as the protons; equal temperatures for the two ion species were highly incompatible with the data. Given those results, we were also able to determine that the ratio of alpha particle to proton densities was highly variable.

It is interesting to reconsider the data in Table 1 as viewed from today's context of a better, faster, cheaper space science program. Back then, we probably were not any cheaper, and so many things went wrong that we could not claim to have been better, but we certainly were faster. NASA launched seven missions to measure the solar wind in a year and a half. From project approval to the launch of Mariner I was 10 months. The launch failures were less devastating then than they would be today, because we usually knew we had another chance. There are essentially no second chances today, in part because since 1962 there has been a blossoming, almost an explosion, of competing groups. Goddard Space Flight Center, Los Alamos, the University of Iowa, and the Max-Planek-Institut all started developing solar wind instruments very early. More recently, good experiments have been developed by Lockheed, the University of Maryland, the University of New Hampshire, Southwest Research Institute, the University of California at Berkeley, and a number of groups in Europe.

OGO, ALSEP, not ISEE, and Giotto

After the success of Mariner 2, Conway and I were selected to fly solar wind spectrometers on two other missions: the Orbiting Geophysical Observatory 5 (OGO 5) and the Apollo Lander Science Experiment Package (ALSEP) for Apollo flights 12 and 15.

OGO 5 was in an eccentric Earth orbit (1.1 x 24.0 R_E), spending a lot of time outside the Earth's bow shock for much of each year. It was launched on March 4, 1968, and operated successfully for over 3 years. Our experiment consisted of (1) an electrostatic analyzer with the same geometry as the Mariner 2 instrument for the sake of intercomparison of the data for the two different times, but with more energy channels and the capability to reverse the polarity on the electrodes to analyze electrons; it obtained an energy spectrum every 5.2 to 76.0 s, depending on the telemetry rate, and (2) a Faraday cup analyzer with a modulating grid which made rapid measurements (0.288 to 4.608 s/measurement) of the total flux of the solar wind and its flow direction (by comparing the currents to each of three collectors). A description of the OGO 5 instrument is given by *Neugebauer* [1970].

On Mariner 2, we had surveyed the general properties of the solar wind, its speed, temperature, and helium content and the variation of those properties through solar wind streams, whereas with OGO 5 we started doing some real physics. I had many fruitful collaborations with Chris Russell (magnetometer data). Fred Scarf and Gene Greenstadt (plasma wave data), Ed Smith (data on low-frequency magnetic fluctuations obtained with a search-coil magnetometer), and others. Among the results of the OGO 5 experiment were analyses of the structure of the Earth's bow shock [*Neugehauer,* 1970] and its dependence on upstream parameters such as field direction, Mach number, and plasma ß (the ratio of plasma to magnetic pressures) [*Greenstadt et al.,* 1975; 1977], the power spectrum of fluctuations in the solar wind [*Unti et aL,* 1973; *Neugebauer,* 1975; 1976a], discontinuities and waves [*Unti et al.,* 1972: *Neugebauer et al.,* 1978], and differential streaming between alphas and protons [*Neugebauer.* 1970; 1976b]. Although our 0G0 5 experiment was designed to study the solar wind, some new and useful measurements were also obtained on the internal structure of the magnetopause [*Neugebauer et al.,* 1974] and in the outer regions of the polar cusp [*Russell et al.,* 1971; *Scarf et al.,* 19741. A good time was had by all involved.

At the time the ALSEP instruments were selected, the basic features of the interaction of the solar wind with the moon were unknown. Between the ALSEP selection and the activation of the instruments on the Moon (November 19, 1969, and August 2, 1971), however, the Explorer 35 spacecraft had been put into a lunar orbit, in July 1967, with periselene 800 km above the lunar surface. The instruments on Explorer 35 were able to determine that the Moon had no bow shock and no appreciable magnetosphere, either intrinsic or induced [*Lyon et al.,* 1967; *Ness et al.,* 1967]. The solar wind almost certainly directly impacted the lunar surface. Explorer 35 had accomplished much of what we had proposed to do with our ALSEP experiments.

Each ALSEP solar wind spectrometer was a cluster of seven modulated-grid Faraday cups configured such that all of the lunar sky was covered by at least one cup. Spectra of both positive ions and electrons were obtained. The sensors were set up ~35 cm above the lunar surface by the astronauts. The instruments blew their dust covers and started operation after the departure of the astronauts. The Apollo 12 and 15 ALSEPs operated for 6.3 and 0.9 years, respectively, obtaining data for about 40% of each lunar orbit around the Earth. More information about the design of these instruments is given by *Snyder et al.* [1970] and *Clay et al.* [1972].

By comparing the properties of the solar wind at the Apollo

12 site, which had a local magnetic field of 38 nT, with those at the at the Apollo 15 site where the local field was only 3±3 nT, and at OGO 5, we determined that the local field at the Apollo 12 site accelerated solar wind electrons, decelerated, deflected, and heated the protons, and generated low—frequency waves [*Clay et al.,* 1975]. The lunar surface clearly turned out to be a poor platform from which to study the solar wind except at sites where there was very little remnant magnetism. Bruce Goldstein joined our group after his graduation from MIT in 1971 to help us dig out from under the great volume of data generated by the simultaneous operation of OGO 5 and the ALSEPs. Bruce took on the analysis of ALSEP electron data and found some interesting effects due to space charge separation in the local magnetic field [*Goldstein,* 1974].

About this time, Conway Snyder dropped out of space plasma physics to concentrate on Mars missions, so I became the head of the group. One of our next activities was to propose instruments for what are now known as the ISEE 1, 2, and 3 spacecraft. The Announcement of Opportunity called for plasma instruments with sufficiently high time resolution to be able to take advantage of the close spacing of the ISEE I and 2 spacecraft to determine small scale plasma structures. We extrapolated our experience with Faraday cup detectors on OGO 5 and the ALSEPs to invent a belt-shaped Faraday cup which could analyze the solar wind or other plasma beams through all phases of the spacecraft spin. Our proposals were not accepted. Members of NASA's review panel later told me that our proposals were the clear favorites as far as the scientific capabilities were concerned, but the program managers from NASA headquarters said they did not have enough money in the project budget to support the development of new instruments such as ours. Our colleague, Ed Smith, was selected to provide the magnetometer for ISEE 3; it was based on spare hardware from the Pioneer 10 and 11 missions. Off-the-shelf hardware clearly won out over innovation. What doubly hurt was that when we then asked for funding for instrument development so we could be ready for the next time, we were turned down because there was no relevant future mission in NASA's plans.

So the ALSEPs turned out to be the last JPL-built plasma instruments. The engineers who had developed our instruments either left JPL to start their own company or moved on to much bigger instruments (mainly imaging) which had much higher priority for JPL as an institution. Where did those unhappy events leave us? In the instrument arena, we focused on the development of an ion mass spectrometer specifically aimed at analyzing the mass and energy spectra of fast or hot ions expected in the magnetosphere of Jupiter or in the coma of an active comet; for this development we obtained funding from JPL's Director's Discretionary Fund and from NASA's Planetary Instrument Definition and Development Program. We toyed with a lot of different ideas for almost a year, and then one night, after the kids had gone to bed, I suddenly realized the wonderful properties of a sector magnet. It could act as a rigidity (momentum/charge) filter while preserving the angular distribution of a fan-shaped beam. If one then applied an electric field roughly perpendicular to the filtered beam, you could spread the particles out onto a two-dimensional detector with mass/charge along one axis and angle of incidence along the

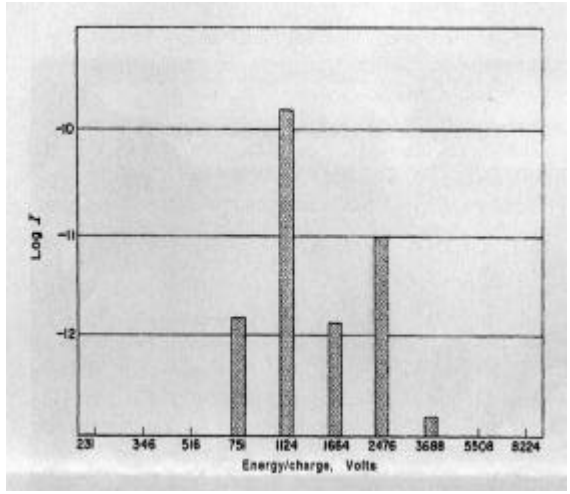

Figure 2. One of the better spectra obtained by the Mariner 2 solar wind spectrometer. The current I is given in amperes.

other. The energy spectrum could be obtained by accelerating or decelerating the ions to the proper rigidity to get through the magnet, while the other angular dimension could be obtained from the spacecraft spin. By this time, we no longer had an engineering staff or a laboratory, but Ray Goldstein and Doug Clay built and scrounged what was needed to test the concept, while Bruce Goldstein and I modeled the performance on the computer. Not only did it work, but it had a mass resolution better than any fast-ion mass spectrometer then flying [*Neugebauer et al.,* 1982]. (The performance of this type of instrument has since been surpassed by the isochronous mass spectrometer based on time of flight in a harmonic electric field.)

Our proposal for flying this instrument on the Galileo mission was not accepted, but we had success in the comet arena. We won a place on the Giotto mission to fly by comet Halley as part of a large consortium, with Hans Balsiger of the University of Bern as the PI. The Giotto ion mass spectrometer (IMS) had two detectors, a high-intensity spectrometer (HIS) furnished by Helmut Rosenbauer of the Max-Planck-Institut für Aeronomie (MPAe), and our high-energy-range spectrometer (HERS). HERS was designed to analyze the pickup ions in the outer coma, while HIS would detect the nearly stationary ions in the inner coma [*Balsiger et al.,* 1987]. I think HERS was probably in the vanguard of tile multi-institutional, multinational collaborations necessitated by the shortage of funding from any single country. For HERS, the theoretical/numerical modeling was done at JPL, with help from Co-I Alan Lazarus at MIT, the ion optics and sensor were built under the direction of Ed Shelley at Lockheed Palo Alto Research Laboratory, the electronics were built by MPAe, while the Swiss provided the magnet, put it all together, and calibrated it with the help of Ray Goldstein who moved to Switzerland for two years. Rather amazingly, it all worked out. Giotto's flight through the coma of comet Halley brought back memories of our first attempts to measure the solar wind. As before, we had only guesses about the densities and

distributions of the ions we wanted to measure; there was a very uncomfortable worry that our assumptions or calculations were wrong, and we wouldn't detect anything. Although HERS started detecting pickup cometary protons more than a day before the encounter, we had a long, tense wait before it saw any heavy ions. Finally, only about 40 mm before closest approach, the heavy ion counts streamed in, well above threshold and on scale. Then, just before closest approach to the comet nucleus, the monitor went blank; HERS was dead, knocked out by a massive electrical transient caused by the high-speed collision of the spacecraft with a dust grain. From our brief run of data, we were able to learn a lot about the chemical composition of the comet and about the plasma dynamics of the interaction of the solar wind with an active comet [*Balsiger et al.*, 1986].

All along, there have been two aspects to my work: conceptual design of instruments and data analysis. I've been fortunate in being able to work on data from other people's instruments as well as our own. For the last few years I've focused on the great wealth of data returned by the solar wind experiment built for the Ulysses mission by Los Alamos. I've also worked with archived solar wind data from many different spacecraft to address specific scientific problems. Over the years I've used ISEE, Helios, and Voyager data to study discontinuities in the solar wind [*Neugebauer et al.*, 1984; 1985; 1986] and the properties of the interplanetary plasma from coronal mass ejections [*Neugebauer and Goldstein*, 1997; *Neugebauer et al.*, 1997]. The solar wind data archived by the National Space Science Data Center (NSSDC) are an extremely valuable resource for a wide spectrum of important problems in heliospheric physics yet to be addressed. Whenever I wanted higher-resolution data or parameters not included in the NSSDC database, the institutions who built and flew the instruments almost always obliged.

Project Work

At NASA centers, scientists are generally expected to get involved in the center's projects. It works to the advantage of both parties. Young scientists are exposed to how flight projects work, receive an excellent education in project engineering and operations, and are exposed to a broad range of space science. Perhaps more importantly, project work provides part time financial support for starving scientists while justifying their existence at a NASA center.

I was JPL's first Project Scientist, for Rangers 1 and 2. A Project Scientist's job is to understand the legitimate requirements of the scientists and to interpret them for the Project team and to interpret and explain the Project team's requirements to the scientists [*Naugle*, 1991]. The Project Scientist is the PI s' on-site eyes, ears, and mouths in the day-to-day operation of the project. The job is not always pleasant. On Ranger, for example, I was caught in the middle of a three-sided battle between the PI of the magnetometer experiment, the PI of an engineering experiment to measure the friction between motor driven surfaces in the hard vacuum of space, and the Project Manager who felt his mandate was to test the Ranger technology, not to do science [*Hall*, 1977]. The Project Scientist is frequently involved in establishing priorities for funding, telemetry rates, power, mass, etc., often with the advice of the PIs as assembled into a Project Science Group. JPL's efforts on behalf of PIs has not always been appreciated, although I think relations have greatly improved since the day in the early 1960s when the world's most eminent space scientist called JPL a "bunch of fatherless engineers." With the current emphasis on faster, better, cheaper, JPL's oversight of non-JPL hardware has declined to more of a philosophy of accepting "black boxes" that don't show any sign of being harmful to the spacecraft.

Another role of JPL scientists is to participate in the analysis and planning of potential or planned new missions. I was the Study Scientist for the Out of Ecliptic mission, which eventually changed its name to the International Solar Polar Mission and then to Ulysses. I was also the first, of many, Study Scientists for a Solar Probe mission. Then began a 20-year effort on behalf of U.S. comet missions. Between 1976 and 1996, when I decided not to do it anymore, I was the Study Scientist or the Project Scientist for five different comet missions, none of which ever happened. Three of the missions reached the stage of NASA releasing an Announcement of Opportunity for science investigations, and two (the Comet Rendezvous Asteroid Flyby mission (CRAF) and the Rosetta Champollion lander) science selections were made before the funding was cut off. During the 6 years I was Project Scientist for CRAF, the launch date slipped by 5 years (in five steps). If I hadn't gotten to learn a lot of cometary science, to participate in the Giotto mission, and to work with a lot of good people I wouldn't otherwise have met, it would have been a complete waste of a substantial fraction of my time and energy for 20 years.

Other Activities and a Look Forward

Of course, the flight experiments and project work described above are only part of what I've done over the last 40 years. Gerry and I have raised two daughters of whom we're very proud. I've had management positions within both JPL and AGU that were both challenging and fun, at least most of the time.

I've also served on more JPL, NASA, and Academy committees than I could count. Particularly noteworthy, mainly because of its relative recency, was my chairmanship (1991-1994) of the Committee on Solar and Space Physics (CSSP), which is a subcommittee of the National Academy of Sciences Space Studies Board and which operates in a federated mode with the Committee on Solar Terrestrial Research (CSTR) supported by the National Science Foundation. The two most important reports issued by the CSSP-CSTR during my tenure were the "Paradox report" [*CSTR and CSSP*, 1994] and *A Science Strategy for Space Physics* [*CSSP and CSTR* , 1995]. The first of these, for which Don Williams, then chair of CSTR, was the driving force, looked at how administrative, managerial, and funding decisions affected the health of a scientific discipline. Some people at NASA headquarters hated the report and thought its publication was counterproductive, but we were proud of it. It addresses topics akin to the problem of wasted effort chasing comet missions.

The "Strategy" report recommends the major directions for research in space physics for the next decade. There are still a lot

of important things to do in heliospheric research, like making in situ observations in the corona to observe directly the mechanisms responsible for heating the corona and accelerating the solar wind, and like discovering where and how the solar wind terminates. We still have to do the canceled half of the Ulysses mission to look down on the equatorial corona from above. In the next decade it should be feasible to use solar sail technology to place a spacecraft in a circular polar orbit at a distance of perhaps 0.5 AU from the Sun.

I feel very fortunate to have been able to participate in what may have been the golden age of space physics. My generation may have cleaned up most of the easy stuff, but the next generation still has many exciting and important tasks to tackle.

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This is the final paper in the Pioneers of Space Physics series.

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