1. How the Launch Almost Didn't Happen

In 1973 I was in my second year of graduate work at the University of Iowa. How I got there is a long story, but suffice it to say that it turned out well, both personally and professionally. Under the guidance of James Van Allen, my master's thesis project was to determine the heliospheric radial cosmic ray gradient using measurements from the Iowa Geiger Tube Telescope on Pioneer 10 as it moved outward through interplanetary space on its way to Jupiter. The problem to be addressed seemed simple enough: Assuming the long-term change in the count rates was due to a radial gradient, determine the gradient. That gradient would provide a critical test of various theories about how the structure of the sun's plasma and magnetic field output could modulate the cosmic rays as they propagated into and through the heliosphere.

However, there were complications that made the analysis thornier than it first appeared. One major one was the penetrating background produced by the radioactive decay of the plutonium in the Radioisotope Thermoelectric Generators that provided power for the spacecraft. This background varied with time as the plutonium and its radioactive daughter products decayed (each with its own half-life). Another complication was that the sun's modulation of the cosmic rays was itself not constant in time. On top of the radial dependence I needed to measure, there were substantial temporal variations caused by variability in the solar wind. And a third difficulty in the work was the need to estimate the uncertainty, which involved estimating the possible errors in all the elements of the calculation and doing the mathematical propagation of those errors into the answer. Finally, to understand the potential significance of the result, whatever it would be, I had to become familiar with the published theoretical work on cosmic ray propagation and the mathematical approaches various people were taking to try to describe the problem. That turned out to be quite a slog!

In the face of all these difficulties, I began to grow a little discouraged and wondered if this was really how I wanted to spend my life. So, with an eye toward a possible career shift, I enrolled in a course on radiation biology. It was quite interesting to learn about the effects of various types of radiation on different biological materials and about repair mechanisms that some cells have. However, that “book-learning” constituted only the first semester of the class. When I learned that for the second semester we would be dealing with live lab animals (rats, I think) and that part of the study would involve sacrificing the animals, I decided that space physics looked a lot more interesting after all. So I skipped the second semester and returned my attention to space.

2. A Fantastic Experience

The other factor that brought me back to space was the impending arrival of Pioneer 10 at Jupiter in early December 1973. As a member of Van Allen's research team, I had the opportunity to travel to NASA Ames Research Center in Mountain View, California, to help receive and analyze the encounter data that would be arriving there. Ames was the NASA center that managed the Pioneer program.

In mid-November, Dan Baker and Davis Sentman, two of Van Allen's graduate students, headed out to Ames to set up Iowa's space in the bullpen that would house the research teams for the various Pioneer instruments. A few
days later Van Allen and his research associate Bruce Randall went out, and I arrived a few days after that. We stayed at a motel in Mountain View and manned our space at Ames in shifts around the clock.

Ames was an amazing place. One of its major areas of research was in the aerodynamics of aircraft, and across its campus there were numerous enormous wind tunnels (one, e.g., was 40′ × 80′ in cross-section), each stretching over most of a city block. It was quite exciting to pass by these behemoths as we went to and from our encounter location in the Center.

The quick-look data for our instrument came to us as computer printouts on 15″ × 11″ fan-fold computer paper, the kind with perforated edges with sprocket holes to allow it to be fed through the printer. We got just seven data points (one count rate for each of the seven detectors) every 15 min (The telemetry rate for the entire spacecraft was only 1,024 bits per second!) To get the data, someone from the team had to go to the data center, where the pages for each instrument team had been torn off and put in the team boxes. Then we had the excitement of plotting those seven points on semilog graph paper, color-coded for the different detectors.

When Dan and Dave first arrived in Mountain View, the count rates for the Iowa detectors were bumping along at the low levels characteristic of the journey through interplanetary space. Eight days before closest approach, the magnetometer and plasma instrument indicated that we had crossed the bow shock that stands in the oncoming solar wind upstream of the magnetosphere. Jupiter's magnetosphere turned out to be huge: If it were visible to a human eye on Earth, it would have a significantly larger apparent size than the full moon. Our first crossing of the magnetopause occurred at a distance of 6.9 million km from the planet! For comparison, the Earth's dayside magnetopause is typically found at about 63,000 km.

Inside the magnetosphere, the count rates settled into a periodic rise and fall roughly every 10 hr, Jupiter's rotational period. Earlier Earth-based radio observations had indicated that the axis of the planet's magnetic field was tilted somewhat relative to the planet's rotational axis, similar to the Earth. If the energetic charged particles trapped within the magnetic field were concentrated primarily near the magnetic equatorial plane, as at Earth, then as the planet rotated, the highest-intensity region would sweep up and down across the spacecraft, producing the periodic variation in count rates that we observed.

Van Allen and his fellow investigators recognized this, and he sought a way to illustrate it for the benefit of the reporters at the daily press conferences. After thinking about it a bit, he called back to his engineering team at Iowa and described the model he wanted them to make. They put it together that day, and packed the disassembled pieces for me to take to Ames the next day (see Figure 1). As soon as I delivered it, Van Allen reassembled it: a central ball decorated with Jupiter-like markings, mounted on a rotating axis, with two rings of holes circling each pole. Corresponding holes in the north and the south were connected by stiff wires like coat-hangers (the “magnetic field lines”), which stuck out as loops into space well away from the ball itself. The ones connecting the lower-latitude holes were shorter and rounder, approximating the shape of the dipole magnetic field; the ones connecting the higher-latitude holes were longer and stretched out radially near the crossing of the equatorial plane. The key element of the model was that the rings of holes were offset from the rotational axis to account for the tilt of the magnetic field. When it was all assembled, the planet rotated and carried its tilted field around, so that a fixed point near the rotational equatorial plane would alternately find itself close to or far away from the magnetic equator. The model was a great hit at the press conference!

The fly-by of Jupiter took a couple of weeks, and our hand plot of the count rates covered numerous pages of graph paper, which we taped sequentially together and posted on the wall above our workspace, ultimately forming a 10-foot long picture of the encounter. As Pioneer 10 drew ever closer to Jupiter, the count rates rose dramatically, as did the excitement level on the team. Within about 10 or so Jupiter radii from the planet, some interesting dips occurred in the rates that didn’t seem to be part of the 10-hr periodicity associated with the rotation of the

![Figure 1. James Van Allen illustrates his description of Jupiter's magnetosphere at a press conference during the Pioneer 10 fly-by of the planet in 1973. After he saw the early data, he conceived of the model, described it by phone to his team in Iowa City, and had the working model delivered within two days (credit: Courtesy of the University of Iowa James A. Van Allen Collection). Van Allen was a wonderful mentor and played a foundational role in my career.](image-url)
tilted magnetic field. Rather, the dips appeared near the orbital distances of some of the moons of Jupiter, the inner Galilean satellites. These dips would ultimately become the target of my Ph.D. thesis.

Pioneer 10 survived the incredibly intense radiation in Jupiter’s inner magnetosphere and made its closest approach to the planet on December 4, coming within about 83,000 miles of the upper atmosphere. As it passed behind the planet during closest approach, the radio signal was lost for about an hour, and we all held our breath until the spacecraft reappeared and the signal returned. The radiation in the inner magnetosphere was so intense that it created a number of upsets in the spacecraft electronics, but the only significant effect was the loss of a few images. On the outbound leg of the encounter, in Jupiter's dawn sector, the count rates generally declined, and the 10-hr periodic decreases associated with the rocking of the magnetic field became extremely prominent, with the dips dropping down to almost interplanetary levels. This pattern indicated that on its dawn side, the magnetosphere was strongly flattened compared to the more dipolar shape in the noon region we had traversed inbound.

Participating in the Pioneer 10 encounter with Jupiter and then at six other subsequent encounters (Pioneer 11 at Jupiter and then at Saturn, ICE at Comet Giacobini-Zinner, Giotto at Comet Halley, the Cassini orbital insertion at Saturn, and the Juno orbital insertion at Jupiter) are some of the most treasured memories of my career. Although the technical capabilities for receiving and analyzing the data have improved enormously over the last 50 years, the experience has been the same: There was the thrill of discovery—of “seeing” something no one had ever seen before. There was the tension of anxious moments waiting for some crucial event or maneuver. There was the opportunity to meet colleagues, from big names in the field to graduate students who would become big names in years to come. And there was the camaraderie, the sense of common purpose, of being so fortunate to participate in a unique event.

3. People

Indeed, “fortunate” is the word I would use to describe my career. I have been very fortunate in the people I have known, in the scientific opportunities I have had, and in the support and encouragement I have received from my family. I owe a great debt of gratitude to the people I have worked for and with through the years, as well as the people whose work has inspired and guided me, and those whose peer review has improved my own work. Here I’m going to highlight just a few of these people (see Figure 2), but there are many others I could expound on if space and attention span were not constrained.

It was my great good fortune to have James Van Allen as my graduate advisor. I learned many things about space science from him, but I also learned some very important lessons about being a scientist. Van Allen held a weekly team meeting with his postdocs and students, each person describing their current progress and difficulties, and in these meetings I came to appreciate what a wonderful person he was. When one of the students would raise unsupportable or illogical proposals, Van Allen would gently puff on his pipe (these were the days when indoor smoking was still allowed) and say, “Now just a minute there.” And then he would respectfully point out the error and steer the study into a more productive direction. Van Allen always treated everyone with the same level of respect, from famous colleagues to the students in his freshman astronomy class. His graduate students were the heart of his research team, and he guided them with wisdom and kindness. It was indeed a privilege to have studied with him.

One of the participants at Van Allen's weekly team meeting starting in 1973 was Christoph Goertz, a research professor recently arrived from South Africa. Chris was an accomplished theorist, full of great ideas, and he became my co-advisor for my Ph.D. thesis. After I graduated, I stayed on at Iowa for a 3-year postdoctoral

Figure 2. Mentors/colleagues/friends: (a) Christoph Goertz, (b) Peter Gary, (c) Bill Feldman, (d) Jack Gosling (with the author). Photo credits: (a) University Relations Records, Special Collections and Archives, The University of Iowa Libraries, (b) Courtesy of Space Science Institute, (c) Courtesy of Planetary Science Institute, and (d) the author's personal archives.
appointment (which allowed me to participate in the Pioneer 11 Saturn encounter and subsequent analysis), and Chris and I developed a productive working relationship and personal friendship. It was brainstorming sessions with him that helped me realize how fun the space science enterprise could be. His creativity, insights, and energy were a real source of inspiration. About a decade after I had moved to Los Alamos, a disgruntled student murdered Chris as he led a meeting of his own plasma research team in the University of Iowa's physics building. In a horrific act that stunned our entire field, five people were deliberately shot to death, including Chris, his colleagues Bob Smith and Dwight Nicholson, a fellow graduate student, and an associate vice president of the university. With the loss of this talented mentor, colleague, and friend, some of the light in my professional life was extinguished, and our field has been the poorer for the loss.

My move to Los Alamos in 1981 brought new and wonderful mentors into my life. I was originally hired by Peter Gary to work on ionospheric instabilities. Peter was irrepressibly enthusiastic about physics and a bottomless reservoir of knowledge about plasma instabilities. He was a true “gentleman and scholar,” and it was a delight to work with him. He was a treasured colleague and friend, and his death in 2021 was another great loss to our field.

After about a year working with Peter, I realized that theory wasn't really my passion; rather, I really wanted to work with data, to learn not just what might be, but what actually is. Luckily, analysis of data from the Los Alamos Fast Plasma Experiment on the ISEE-1 and ISEE-2 spacecraft launched in 1977 was just hitting its stride at LANL, and I was swept up in the enthusiasm and the excitement of new findings. My participation was welcomed by my LANL colleagues, and I worked closely with Bill Feldman and Jack Gosling to study the physics of the Earth's bow shock and foreshock. Bill was (and is!) an absolute delight, so full of ideas and fervor and joy. It was wonderful to work with him, but not too much later he switched his allegiance to his new love: neutron measurements and planetary physics. Such migrations were enabled by the size and diversity of work at Los Alamos, and I think it is one of LANL's greatest strengths.

Jack Gosling was my closest collaborator at Los Alamos. I co-authored 67 papers with him. Jack was an observer's observer, dedicated to understanding nature by what it reveals to us through remote or in situ observations. He was a master at recognizing new features or patterns in data (he called it “bird-watching”), assimilating them into an interpretive framework, and communicating the results. The extent of Jack's scientific impact is due, I think, to his single-minded devotion to good science, and most importantly, to his ability to home in on the really important problems for study. His favorite question, posed to himself as frequently and aggressively as to other scientists, was “So what?” When you think about it, this should be the mantra of every working scientist because it draws us to focus on the significant and not waste our energy on the trivial. In my mind, the most important aspect of Jack's career was integrity. I know few people as committed to scientific excellence as Jack, in his own work and in the work of others. Everyone who has written a paper with Jack as co-author has certainly experienced his honest (and sometimes painful) critiques of our work. We even had a term for the treatment: “Goslingation.” The remarkable part of this treatment, however, is that every one of his suggestions clarified and improved the analysis and presentation. I don't think I ever overrode one of Jack's suggestions.

In addition to these mentors, I have had the privilege of working with many other wonderful colleagues, postdocs, and students. I hope they know my appreciation for their openness, willingness to collaborate, mutual respect, friendship, promotion of others, and kindness. I have also along the way encountered a few bad apples, who likewise shall go unnamed but whose competitiveness, envy, secretiveness, bitterness, superiority, and self-promotion have temporarily soured the endeavor.

4. Some Lessons

From all of these people I have learned important lessons:

First and foremost is that a passion for science is the alpha and the omega. Fancy analysis techniques or graphical displays are great, but never lose sight of the real target: What is nature doing here? Jack Gosling's question “So what?” should be ever in our mind.

Second is the importance of honesty and integrity. These are the foundation of trust in science. If you lose that trust, your work as a scientist is totally in vain.
Third is publish or don't bother. If you don't communicate what you have learned, you might as well not have done the work.

Finally, remember with humility that the work you do is made possible by the work others have done and are doing. Those of us who work with spacecraft data are particularly indebted to the people before us who have conceived of each mission, designed and developed the instruments, built the spacecraft, and mounted the mission. Space science is a grand endeavor in which it is our great privilege to participate.

Data Availability Statement

Data were not used, nor created for this research.

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