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The Rest of the Solar System

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Abstract

Should one call it serendipity to have stumbled into a career in space plasma physics within the first decade of the era of satellite exploration? The author had the good fortune to have done so. In early years, she repeatedly was told that she did not look like a physicist, but it was physics that provided her a rewarding opportunity to elucidate the characteristics of the space plasmas of terrestrial and planetary magnetospheres and to discover unexpected properties of the Galilean moons of Jupiter. Here, she describes some of her scientific contributions and introduces family members, colleagues, students, and friends who helped her along her trajectory and participated in her scientific investigations.

INTRODUCTION

Writer's block inevitably attacks as the first words of a paper appear on my computer screen (often to be removed, replaced, and later restored), whatever the subject. The attack this time is more virulent because the subject is not just space plasmas but also my scientific career. Yet in a moment of lapsed judgment I agreed to provide an article for Annual Reviews, and so I plunge ahead hoping that my readers will find something of interest in both subjects. It may seem ironic for me, a space plasma physicist, to be asked to write an introductory review article for a journal on "Earth and Planetary Sciences." I try to understand the properties of systems filled with almost nothing (space plasmas are often described as dense when particle densities reach a few thousand ions-cm^{-3}), in a highly ionized state and organized by electromagnetic interactions, whereas most of the interesting properties of the bodies of the Solar System must be understood in terms of truly dense and still denser, electrically neutral, and organized by gravity and thermal gradients. Nonetheless, magnetized plasmas link the Earth and planets to their surroundings, and ultimately back to the sun, and carve out of the galaxy the portion that can be identified as our Solar System. Over eons, the charged particles of space plasmas have, for example, created comet tails, modified surfaces of moons, contributed to the evolution of atmospheres, generated electromagnetic radiation pulsing at planetary rotation periods, and produced dynamic auroral emissions. Thus it is not inappropriate to encourage the view that space plasmas are an important part of the story of Earth and planetary science.

I did not choose to be a space physicist. However, I was lucky enough to stumble into the field during its scientific infancy, when the many fundamental processes that link different plasma regimes, accelerate particles to relativistic energies, and produce natural phenomena such as aurora and geomagnetic activity were first being explored in situ. I can take credit only for saying "yes" when offered opportunities to contribute to space science. Involvement in spacecraft missions, interactions with colleagues well versed in the fundamentals of the field, and exposure to clever students provided stimulation and challenges that gave me remarkable opportunities to participate in unravelling some of the mysteries of space. Invited to write about my career and my science, a chronological approach will guide, but not constrain, me, so jump ahead if you are looking for the promised ruminations on the planets and their tenuous plasma environments.

EARLY YEARS

My life began shortly before the collapse of an economic bubble diminished expectations for most; it has spanned years of international turmoil and warfare, but through it all, I have been enviably fortunate. My girlhood was spent in New York City, a city that throbbed with activity and provided endless opportunities for entertainment and education. Surrounded by a loving family that was for long the center of my life, I loved school, summer camp, and the friends in both places. I was happy to travel during holidays, often to visit my father's relatives in Wilkes-Barre, Pennsylvania, and sometimes to experience rugged natural surroundings that contrasted with the concrete sidewalks and manicured parks of the city. Children of my youth had far fewer toys, lessons, and diversions than do children today, but I do remember going to plays and movies, museums, and historical sites, and ending the busy days engaged in lively dinner table conversations with my father, my beautiful mother, and my closest friend, my younger sister Eleanor. The pall of the depression was remote to a child growing up in a middle class professional family, but not the early rumblings of World War II. My Jewish family was very early sensitive to what Hitler was contemplating and worked to save relatives abroad. How different was my life during the war than it might have been had I been one of them.

With a physician father and a mother who had studied physics at a university that included Planck and Einstein on the faculty (Galland 1999), I may well have been destined to study science. However, I do not recall any exceptional interest in the subject until nearly the end of my high school career. Was that because general science was, and still often is, taught badly? I am not sure, but I do know that math, in which I excelled, became great fun when we began to study advanced algebra and calculus, especially when our teachers challenged us with extra credit problems. So I suppose that my direction was established well before I recognized what I would specialize in.

RADCLIFFE DAYS

My all-girls school emphasized lady-like behavior and expected students to attend one of the Seven Sisters colleges or equivalent, so I applied to Radcliffe and Wellesley, and was accepted at both. The decision was not obvious. Wellesley had a beautiful campus and the enthusiasm of the students and faculty appealed to me when I visited the campus. But then I examined the catalogues of courses for both colleges. Discovering that Radcliffe classes were actually taught by Harvard faculty and that classes, at least in the upper division and beyond, were coeducational, I can't honestly say whether it was because there were so many graduate science courses offered by Radcliffe or the thought of the Harvard men on the campus that led me to accept their offer. I entered Radcliffe in the fall of 1946, just in time to join the first Harvard class filled with veterans of the war. Possibly it was the presence of older students, or even their numbers, that prevented Harvard from returning to the prewar system of segregated classes for women. I remember my first introductory physics lecture being presented on the Radcliffe side of the campus to five or six women students. At the end, the professor announced, "Today we are meeting at Radcliffe at 11 AM. On Wednesday, you will meet in Jefferson at 10 AM." On Wednesday, we found that our arrival increased the size of Harvard's physics class by 1 or 2%. As I continued to the more advanced classes, the ratio remained unchanged. I was usually the only woman in the class.

I studied physics and math, took a history course in which I did not perform with distinction, and tried philosophy and sociology, which were fun. Nonetheless, I soon realized that I was getting my greatest pleasure from my science courses. Two developments critical in setting the path of my life occurred in the spring of my freshman year. First, I was asked to select a field for my major. I decided on physics after attending a Radcliffe tea party (I suspect the men went to a beer fest) at which professors of math and physics described the excitement of their fields. It was Wendell Furry who convinced me that physics would be fun, mainly by showing magical effects that could be produced by playing with two sheets of polarized glass and convincing me that I could learn to understand why. Even more important was the second event. I was introduced to Daniel Kivelson, a Harvard sophomore who stole my heart on our first date. I think it was mostly his dazzling smile, but it did not take long for me to confirm that the smile was only part of his charm. He had an incisive, original mind; a gentle personality; high principles; cared passionately about making the world a better place; and was great fun to be with. In today's language, Daniel and I became an item immediately, although we waited until the summer following my junior year to marry (at age 20). Daniel, also a scientist, had acquired a group of extraordinary friends who welcomed me into their circle. The opportunity to penetrate the mysteries of physics and math by working alongside of fellow students (few Radcliffe classmates were available to help me understand these subjects) was critical to my learning experience, and it was definitely encouraging to become part of a group keen on science and politics, food and alcohol, and literature and art.

Ph.D. STUDIES UNDER JULIAN SCHWINGER

Daniel's graduate studies in Chemistry (with E. Bright Wilson, Jr.) began during my senior year, so it was convenient and appealing to remain at Harvard for my graduate work. The most exciting breakthroughs of the era were occurring in quantum electrodynamics, so I had the temerity to ask to work with Professor Julian Schwinger, a remarkable scientist and later a Nobel laureate. I am not sure that at the time I recognized what a privilege it would be to become one of his students, but, having done so, I found myself a member of a dynamic and supportive cohort, roughly 13 strong and now a veritable Who's Who of physics, from whom I learned as much as from my remarkable mentor. (Schwinger mentored more than 70 Ph.D. students during his illustrious career at Harvard and UCLA, but I believe that I was the only woman.)

Schwinger used his courses to develop cutting edge tools of analysis and never taught a course the same way twice, so his students found themselves attending such courses as quantum mechanics repeatedly to keep up with his new ways of looking at the material. One year all of quantum mechanics seemed to have been reduced to projection operators, with the Schroedinger equation coming out as a special case in what I remember as the last lecture. Another memorable classroom moment was the characterization of a solution as the "generating function of the well-known Gegenbauer polynomials." I was not the only student to question the implied familiarity. Yet despite the intensely mathematical focus and the esoteric material, the underlying lessons of the importance of symmetries, conservation laws, and rigorous and economical reasoning proved extremely portable when later I moved from one branch of physics to another.

My thesis on *Bremsstrahlung of High Energy Electrons*, completed in 1957, comprises some 60 pages of mathematical manipulation from which emerged an expression for the cross section valid for forward scattering to all orders in the Coulomb interaction.

Sadly, I never published the results because I found that the issue of *Physical Review* that reached the library shelves on the afternoon of my final oral exam included a paper (Olsen et al. 1957) that used a very different mathematical approach to obtain the same result that had emerged from my lengthy analysis. I was then under the impression that one did not publish results that confirmed previous work, even if they simultaneously introduced a new analysis method. Professor Schwinger was too engaged in his own work to notice that I never submitted a paper. I should have done so.

My life changed in important ways during my years of graduate studies. In 1954, my son Steven was born, roughly at the time that Daniel finished his own Ph.D. work and began teaching at MIT (as an Instructor at a laughably small salary). A year later, Daniel accepted an appointment at UCLA and we headed to Los Angeles, temporarily we were sure because we were far too sophisticated to want to remain in the land of lotus eaters. Daniel had chosen UCLA over other offers partly because there were many job possibilities for me in the aerospace industry, which at the time was desperate to hire trained scientists. His decision was made well before the issue of two-career families became central to the lives of young professionals and was both forward-looking and generous. He greatly wanted me to have a successful career, partly because he had seen the frustrations experienced by his gifted mother, Eva Kivelson, whose training as a physician had been used only intermittently and never very seriously.

AT THE RAND CORPORATION

I soon found a job as a consultant at the RAND Corporation, where I was given a flexible schedule (more or less half-time) and encouraged to finish my thesis before moving into significant additional research. The thesis was completed at long distance, but fortunately I had formed a pretty clear idea of what I was doing before I left Cambridge for the Wild West. My daughter Valerie was born in 1957, just months after I had completed my degree.

The highlight of my years at RAND was the opportunity to work closely with Don DuBois, with whom I wrote several papers that are still being cited more than 40 years later. The papers dealt with collective interactions in plasmas and electron gases by applying mathematical techniques that related closely to those used in quantum electrodynamics. In particular, we derived a useful correction to Landau's relation for the damping of excitations in an unmagnetized plasma (DuBois et al. 1963). That we omitted effects of the magnetic field seems ironic because I would later, rather accidentally, begin to specialize in the physics of magnetized plasmas.

At RAND, I also learned something about the sciences of the environment from Jack Welch, an Air Force officer with a physics degree. Jack was posted at RAND for two years to serve as an interface between the customer for RAND studies and the suppliers. He had a good, practical understanding of a vast range of physical phenomena and was writing a book on gas dynamics with J.W. Bond, Jr. and K.M. Watson (Bond et al. 1965). Jack asked me to read the manuscript and comment on it, and that effort was not only useful to him but also greatly broadened my scientific knowledge. Together we wrote a cute paper (Kivelson & Welch 1968) on radiation smoothing of shocks (showing how energy radiated from the heated gas smoothes the shock discontinuity) that has never been cited. Optimistically, I suggest that this is because we published it in the proceedings of an obscure conference.

Sabbaticals are a remarkable feature of academic life that are not available to researchers who do not have faculty appointments. I was lucky enough to have a husband motivated to take advantage of the opportunities provided by his faculty prerogative and I managed to find ways to join him in spending time in homes away from home. Daniel's first sabbatical, in 1959, took us to Paris. With two small children, I took the only nonworking leave of my career and used the time to fall in love with Paris and France and to learn much from the richness of culture accessible there. Later sabbatical destinations were chosen so that we both could pursue our scientific interests. Daniel's second sabbatical leave in 1965–66 was spent at MIT. I took leave from RAND, supported by a fellowship from the Radcliffe Institute for Advanced Study (yes, that was its official name). The Institute has changed its mission several times since its early years, but when I was there, its purpose was to give an opportunity for research, writing, painting, or whatever was relevant to women with academic interests who had been sidetracked by family or other responsibilities and wished to return to a serious career. Stipends were small but came with a title, office space, and encouragement to interact with scholars both at the Institute and on the Harvard faculty. I attended Paul Martin's graduate class on condensed matter, went to numerous seminars, talked physics with my Harvard and MIT colleagues, and interacted with a remarkable group of Radcliffe Institute fellows. I began work on a paper on rotational relaxation in fluids with Daniel and our friend and colleague, Irwin Oppenheim (Kivelson et al. 1970). The most important development of the year was that I recognized that my interests and abilities were better suited to a university environment than to RAND, so when I returned to Los Angeles I campaigned to get an appointment at UCLA.

UCLA AND DISCOVERING SPACE PHYSICS

A faculty appointment at UCLA seemed out of my reach, but Willard (Bill) Libby (later also a Nobel laureate) was well funded to support NASA-sponsored research through the Institute of Geophysics and Planetary Physics, and I was happy to transform myself into an Assistant Research Geophysicist. A chemist, Bill seems not to have recognized that the work I had done on plasmas was peripheral to the problems of the magnetized plasmas of interplanetary space when he hired me to work with two students who were somewhat adrift in his research group. One of the students, Dale Barry, was working on the wave measurements from the Canadian *Alouette* spacecraft, and I fear that I was never really helpful to him. However, I was helpful to the other student, Robert G. Wilson, and I became fascinated by the problems on which he was working; many of my later interests date back to his thesis topic. Bob was using data collected by Jim Warwick's Boulder Radio Spectrograph to explore the Io control of decametric emissions from Jupiter. Thus, early in my UCLA years, I met one of the more colorful figures in the study of the properties of the outer planets.

Both of Libby's students came regularly to discuss their work with me. They phrased their questions in the jargon of the field of space physics with which I was largely unfamiliar. I would listen attentively to their accounts of "particles conserving μ and J" and possibly "interacting with whistler waves." I would then ask them to return the next day to give me a chance to think about their questions. As soon as they left, I would run to the library to read a bit more about space plasmas so that I could figure out what they were talking about. Before long, I began to understand their concerns and could occasionally answer their questions without the help of the library. I must have been somewhat useful to Bob because his thesis thanks me for my "many delightfully creative and evaluative discussions." I hoped that creative was not a proxy for nonsensical.

Once Bob completed his Ph.D., Bill Libby arranged a lateral transfer that shifted me into Paul Coleman's space physics group at UCLA. I was to begin working for Tom Farley, who had provided electron spectrometers for the Earth-orbiting OGO-5 and -6 missions and was beginning to interpret the data being collected. I was given the assignment of using the OGO-5 energetic electron data to interpret the dynamics of Earth's magnetosphere. In those pre-PC days, I found it useful to corner the market on 5 $\times$ 3 decade log-log graph paper so that I (later a student helper) could plot the measured spectra, first for the purpose of instrument calibration and later for scientific analysis. Tom was a patient and knowledgeable teacher who helped me fill in the gaps in my background.

It was not long before scientific results began to emerge from the OGO-5 data. Of particular note were papers coauthored with Chris Russell and Michel Aubry, a postdoctoral visitor from France (Aubry et al. 1970, 1971), that gave direct evidence of the inward motion of the dayside magnetopause in response to magnetic reconnection and inferred the amplitude and wavelength of Kelvin-Helmholtz waves on the dayside magnetopause. In the event examined in these papers, the magnetopause moved Earthward during an extended interval of constant solar wind dynamic pressure. The gas-dynamic limit does not predict motion of the boundary for such conditions. However, the solar wind is magnetized and concurrent with the inward motion of the boundary, the interplanetary magnetic field rotated from northward-pointing to southward-pointing, suggesting that magnetic reconnection at the day side must have peeled off the outermost flux tubes. At that time, magnetic reconnection was poorly understood and there were those who believed that the process could not occur. The Aubry et al. (1970) paper provided one of the observational pillars that ultimately convinced most of the space science community that Jim Dungey's prescient description of the magnetosphere's response to the orientation of the interplanetary magnetic field (Dungey 1961) through the mechanism of magnetic reconnection must be valid.

In the early 1970s, space plasma physics was an emerging discipline. UCLA was fortunate to have recruited some of the most active members of the developing space science community. We often joked that the concentration of scientists in our field was an accident that had occurred only because no one in the administration noticed how many departments were hiring space scientists. In addition to those of us in the Institute of Geophysics and Planetary Physics (Paul Coleman, Bob Holzer, Bob McPherron, Chris Russell, and me), there were Charlie Kennel, Ferd Coroniti, and Mike Cornwall, and soon also Maha Ashour-Abdalla, in Physics, and Richard

Thorne and George Siscoe in what was then Meteorology. This concentration of talent attracted many visitors, both postdocs and senior scholars, and the spirit of collegiality was notable. It seems to me that in those days we spent more time talking about science and less time working on administrative problems or running to airports than we do today, but that may well be a sign of my age. Among those who came for short or long stays were Keith Runcorn, Jim Dungey, Valeria Troitskaya, and a few other Soviet scientists. Of greatest consequence for my future scientific career was the arrival of David Southwood, a recent Ph.D. from Imperial College, London, who spent a year in 1970–71 as a postdoctoral visitor with Charlie Kennel. During David's visit we started a conversation that has not yet run out of subject matter and that developed into a life-long collaboration.

A YEAR AT IMPERIAL COLLEGE, LONDON

Two years later, another sabbatical was on the horizon. To me it was clear that the most exciting place to spend a year would be in Jim Dungey's group at Imperial College where I would be able to learn from Jim and work with David. Daniel, ever the supportive husband, arranged to visit John Rowlinson's lab, also at Imperial College. I boldly applied for a Guggenheim Fellowship to support my adventure, truly not expecting it to come through. I suspect that the fact that Bill Libby was on the selection committee was helpful, although one never knows. In any case, to my delight, I was awarded a Guggenheim. That fellowship gave me for the first time the sense that I was being taken seriously as a scientist. More than money, it gave me status and increased my self-confidence considerably.

For me, the year in London (1973–1974) was immensely rewarding. Dungey's group was as lively as I had anticipated. David was by then on the faculty as was Stan Cowley. Maha Ashour-Abdalla was just completing her Ph.D. and was off to France to carry out her important work calculating wave growth of electrostatic instabilities (Ashour-Abdalla et al. 1975). Jeff Hughes was working with David to understand how ultralow frequency (ULF) waves are modified by ionospheric conductivity (Hughes & Southwood 1974). Chris Green, analyzing ground magnetometer data, was able to identify a coastline effect and to detect azimuthal wave phase propagation (Green 1976), observations that enabled Dungey & Southwood (1975) to work out the nature of the Poynting flux in ULF waves.

Unexpectedly, my year in London initiated a lifelong friendship with a Soviet colleague. In October, Valeria Troitskaya arrived at Imperial, bearing rolls of magnetometer records. For years I had been listening to mind-deadening talks describing magnetic signatures of ULF waves. I was not at all interested in hearing more about wiggles on charts, but when Valeria started explaining what she saw when she examined the data, I became a convert. Valeria used the spatial and temporal distribution and spectral characteristics of wave power to infer probable sources within the magnetosphere (e.g., Jacobs et al. 1964), an approach not far different from that used in today's helioseismology. She demonstrated that wave properties provide a powerful tool for probing magnetospheric processes, and soon I was making my own contributions to this area of research.

During the week of her visit, Valeria shepherded Daniel and me to a number of cultural events, she being eager to view the films (such as Tarkovsky's *Andrei Rublev*) and TV programs that were banned in the Soviet Union. The three of us found ourselves glued to the news programs reporting on the progress of the Yom Kippur war, which unfolded during that week. Each day I clipped articles from the *International Herald Tribune* for Valeria to smuggle past the inspectors and share with family and colleagues.

The sabbatical did not work out quite as anticipated for Daniel because, within a few months after our arrival, John Rowlinson moved to Oxford. Daniel was left without collaborators close by, but, nonetheless, did manage to start work on important papers (Kivelson & Madden 1980) with Paul Madden who came to London frequently. However, for me, the planned interactions with David developed better than I anticipated. We began work on a series of papers that reformulated the theorist's view of magnetospheric plasma behavior in terms of directly measurable quantities. Theorists, for example, like to describe the trajectories of charged particles moving at constant values of the first two adiabatic invariants (i.e., the magnetic moment, μ, and the bounce invariant, J). They can then show that the trajectories of particles moving through the equatorial magnetosphere fall into two classes, with a boundary called an Alfvén layer separating trajectories that extend from the magnetotail to the dayside magnetopause in the outer magnetosphere from trajectories that close around Earth in the inner magnetosphere. The problem for the data analyst is that neither μ nor J is directly measured by spacecraft instruments. Applying the existing theory to the interpretation of sharp boundaries in available spacecraft measurements made at fixed energy and pitch angle was either extremely cumbersome or not possible. Our formulation of the structure of the Alfvén layer was pragmatic; we developed equations that describe a related structure, a boundary implicit in the Alfvén layer model but one across which the fluxes of particles at fixed energy measured on a near equatorial spacecraft would, for most conditions, change abruptly (Kivelson & Southwood 1975). This formulation enabled us, for example, to develop interpretations of fronts of energetic particles accelerated during magnetospheric substorms and injected into regions near geostationary orbit (Kivelson et al. 1979a).

PIONEERS 10 AND *11*

Shortly after my return to UCLA in the fall of 1974, Paul Coleman, who was a co-investigator on the *Pioneer 10* and *11* magnetometers, asked me if I would like to take responsibility for leading his group of researchers studying Jupiter. He warned me that if I agreed, I probably would not have a free moment for years to come. He was right in his prediction, but, nonetheless, I said yes. And in that portentous instant, my commitment to the study of the Jupiter system was sealed.

As my first official duty, Paul sent me to NASA Ames Research Center to participate in the activities related to *Pioneer 11's* flyby of Jupiter (December 2, 1974). For the first time, I experienced the intensity of a big mission: waiting for signs of an encounter with Jupiter's bow shock; the first entry into the magnetosphere; the resumption of communication when the spacecraft emerged from some 20 min of silence as it passed behind Jupiter. I watched the activities of the magnetometer groups and of other groups. I saw signs of lively cooperation in Jim Van Allen's group where students and postdocs worked in what seemed like pleasant harmony beneath the benign visage of their mentor. Other instrument teams seemed to show signs of stress, and competition often dominated cooperation between teams. Still, the success of the challenging encounter and the fine quality of the data left me ebullient.

My principal collaborator on *Pioneer* studies of the distant solar wind and the Jovian magnetosphere was Ron Rosenberg, an eccentric scientist who could pour over pages of IBM printout and discover subtle patterns in the numbers. With Paul Coleman, he had studied the magnetic polarity of the solar wind magnetic field. The polarity of the interplanetary magnetic field had been shown to reverse $2n$ times per solar rotation, breaking the solar wind into sectors. The sector structure of the solar wind had first been thought to arise from sources of differing magnetic polarity organized on the solar surface like orange segments. Rosenberg and Coleman gave evidence that the polarity reversals occurred across a distorted heliographic equator, with the dominant field directions in each hemisphere being dictated by the polarity of the solar magnetic dipole moment. This important result was later confirmed and credited to others when the description of a "ballerina skirt model" became popular, but there is little doubt that the model was first described by Rosenberg & Coleman (1969).

My newly acquired enthusiasm for learning about the magnetosphere from properties of ULF waves led Ron and me to investigate the properties of such waves in the Jovian magnetosphere. We found that, as in the environment of Earth, the character of the waves changed from one part of the system to another. Compression dominated in regions that we knew to be the equatorial plasma sheet and in an outer boundary layer just inside the dayside magnetopause. I wrote a paper (Kivelson 1976) on Jupiter's distant environment arguing that there was evidence for the persistent presence of a layer of magnetic turbulence just inside the magnetopause. When presenting this work at a meeting, I was called to task by Norman Ness for using the term turbulent without evidence of the spatial structure of the fluctuations; although he was technically correct, I suspect that most of my readers understood that I was referring to large amplitude fluctuations with a broad and undifferentiated power spectral density. The abstract of this paper concluded with the statement, "The importance of the presence of the turbulent layer to theoretical models of the magnetosphere is stressed," and many years later David Southwood and colleagues analyzed the isolated magnetic nulls that produce large fluctuations of field magnitude in the *Ulysses* data taken near Jupiter's dayside magnetopause. They proposed that the nulls appear where bits of hot plasma are torn off the outer edge of the plasma sheet (Southwood et al. 1993, Haynes et al. 1994).

PROFESSOR AT LAST

In 1975, my unconventional career moved into a new phase when I was given a half-time appointment as associate professor-in-residence in the Department of Planetary and Space Science, which soon renamed itself the Department of Geophysics and

Planetary Physics. In the other half of my time, I remained a researcher in the Institute of Geophysics and Planetary Physics (IGPP) for several more years. I give all these details because only academia could come up with the subtle distinctions that the various names imply. The professor-in-residence title does not convey tenure but does allow the faculty member to teach classes and guide graduate student research. With an in-residence title, my foot had slipped under the tent and I continued to push steadily. In 1981, I gained tenure and a full professorship for my half-faculty appointment in the department that, in the intervening years, had merged with Geology and become Earth and Space Sciences (ESS). A faculty position in IGPP was provided only in 1983. Even though tents don't have back doors, I often say that I came to the faculty by the back door, slipping in slowly and stealthily. It was an unusual trajectory for a faculty member, but one that allowed me to reach my career goal over a much longer period of time than is allowed to those who start in tenure-track positions. I used that extra flexibility to spend some time with my growing children, so I have no complaints.

My irregular academic status notwithstanding, I had for some time been receiving invitations to serve on university committees and to participate in assorted extramural activities. On the university front, my contributions were overwhelmingly linked to the issue of equitable treatment of women as students, staff, and faculty. Soon after coming to UCLA, I became a member of the Association of Academic Women at UCLA, for which I served in various positions, including President in 1977–78. The Association gave me a platform that enabled me to needle the administration on the issue of unequal treatment and led to my being appointed to the Chancellor's Advisory Committee on the Status of Women (CACSW) when the passage of Title IX legislation demanded that the university take official action. It is hard to believe how blatantly women were excluded from privileges, fair salary, and even faculty appointments at that time. In investigating inequities in the system, our committee found that marriage to a male resident of California entitled a previously nonresident woman student to resident status for tuition purposes. On the other hand, marriage of a previously nonresident male student to a resident female removed both from resident status. We found job descriptions, publicly available, that announced identical qualifications for "female bookbinder" and "male bookbinder" but reported a higher salary for the latter. Women students were still subject to special oversight from the Dean of Women. Believable reports informed us that renowned women candidates had been rejected without consideration in a department whose all-male faculty did not want to destroy their camaraderie. This was the era when Maria Goppert Mayer, soon to be awarded the Nobel Prize, was unsuccessfully put forward for appointment to the faculty in Physics. The early reports of the CACSW generated generally useful responses, more from the administration than the faculty. Blatant discrimination became politically incorrect, but attitudes do not change overnight, and I continued to participate in groups that urged UCLA to establish a program in Women's Studies (they did, one of the first in the nation) and that encouraged more women to enter careers in science and to remain in them.

Outside of UCLA, I began to serve on government advisory committees. I greatly enjoyed being a member and later Chair of the Advisory Committee to the Division of Atmospheric Sciences of the National Science Foundation, to no small extent because the folks at NSF really wanted to take the pulse of the research community that they supported. My role on committees of the Department of Energy gave me a chance to state my views to my old friend John Deutch and to know that he had to listen even if he didn't follow the advice.

In 1977, I was elected an Overseer of Harvard College, my name probably having been put forward because there was a push to diversify this august body. A few years earlier, Harvard had begun to include one or two women among ten nominees for five Overseers selected each year, so there were already a few women on the 30-member Board. In those days, no woman nominee failed to be selected. (I often remarked that I would recognize progress toward equal treatment when the ballot listed so many women that it made sense not to vote for all of them, and that has finally happened.) Serving as an overseer of Harvard College from 1977 to 1983 was truly rewarding. Our role was restricted to giving advice and approving the appointment of a new President. This sounds very limiting, but in seeking advice, the Harvard participants were often challenged to rethink their plans. During my years on the Board, Overseer reactions to issues linked to strained relations with the Cambridge community and to possible venture capital investment in faculty-led companies seemed to me to have encouraged the university to modify some of its actions. Without any real power, we still felt useful.

Overseers are asked to serve as chairs of Visiting Committees and I found that part of my responsibilities challenging and rewarding. I discovered that an important aspect of a visit is that it activates self-examination within the department. Many times problems had been identified and resolved before we arrived on the scene. During the meetings, we had the opportunity to talk to everyone from students to faculty and I was amazed to find how frankly people within a department air their concerns to total strangers. I believe that a department can benefit from the way that an objective Visiting Committee effectively amplifies messages to the administration regarding problems and/or successes and that such committees provide insight helpful in the allocation of resources.

At the end of their six years on the Board, Overseers are invited to make a statement of their views and concerns for the university. I directed my parting remarks to the imbalance I saw between efforts in science/technology and the other aspects of intellectual endeavor. I urged Harvard to place greater emphasis on the scientific enterprise and to consider the establishment of a school of engineering to balance the strong role of such professional schools as business, law, and public administration. Years passed without any relevant action, but recently I have read of changes that appear to be heading in the direction that I encouraged.

While serving as Overseer, I was also supervising my first Ph.D. students, and still my good friends, Stan Kaye and Howard Singer, excellent students who became leaders in their fields. Stan and Howard both wrote theses that included considerable analysis of the newly available ISEE 1 and 2 data provided by Chris Russell who was the magnetometer Principal Investigator (PI). Chris shared his data generously, an approach that remains rare even today. I believe that he is motivated by his deep desire to understand more about the magnetosphere and the solar wind and a conviction that the goal will be reached most effectively if many people analyze the data. I learned a great deal about the value of scientific generosity from Chris.

Stan and Howard examined properties of magnetospheric ULF waves and their interaction with charged particles. Their theses included theoretical sections on which we collaborated with David Southwood. Singer et al. (1981) investigated the structure of Alfvén waves in a realistic (nondipolar) field and provided tools for understanding how ULF wave frequencies depend on the structure of the background magnetic field. The work was formulated so that it can be applied to any field model, and is being used today in work relating resonant frequencies to mass content of plasmaspheric flux tubes (Berube et al. 2006).

GALILEO: BEGINNINGS

Opportunities to learn more about Jupiter's magnetosphere and its moons became concrete when, in 1976, NASA solicited proposals to provide instruments for the Jupiter Orbiter Probe mission. I became the PI for the UCLA magnetometer proposal, with Paul Coleman, Chris Russell, Bob McPherron, and Charlie Kennel as co-investigators. I had never written a proposal for a spacecraft mission, but my team and our chief engineer, Bob Snare, and his associates were old pros and showed me the way. We decided to propose investigations both for the orbiting spacecraft and for the probe to be dropped into Jupiter's cloud tops. That decision was fortunate because proposals for the probe, on which a magnetometer was not a priority investigation, were due roughly a month before the deadline for orbiter proposals and I was able to practice on the first (not selected) and refine the second proposal. In 1977, our team was awarded the coveted opportunity to provide the magnetometer for the orbiter on what ultimately became the *Galileo* mission and I started a new phase of my career.

The Jet Propulsion Laboratory (JPL) became an important part of my life and a series of instrument scientists from JPL, meeting with us weekly, helped us keep track of deadlines to be met, tests to be run, interface requirements to be implemented, etc. My language expanded to include endless acronyms and I soon became comfortable with submitting ECRs (engineering change requests) and turning acronym-nouns into verbs (as in "we will have to ECR that change of voltage"). I found that being a PI on a large mission required not only a science and engineering background but also the ability to negotiate with other teams in designing the mission. Orbit planning was sometimes contentious. Orbits that enabled one set of investigations to acquire important data could be all but useless for some other set of investigations. Fields and particles investigators, recognizing that important dynamical processes must occur on the night side of the magnetosphere, deep in the magnetotail, were eager to include at least one long looping orbit centered near midnight. For imaging instruments, such an orbit, on the antisunward side of the planet and lasting for months, was all but useless. There was pressure to shorten the night-side orbit by moving apoapsis closer to Jupiter, and I remember presenting the arguments for a minimum distance of 150 R_J (R_J = radius of Jupiter), which was adopted and approximately achieved on one orbit.

The mission lurched from crisis to crisis, as do many large missions. Early on, there was a time when it seemed possible that the costly *Galileo* mission would be canceled by Congress. There was no available upper stage booster rocket and it was not clear that one would be developed in time for the launch, originally planned for 1981 or 1982. Delays ensued, but the spacecraft was finally completed and shipped overland from California to Florida for a planned launch in 1986. While the spacecraft was awaiting launch, the space community as well as the rest of the world was shattered by the tragedy of the *Challenger* accident; space shuttle launches were canceled. The spacecraft was shipped back to California. Uncertain of the future, the *Galileo* scientists and engineers waited as the space shuttle was restructured and the rules for its use were changed to improve its safety. The changes reduced the maximum allowed lift load and constrained the types of upper stage boosters that could be launched into low Earth orbit. *Galileo's* ability to reach Jupiter was compromised by the changes. Creative analysis by the remarkable *Galileo* navigation team led to the proposal of a meandering interplanetary trajectory that could deliver the spacecraft to Jupiter in six years. The distinct drawback of the delay in arriving at Jupiter was somewhat compensated for by the opportunities provided by the indirect path to make measurements en route to Jupiter.

While JPL was modifying its plans for the *Galileo* mission, NASA had corrected critical problems with the space shuttle and was planning resumption of flights. Once again the *Galileo* spacecraft was trucked across the country and it seemed that it would finally be launched. October 1989 was the magic month. The magnetometer team, several with family, assembled in Orlando to cheer. My mother, daughter Valerie, son-in-law Tim, and granddaughter Rebecca joined Daniel and me. Launch windows for missions to other planets are of short duration, and delays are troublesome. Although the procedures went smoothly, weather problems developed both locally in Florida and at the alternative landing site in Spain. The launch was twice postponed; other responsibilities forced my family cheering section to leave, but on October 18 at 22:23 UTC, the UCLA magnetometer team was rewarded for years of patience as our spacecraft carrying our instrument was finally sent off on its tour of the Solar System.

The magnetometer was the first science instrument to be powered on. It acquired data while the long boom on which it was mounted extended to its full length of 11 m from the center of the spacecraft, the magnetometer confirming the successful deployment by recording the untwisting of the structure. We were off to other worlds. The path to Jupiter would take us by Venus once and by Earth twice; it would pass close to two asteroids, and would finally reach its target. The pass by Venus and the second pass by Earth approached from the antisolar direction and, quite fortuitously, grazed the distant bow shocks of both planets. Our measurements enabled us to study the asymptotic structure of the shock and to demonstrate that it had an asymmetric cross section readily understood in terms of the asymmetry of the fast mode wave speed in the solar wind (Khurana & Kivelson 1994, Bennett et al. 1997). The first flyby of Earth provided a rapid pass that sped toward the sun along the magnetotail and provided opportunities to study substorm dynamics in a new way. Two flux rope encounters inspired us to develop a mathematical model of a flux rope (Kivelson & Khurana 1995) in which thermal pressure contributions invalidated the usual force-free assumption that, within the structure, $\mathbf{j} \times \mathbf{B} = 0$.

The saga of *Galileo's* problems did not end with the launch. The high-gain antenna, designed to send precious data to the Deep Space Network from Jupiter's distance, had been folded like an umbrella during the first year of the mission to protect delicate systems from the high temperature and intense radiation encountered as the trajectory moved closer to the sun on the Venus flyby. Following the first flyby of Earth, *Galileo's* interplanetary trajectory continued beyond 1 AU, making it safe to open the large antenna. The command was sent, but the antenna opened only slightly, leaving it in a configuration as useless as a partly open umbrella. Repeated attempts to resolve the problem failed. Most likely, the multiple, unanticipated road trips across the country had damaged the delicate mechanism. Gone were dreams of relatively high-resolution data and tens of thousands of images from Jupiter orbit.

Once again the resilient team at JPL rescued the mission. The spacecraft had been provided with a small antenna that was needed for communication during the initial year prior to the planned transition to the high-gain antenna. Limited data could be acquired using this small antenna; although, even after modifications to the ground receiving system and on-board software, data rates would decrease by a factor of 1000 (from an initial 134 kbps). But on-board data compression could be provided and thereby effectively provide an order of magnitude improvement. One might wonder why data compression had not been used from the start. The reason was that, at the data rate originally intended, the limited capabilities of the on-board computers (designed in the late 1970s and already regarded as ancient technology when *Galileo* was launched) would have been fully exploited in acquiring and transmitting data. With a marked reduction of the data rate, computer processing time was liberated for use in data compression. Only during key intervals during the mission would data be acquired at the rate originally contemplated, stored on a tape recorder and slowly sent back to Earth. (Problems with the tape recorder are also part of the saga, but they, too, were solved by the team at JPL.) The low data rate available over much of the mission was disappointing not only because it constrained the kinds of scientific questions that could be addressed but also because it required the science teams to agree on revised plans for observations in a rather contentious environment. It is much easier to consider the needs of others when resources are plentiful than when they are in short supply. Fortunately, the magnetometer team found it easy to decide that our highest priority was continuity of the field measurements and that we would average our measurements over whatever intervals were needed to provide uniform sampling of the data between data downloads. At UCLA, Joe Means, our quietly effective data engineer, reprogrammed the tiny instrument control unit to provide averages over long time intervals. Over most of the planned orbits, our sampling rate would drop from 1/4 s to 24 s averages, but with data at this rate or even lower, it would be possible to monitor the large-scale dynamics of the system. The new software for data acquisition and instrument operation were to be uploaded only after reaching Jupiter.

To this time, my interests had not extended to the interiors of solar system bodies, but Galileo's flybys of asteroids (the first ever such close encounters) broadened my horizons. Magnetic rotations observed on close passes by the asteroids Gaspra in 1991 (Kivelson et al. 1993) and Ida in 1993 (Wang & Kivelson 1996) led our team to suggest that Gaspra may have a significant magnetic moment and that Ida is a conducting body. The possibility that the signatures were merely solar wind disturbances that fortuitously occurred during the close flyby could not be ruled out, even though we analyzed the remote solar wind and confirmed that the statistical probability of finding signatures that could be interpreted as asteroid interactions was quite low. Nonetheless, the results are not solid and it remains uncertain whether asteroids can have large-scale magnetic moments.

ANTICIPATING ARRIVAL AT JUPITER

Through the years of *Galileo* planning and instrument development, my research on Jupiter had continued. Based on *Pioneer 10* and *11* data, I had proposed a formula for the spatial structure of Jupiter's warped magnetospheric current sheet (Kivelson et al. 1978) with Lucien Froidevaux, a student visitor—son of Claude Froidevaux, himself an occasional UCLA visitor widely known for his work on lithospheric processes. Lucien went on to make his mark in stratospheric research. Ray Walker and I (Walker et al. 1978) demonstrated that the magnetospheric plasma sheet, within which the current sheet is embedded, is dominated by the thermal pressure of the energetic plasma, a feature soon taken for granted.

Taking notice of a paper by Neubauer (1978) that estimated the magnitude of dipole fields that might be present in the Galilean moons, Southwood, Slavin, and I had fun speculating on the magnetospheres that might form through interaction with Jupiter's flowing plasma (Kivelson et al. 1979b). We got many things wrong but we got some right for the wrong moon. For example, because we knew that the Io-modulated radio emission implied strong coupling between Io and Jupiter's ionosphere, we argued that Io's dipole moment would be roughly antiparallel to Jupiter's spin axis so that reconnection would link Io's magnetosphere to Jupiter's. Our arguments regarding Io turned out to be pertinent to Ganymede, where an internal field with the hypothesized orientation is present. We had been misled in our analysis of Ganymede because no one had found modulation of radio signals at Ganymede's orbital period, so, until years later when Hubble images proved otherwise, it seemed that Ganymede was not coupled to the surrounding plasma. For this reason, we proposed that, if magnetized, Ganymede's magnetic moment would be aligned with Jupiter's, a situation in which the magnetosphere would carve out a bubble in the ambient Jovian plasma and, to a good approximation, would not couple to Jupiter's ionosphere.

In February and March 1979, *Voyager 1* was nearing Jupiter. Media coverage of the approach was extensive and I remember sitting in front of our erratic and grainy black and white television set, frustrated by not being able to see the varied colors in Jupiter's clouds. I convinced Daniel that we needed a color set, and we had it in place within days, allowing us to appreciate the remarkable images (and our children to watch mind-numbing afternoon programs). Interest in the encounter was considerable when, in early March, David and I visited the Department of Physics and Center for Astrophysics and Space Sciences, UCSD, and I presented a seminar entitled "Magnetospheres of the Galilean Satellites and Their Interaction with Jovian Magnetospheric Plasmas." I no longer recall whether it was the evening before or after the seminar that we saw the first image of a volcano on Io, and this evidence that Io is geologically active increased our enthusiasm for the idea of an internal magnetic field. It took us several years to understand the full implication of a source of neutral gas for the modification of the interaction at Io.

On March 5, 1979, *Voyager 1* swung by Jupiter, providing data on the fields and plasmas of the magnetosphere and some remarkable images of Jupiter and the Galilean moons. *Voyager 2* followed in July of that year. David and I eagerly awaited the publication of the magnetometer data (Ness & Acuña et al. 1979), especially that from the vicinity of Io. We considered what the interaction should look like both globally and along the spacecraft trajectory. In the paper that resulted (Southwood et al. 1980), we described the nature of the interaction in terms of an Alfvén wing model, and, still enamored with the idea that Io was magnetized, described aspects of charged particle fluxes that appeared to us consistent with the contribution of an internal magnetic field. Before our paper was published, Neubauer (1980) provided an analogous description of the Alfvén wing interaction, but, correctly as it would turn out, considered the interaction as cometary.

To better to understand the nature of the submagnetosonic interaction between the Jovian plasma and Io, my student, Jon Linker, undertook the development of a computer simulation, insisting that he wanted to write a code himself from scratch. To no avail did I argue that this was far too ambitious an undertaking for a thesis problem. Jon was insistent, so with Ray Walker as a knowledgeable guide, the work began. I learned the little I know about the ins and outs of simulations from this experience. At our weekly group meetings, Jon would introduce us to esoterica such as the implications for his analysis of elliptical versus hyperbolic boundary conditions, the Courant-Friedrichs-Lewy condition on the time steps, and the problems that were arising near the poles of the spherical coordinate grid that he was developing. His graduate studies took longer than average, but he succeeded in developing a most impressive code for the study of plasma interactions at moons, both magnetized and unmagnetized (Linker et al. 1988, 1991). Recently, Xianzhe Jia, a student of Ray Walker's, has modified Jon's codes to develop a remarkable simulation of Ganymede's magnetosphere (Jia et al. 2006).

A momentous development for the magnetometer team was the 1985 arrival of Krishan Khurana. Krishan was a newly minted Ph.D. from Durham where he had worked on the geodynamo problem; he also held a Ph.D. in geophysical exploration from Osmania University, Hyderabad, India. Having changed fields once, he was unfazed by having to do so again, and he set himself to learn plasma physics. Krishan rapidly became indispensable, and the one-time postdoc remained at UCLA where he became a senior member of the research staff of the Institute of Geophysics and Planetary Physics. One of his earliest challenges was to carry out ground calibration of the magnetometer instrument under the experienced guidance of Bob Snare and Chris Russell. Later, he developed (with Larry Kepko) powerful mathematical tools that streamlined and extended the highly intuitive techniques that Chris Russell had applied effectively for the calibration of magnetometers on earlier missions (Kepko et al. 1996, Khurana et al. 1996). The approach is now in use for calibration of other (Cluster, Themis) magnetometers.

JUPITER AT LAST, AND MEASUREMENTS AT IO

The years of research based on data from earlier Jupiter missions ended in 1995 when *Galileo* reached its target. On December 7, following a close flyby of Io, a powerful rocket engine firing placed the spacecraft into a capture orbit. Ahead of us were years of data acquisition in Jupiter's equatorial magnetosphere, punctuated by numerous flybys of the Galilean moons. Despite all the obstacles, the first spacecraft to go into orbit at a gas giant planet would reap a harvest of knowledge. However, continued patience was needed because it took months for the tiny antenna to transmit the data from the inbound pass.

Data from the inbound pass were special because no other flybys of Io were planned during the nominal two-year mission. *Galileo* had crossed Io's orbit less than 900 km above the moon's leading surface. From the perspective of the magnetometer team, it was significant that the flyby occurred downstream in the flow of Jovian plasma, in Io's wake, where dramatic signatures were found in data from all fields and particle instruments. The field magnitude decreased by an unexpectedly large amount. Our initial interpretations were reported in two papers written shortly after we received the data. The first publication (Kivelson et al. 1996b) reveals us strongly promoting the view that Io has an internal dynamo-generated magnetic field. The abstract reads:

> During the inbound pass of the Galileo spacecraft, the magnetometer acquired 1-minute averaged measurements of the magnetic field along the trajectory as the spacecraft flew by Io. A field decrease, of nearly 40% of the background Jovian field at closest approach to lo, was recorded. Plasma sources alone appear incapable of generating perturbations as large as those observed and an induced source for the observed moment implies an amount of free iron in the mantle much greater than expected. On the other hand, an intrinsic magnetic field of amplitude consistent with dynamo action at Io would explain the observations. It seems plausible that Io, like Earth and Mercury, is a magnetized solid planet.

(Aside: note that *Science* uses lower case for adjectives made from proper names—a convention of which I approve.)

Later in the year when the second set of papers appeared in *Science*, we had become more cautious about the interpretation of the decrease in field magnitude. The abstract of Kivelson et al. (1996c) described the magnetometer observations as follows:

> Galileo magnetometer data at 0.22-second resolution reveal a complex interaction between Io and the flowing plasma of the Io torus. The highly structured magnetic field depression across the downstream wake, although consistent with a magnetized Io, is modified by sources of currents within the plasma that introduce ambiguity into the interpretation of the signature. Highly monochromatic ion cyclotron waves appear to be correlated with the local neutral particle density. The power peaks in the range of molecular ion gyrofrequencies, suggesting that molecules from Io can remain undissociated over a region of more than 15 Io radii around Io.

The data from the first pass were indeed ambiguous and for several years, we could not resolve the question of Io's intrinsic magnetic moment even with the insight provided by new simulations (Linker et al. 1998). We considered it plausible that the interior is set into convection by tidal heating, known to be significant at Io. Even though tidal action deposits heat too close to the surface to drive a classical dynamo, the possibility remained that the asymmetry of the heating would drive convective flows capable of generating a field dominated by higher order (than dipole) multipole moments. Jerry Schubert and others (Bill Moore, Gary Glatzmaier) specializing in studies of planetary interiors shared our interest and gave us the great pleasure of working closely with them.

Had the mission ended after two years, the question of a magnetic field at Io would not have been answered. Fortunately, the mission was granted multiple extensions, ultimately providing eight years of Jupiter system measurements, including several additional relatively low-latitude flybys of Io in October 1999, February 2000, and January 2002. Three high-latitude passes in November 1999, August 2001, and October 2001 would be critical in establishing the presence or absence of an internal field. Data from instruments other than the plasma wave investigation were lost on the critical November 1999 pass, as were data for the pass of January 2002. The two low-latitude passes in 1999 and 2000 did not completely eliminate the possibility of an internal field, leading us to entitle our next report "Magnetized or Unmagnetized: Ambiguity Persists Following Galileo's Encounters with Io in 1999 and 2000" (Kivelson et al. 2001). The final passes at high northern and southern latitudes in 2001 provided the needed evidence. Io does not have a significant internal magnetic dipole moment (Kivelson & Khurana 2002), although the strong plasma perturbations may have hidden a small dipole moment with a surface equatorial field magnitude as large as ~200 nT (Kivelson et al. 2004). The signatures that we originally interpreted as possibly linked to a much larger internal magnetic moment resulted from extremely high rates of ionization near Io; the field perturbations in our data were predominantly produced by the electric currents associated with the ionization. Once we recognized how much pickup was occurring in the immediate vicinity of Io, we accepted that the signatures we had been examining could be understood even for an unmagnetized moon. Further work on this problem requires much improved modeling of the plasma currents and Krishan and I have not given up hope of extracting a more meaningful bound to the internal field of Io.

We probably should have recognized earlier how much ionization was occurring near Io. Analysis of the sputtered atmosphere of Io had led some of our colleagues (e.g., Smyth 1998) to suggest that at least a large fraction of the magnetic signature observed on the initial pass could be attributed to currents generated when neutrals from the atmosphere were ionized. We ourselves had reported signatures of ion cyclotron waves over a considerable part of the pass by Io as described in the abstract above (Kivelson et al. 1996c). Ion cyclotron waves are generated by newly ionized ions whose thermal velocity perpendicular to the background field is significantly larger than the thermal velocity of ions of the same mass per unit charge in the background plasma. The wave power that we reported implied a strong source of pickup ions, but we remained inappropriately skeptical that their contribution was large enough to cause the field to decrease so dramatically.

It was evident that the waves were generated by molecular, not atomic, ions. The wave frequency depends inversely on the mass of the newly ionized ion. The observed frequency required an ion mass of 64 proton masses, far heavier than that of any probable atom but quite plausibly SO_2^+. I was particularly interested in this conclusion because the contribution of molecular ions had not previously been included in estimates of the local ionization rate based on the intensity of UV emissions near Io (Shemansky 1980). Now we had evidence that such ions were present in considerable number (Huddleston et al. 1997, Warnecke et al. 1997), although we failed to establish what fraction of the pickup ions were molecular.

GALILEO'S MAGNETOMETER: MORE SCIENTIFIC HIGHLIGHTS

The interpretation of data from the magnetosphere and from flybys of Io and the three icy moons, Ganymede, Europa, and Callisto, engaged the entire team. Chris Russell focused on ion cyclotron waves and magnetospheric dynamics. For Krishan and me, the moons were initially the central issue. After all of our speculations about an intrinsic field at Io, it was still with amazement that we viewed the incontrovertible evidence that Ganymede had an intrinsic field, strong enough to stand off the Jovian field and plasma that surrounds it and to form its own unusual magnetosphere within Jupiter's magnetosphere (Kivelson et al. 1996a, 1997a, 1998). Once again, our results attracted Jerry Schubert's attention and gave us the opportunity to benefit from his deep knowledge of planetary interior structure and dynamics.

Great excitement also arose when we were able to demonstrate that the magnetic signatures that had been found at Europa (Kivelson et al. 1997b) were consistent with an inductive response driven by the time-varying component of Jupiter's field at Europa's position (Khurana et al. 1998). An induced field of the strength observed can arise only if there is a global-scale conducting layer not far below the surface. Because ice is not a good conductor (except very close to its melting point), Khurana et al. proposed that there must be a layer of liquid water buried beneath the icy surface. The proposed Europa ocean engendered great excitement, with an all-too-understandable reaction that if there is water there may be life. Signatures of an inductive response were also found at Callisto, although the signatures are a bit more ambiguous than they are at Europa.

Questions arose. How deeply is the ocean buried in the ice? How deep is the ocean layer? Postdoctoral fellow Christophe Zimmer was the lead author on the paper (Zimmer et al. 2000) containing a full analysis of the inductive response problem. Inferences from flyby data provide information in which the electrical conductivity of the ocean and its thickness are inextricably linked. If the conductivity is that of terrestrial sea water, a depth of ~10 km suffices to account for the signature. We could not establish the depth of the ocean beneath the surface. However, the analysis showed that if a spacecraft were placed into orbit around Europa and stayed long enough to measure Europa's magnetic response at the orbital period as well as at Jupiter's synodic period, one would probably be able to infer separately the conductivity of the fluid layer and its depth. This has become a major goal of a Europa mission that is under study for launch in the next decade.

As the mission continued, the apoapsis of *Galileo's* orbit rotated from near dawn toward midnight to dusk and beyond. Much attention was directed to studies of dynamics of the magnetotail (Woch et al. 1999, Russell et al. 1999), mainly directed to understanding the relative roles of internal and external sources of the observed instabilities. (In this argument, I am a proponent of the view that internally driven processes dominate, but uncertainty remains great.) Krishan has provided invaluable models of the global structure of the current sheet (Khurana 1992, Khurana & Schwarzl 2005), and Krupp demonstrated that the plasma flow vectors vary systematically with local time (Krupp et al. 2001).

Galileo, despite repeated crises and as the result of creative solutions, outlived its design lifetime by many years. When little fuel remained for maneuvers, the project was instructed by NASA to design a terminal orbit that would destroy *Galileo*, either by crashing it into the surface of Io or by sending it into Jupiter's cloud tops so that it would not, by accident, land on one of the icy moons. On September 21, 2003, *Galileo* entered Jupiter's atmosphere without any way of sending a farewell signal. The journey had been remarkable and the scientific value immense.

Where do we stand today? More research is needed to characterize the interior structure and plasma interactions of the moons and to clarify the processes that transport Iogenic plasma through the system. The *Galileo* data undoubtedly can provide more insight into these matters, but the pace of work has slowed. Still, the future of Jupiter science is bright, with results from a trajectory down the magnetotail by *New Horizons* soon to be revealed and Juno in development (launch by 2010) for exploration of Jupiter's polar regions. NASA is also sponsoring studies of future missions to Jupiter and to Europa. One wonders what surprises will emerge from all of these activities.

ACTIVITIES IN PARALLEL WITH *GALILEO*

During the years of working on the *Galileo* team, I was involved in many other activities. In the spring of 1983, Daniel and I accepted invitations, issued jointly by Peking University and the Chinese Academy of Sciences, to visit China for six weeks supported by the Distinguished Scholar Exchange Program of the Committee on Scholarly Communication with the People's Republic of China, National Academy of Sciences. Our principal host was Professor Pu Zuyin, who had spent two years with my group at UCLA just as China started to emerge from the damage of the Cultural Revolution. During that visit he and I had studied the properties of Kelvin-Helmholtz waves on the magnetopause, modeled as a tangential discontinuity in a compressible plasma (Pu & Kivelson 1983a,b). We were eager to continue our scientific collaboration, and he and his colleagues could not have been more hospitable. We were taken on tours of many laboratories and were impressed by the signs of government support for the academic enterprise, although we were disappointed to find that much state-of-the-art equipment was so vigilantly protected that it was not being used. The academics had not recovered from the atrocities of the previous decade and were generally timid about speaking with us. Only with permission from the authorities could they invite us to their homes. When they did, we were embarrassed by the generosity of their hospitality, which must have cost far more than they could easily afford. We met Pu's wife, Liu Ping, and his two young sons, Pu Shi and Pu Su, who will come back into this narrative when it turns to the recent past.

Both Daniel and I gave numerous lectures for which we were rewarded with endless banquets and fascinating sightseeing, including Chinese opera; acrobats; cultural icons, such as the Forbidden City and the Temple of Heaven in Beijing; two lengthy train journeys so that we could spend weekends in Inner Mongolia; and a final two weeks of purely tourist travel to Xian and the clay army, Guilin and the Li river, Shanghai, and Guanzhou. I met many of the lively young students at Peking University, some of whom ended up in the United States in the following years. In particular, shortly after my visit, Xiaoming Zhu joined my research group. On my return to the United States, I wrote a lengthy description of the experience that I circulated to friends who urged me to publish the account, but I never pursued that idea.

In the fall of 1983, I spent a few months of sabbatical leave at the Observatoire de Paris at Meudon, hosted by Chris Harvey, planning to work with data from ISEE-1 and 2. I interacted as well with researchers interested in Jupiter (for example, Yolande Leblanc and George Dulk) and was taken under wing by Madeleine and Jean-Louis Steinberg (Jean-Louis was then the director of the space physics group at Meudon) and soon began to count them as close friends. While I visited Meudon, Daniel worked with Gilles Tarjus at Jussieu (Paris VI), a more accessible institution where his cramped, shared office looked out on Notre Dame.

It was while examining ISEE data at Meudon that I noticed some unusual, long-duration, relatively monochromatic compressional waves that persisted over a considerable range of radial distances in the dayside magnetosphere. Jacqueline Etcheto, Jean Gabriel Trotignan, and I published a paper describing these waves as global mode compressional waves standing between radial boundaries in the magnetosphere (Kivelson et al. 1984). The wave properties were later examined theoretically in papers written with David Southwood (Kivelson & Southwood 1985, 1986; Southwood & Kivelson 1986, 1990) and others (Zhu & Kivelson 1988, 1989). We identified the compressional waves as normal mode waves of the magnetosphere that can stand in regions of relatively low Alfvén velocity between the plasmapause and the magnetopause. Where the waves cross flux tubes whose resonant shear wave frequency matches that of the compressional waves, strong coupling occurs, thus accounting for the excitation of localized waves of discrete frequency that had long been observed. Our work opened up a new way of thinking about magnetospheric ULF waves, and others carried these ideas further, recognizing, for example, that the discrete frequencies of compressional waves are better described in a wave guide model than in a cavity model (Samson et al. 1992). Computer simulations such as those of Lee & Lysak (1989) gave considerable insight into the excitation mechanism and the spatial distribution of wave power.

I returned to UCLA sufficiently refreshed by my leave to be willing to accept appointment as Chair of the Department of Earth and Space Sciences (1984–1987).

It was fortunate that Bill Kaula was my predecessor, because Bill was one of the most organized people I have known. He spent hours instructing me on the complexities of budget with its different funding sources and restrictions. He filled me in on unresolved conflicts and incomplete personnel actions. Remarkably, he and other predecessors, such as Gary Ernst, were available to give wise advice but not intrusive when I preferred to make decisions on my own.

Little did any of us expect that George Lapins, who had served as the department's business manager for 28 years, would leave during my first month on the job. In today's computerized world, that could well have presented a problem, but in 1984, it was a disaster. There were few records of how the budget was allocated within the department or what major expenses would require funding late in the year. Having George remember everything had worked very well for a long time, but there was no longer a George. With the help of two extremely supportive vice-chairs (David Jackson and Wayne Dollase), I recruited a new business manager, Paul Stoney, a young man with a business degree from Stanford. He modernized the department by introducing computers into the business office, developing tools for keeping track of our expenditures, and instituting personnel reviews for staff who had never been reviewed. Unfortunately, the College of Letters and Science was entering a period of constrained budgets and our dean, Clarence Hall, was demanding economies. Paul and I were forced to initiate charges for services that had previously been free. We were forced to stop offering other services. And we identified one or two long-term employees who were not performing well. These actions were not well received by some of the faculty who, for the most part, placed the blame on Paul. That was fortunate for me because I needed support from the existing faculty in efforts to keep some of our stars from leaving and to hire new faculty in the era of tight budgets. My memory is that during the years of my chairmanship there were losses and gains, but that our department maintained academic strength in the face of external challenges and that interactions among the diverse elements of the department became more effective.

The obligations of being chair were keeping me from meeting regularly with my students. It seemed that my nominally unscheduled hours were often preempted by emergency meetings or faculty/student crises. To be certain that I interacted with my students at least weekly, I decided to schedule regular group meetings on Wednesday evenings. We would go to dinner together, on campus except when food services closed during holidays, and return to the lab at approximately 7:30 PM. No phones, no emergencies interrupt meetings at that late hour. We would go around the table, each student describing the week's progress and raising questions about difficulties encountered. The process was very informal. Polished presentations were not encouraged, the idea being to generate interaction. Sometimes we finished within less than three hours, but often I had to break off the meeting when it was still going strong at 11 PM. The meetings provided new tools for advancing research as one student helped another and the students began to understand that their individual research problems were interrelated. We grew used to having Jon Linker start his presentation by saying that he had nothing to tell us about and then talk for more than an hour. Others would indicate that they needed more than the usual length of time for their discussions but would finish quickly. When I would finally terminate the meeting and meet my sleepy husband for the drive home, I was usually in high spirits because I had had such fun. The students also seemed to regard these meetings very positively. Some who moved into scientific careers elsewhere have told me that our group meetings were the most intellectually exciting and challenging of their careers. Several have told me that they have established evening group meetings in their own research groups. Our weekly meetings continue to this day.

Another student who worked with me while I was chairing the department was Harlan Spence, with whom I explored the pressure structure of the terrestrial magnetotail both theoretically (Kivelson & Spence 1988) and through data analysis (Spence et al. 1989). The former paper argued that as convection builds up thermal pressure in the inner magnetosphere, the pressure develops a duskward gradient because inward-moving energetic ions inevitably drift westward. Westward drift also implies that energetic ions can be lost at the dusk flank of the magnetotail and that some existing estimates of the pressure at the inner edge of the plasma sheet were, therefore, overestimates. This result placed constraints on arguments based on a less-restrictive tail model (Erickson & Wolf 1980), suggesting that inward convection results in a pressure catastrophe in the inner magnetotail and causes instabilities to develop.

In 1988, released from my administrative responsibilities, I was entitled to another sabbatical leave. In January, Daniel and I accepted an invitation to visit Australia, where I lectured at the Australian Bicentenary Congress of Physicists on two of my areas of special interest: "Trends in Physics Education for Women" and "Compressional, Transverse, and Coupled MHD Waves in the Magnetosphere and the Ionosphere." We were accompanied by my 83-year-old mother, who was a stalwart travel companion and who particularly enjoyed our visit with Brian Fraser at the University of Newcastle. Later in the year, Daniel and I headed to MIT, where he worked with his good friend Irwin Oppenheim in the Chemistry department, while I visited the Center for Space Research, also spending time at the Center for Space Physics at Boston University, continuing to work on problems in both the terrestrial and Jovian magnetospheres.

In 1990, Chris Russell and I invited a distinguished group of experts in our field to participate in a colloquium at UCLA. The topic was the entire field of space physics, theory, and observation, and the intention was to turn the lectures into book chapters that could serve as a textbook for first-year graduate students in space physics. The colloquium was extremely successful for both faculty and student participants. Turning the presentations into book chapters was challenging, and imposing some uniformity of style and scientific sophistication was even more so. However, in 1995 we held in our hands a beautifully produced textbook (Kivelson & Russell 1995) and the effort seemed worthwhile. The book has been translated into Chinese; the English edition has gone through several printings and is widely used. I find it thrilling to meet students all over the world who link me to that book.

As I write about still more sabbatical leaves, it occurs to me that the reader may think UCLA faculty are entitled to take leave more often than the traditional one year out of seven. Not so! But we may take sabbaticals one quarter at a time, and, with three academic quarters per year, it is possible to take three leaves every seven years. Thus, in 2003, I returned to the Observatoire de Paris, Meudon, to work with Renée Prangé, Philippe Zarka, and Fran Bagenal, who was also a visitor. Our shared interests relate to how Jupiter's auroral and radio emissions are generated and how they link with the magnetosphere. With a morning espresso in Renée's office and a conversation about our latest ideas or newest perplexities to get us started each day, the visit provided the stimulation that one is supposed to get from a sabbatical. It didn't hurt that after work, Fran and I had great fun sharing a splendid apartment on Place Saint Sulpice in Paris and finding little restaurants or good markets on our way home.

Another institution to host this wandering scientist was the Department of Atmospheres, Oceans, and Earth Sciences at the University of Michigan. My interests (MHD, outer planets, terrestrial magnetosphere) fit well with those of Tamas Gombosi's group and my visits with them have been most rewarding. My first extended visit was in 2003 after the month in Paris. The visit was sponsored in part by the university's ADVANCE program (an NSF-funded program to bridge the gender gap in science and engineering), which organized a well-attended lecture on "Careers, Leadership, and Speculations on Why Academia Loses Women." The material that I assembled for that talk formed the basis for presentations in other venues and the question continues to interest me. During my visit, I became a focus for conversations on career issues for faculty and graduate students, both men and women, and I find that the conversations have resumed on subsequent, shorter visits.

In the fall of 2006, with Krishan Khurana, I spent roughly 20 days in China as a guest of Professor Pu Zuyin. We lectured to groups at institutes and universities in Beijing, Shanghai, and Hangzhou, and were provided with most agreeable companions for visits to numerous tourist attractions. I was amazed at the transformation of the buildings on the campuses and in the cities. Breathtaking architectural gems abound. Our friends are far better housed than on my earlier visits and their laboratories and lecture rooms and instrumentation are of comparable quality to those in the United States.

In Shanghai, I lectured on "Auroras and Related Phenomena at Moons and Planets" at the Shanghai Association for Science and Technology. My lecture drew an audience of ~1000, including students of all ages and at least one elderly gentleman who had traveled by train for several hours to attend. The presentation was transmitted live by video and at the end I responded to many questions from the audiences in the room and on the air, potentially a billion or so! I was a local celebrity, photographed and presented with more flowers than I could hold, as can be seen in **Figure 1**.

Once again, Professor Pu and his family were solicitous hosts. In Beijing we were entertained by Pu's older son Pu Shi and met his grandson, a precocious young man. Pu let us in on the family secret, that his younger son, Pu Shu, is a famous rock star. We tested the fame by telling young people in various cities that our companion, Professor Pu was Pu Shu's father. The reactions made it clear that they knew—and liked—Pu Shu. The high point of the trip occurred on our last evening in Hangzhou, when Professor Pu told us that his son would like to take Krishan and me, him, and his wife to dinner. At 6:30 PM, a stretch limousine drew up to the front of the hotel, and a

Figure 1

Photographs from my lecture on “Auroras and Related Phenomena at Moons and Planets” at the Shanghai Association for Science and Technology.

slender young man who looked like a rock star, wearing a visor over his rather long hair, stepped out and gave me a hug. A stretch limousine and dinner with a rock star: strange to go to China for that experience, but it was a memorable and delightful evening.

A COLLABORATION OF LONG DURATION

As I reach the end of this collection of reminiscences, it seems appropriate to look back on a unique aspect of my scientific career: my collaboration with David Southwood. Our joint work began in the 1970s and continued over decades. Our interactions were often at a distance but peaked during his annual summer visits. Some of our joint papers have been mentioned previously, but there were many others. Our theoretical interests were always tightly linked to data interpretation, and we found that in most cases, MHD considerations could take us far in developing understanding of magnetospheric processes. Much of our work was funded by grants from the Division of Atmospheric Sciences of the National Science Foundation, where we found the program managers to be extremely supportive. Our grants enabled us to work on many topics. A few more examples will suffice. We analyzed the mirror instability, pointing out the underlying kinetic processes that drive its growth (Southwood & Kivelson 1993) and later considering the nature of the fully developed nonlinear case (Kivelson & Southwood 1996). Anticipating the importance of interchange at Jupiter, we analyzed how the process is affected by a realistic field geometry with curved field lines and how the growth rate of the instability is controlled by the ionospheric conductivity (Southwood & Kivelson 1987, 1989). Inspired by observations of anticorrelated field and density perturbations in the magnetosheath close to the magnetopause (Song et al. 1992), we argued that slow mode MHD waves generated at the magnetopause produce anticorrelated field and plasma perturbations only earthward of a nearly field-aligned front (Southwood & Kivelson 1992, 1995). The predictions of our model were challenged. Hubert (2001) took issue with Song et al. (and with our theoretical interpretation), arguing that the anticorrelated field and density signatures that they observed are features of the upstream solar wind. Song & Russell (2002) rejected the criticism and found that our model was consistent with the available observations, so our work seems to have held up.

In an attempt to understand the significant differences observed in measurements of fields and plasmas at different local times in Jupiter's magnetosphere, we analyzed the role of centrifugal acceleration as flux tubes rotate through the system, passing into a region of diminishing scale size in the dawn to noon sector, next through a region of increasing scale size in the noon to dusk sector, and finally into a region on the night side of the magnetosphere that lacks limiting boundaries (Kivelson & Southwood 2005). Data from the recent (2007) pass of the *New Horizons* spacecraft down Jupiter's tail may test our phenomenological theories.

When the *Cassini* mission to Saturn was announced, David was naturally eager to explore another gas giant planet and was pleased when he was selected as PI of the *Cassini* magnetometer. However, before *Cassini* reached Saturn, David was appointed Director of Science by the European Space Agency, and transferred the PI responsibility to Michele Dougherty. Yet David's interest in understanding the data did not diminish and once data from Saturn's magnetosphere were received, he became deeply involved in its interpretation. I was delighted that both David and Michele invited me to join the magnetometer team. My most recent work with David addresses the question of how a planet with an axially symmetric magnetic field imposes a periodically varying magnetic signature on the magnetosphere, the period being close to that of planetary rotation. We have proposed an external current system that flows from ionosphere to ionosphere with the amplitude and sign varying sinusoidally with the phase of the magnetic signal (Southwood & Kivelson 2007). We hope in the future to figure out why such a current system develops.

CODA

A retrospective of a career encourages one to identify themes and to spot turning points. Most critical to my career was the extraordinary opportunity to pursue research in a great university working with accomplished and helpful colleagues and students. That would not have been possible without encouragement and support from my husband and children. Students, both undergraduate and graduate, have kept me rethinking what I think I know. I started in the field of space physics at its inception, when so little was known that it was straightforward to find something interesting that had not yet been worked out. I was in the right place at the right time to get involved in studies of the outer planets. And to my great good fortune, it turned out that the tenuous plasmas of planetary magnetospheres, fascinating in themselves because of the beauty of the physics that they reveal, also hold clues to what is going on deep within the bodies that they envelope.

DISCLOSURE STATEMENT

The author teaches at UCLA, serves as a consultant at the Jet Propulsion Laboratory, and serves on committees of the National Academy of Science. Her research is supported by grants from NASA and the National Science Foundation.

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