A half-century in plasma physics

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Abstract. This memoir is an autobiography of my life as a plasma theorist, highlighting the many individuals who contributed to my intellectual development.

My life in plasma physics began in 1955, when Dick Post gave a lecture at the Livermore Radiation Lab (now LLNL) on the (classified) Sherwood program for controlled thermonuclear energy, and on the mirror-machine research which he directed. (Sherwood was named after Jim Tuck, one of its founders. Jim had been one of my experimental research supervisors at the University of Chicago.) The mirror machine was introduced by Edward Teller and Herb York, who also headed the nuclear weapons program at the newly founded Livermore lab. Edward, one of my professors at Chicago, had brought me to Livermore in 1953, to work on weapons.

Utterly fascinated by the scientific and applied aspects of Sherwood (“success in 20 years”), I asked my weapons-research supervisor Johnny Foster to arrange my transfer to Sherwood. And so this new life began. This was the ground floor of plasma physics, and I was hoping to contribute to its development.

In this memoir, I would like to recognize the many persons who interacted with me, and who provided the exciting stimulation for my scientific life. Let me begin with my pre-plasma life.

1. My parents
My parents were married in 1926, the year of the Schroedinger equation. I was born at the University of Chicago Lying-In Hospital in 1927, oblivious of the wonderful developments in physics at that time. Both my parents had emigrated to Chicago from Germany in 1910, for economic reasons.

My father Justin was born in Cochem in 1888, the one year (actually only 99 days) that Germany had a liberal Kaiser, Friedrich III, son-in-law of Queen Victoria [1]. Cochem is a charming town, among steep vineyards on the Mosel River in the Rhineland, and at the foot of a magnificent medieval castle, built in the 19th century. His father Arnold was a watchmaker; his mother Babette Rothschild (no relation) was born in Bavaria in 1858, and remembered hearing about Lincoln’s assassination. In Cochem he received a sound high-school education from a Catholic institution, highlighted by 5 years of Latin. (He transmitted his love of Latin to me, with many quotations: “quamquam sunt subaqua ...”) After working for a few years in Furth, in 1910 he came to Chicago, to work for a cousin in retail clothing. He joined the Continental Grain Co. as an accountant, and rose to the position of Treasurer, retiring in 1957. This international family-owned (the Fribourgs) concern, headquartered in Antwerp, bought grain from farmers, stored it in silos, and sold it to flour mills. In my teens, I worked as an office-boy in their Board of Trade office on LaSalle St, trying to understand the mysterious world of business.
My mother Millie Low was born in 1890, in Friedberg, Hesse, a few miles north of Frankfurt. The Low clan was believed to be descended from Rabbi Loew of Prague, creator of the legendary golem. Her father Nathan was a cattle dealer; her mother Clara died when she was only 5 years old. Thus she was raised by an aunt, and had sad memories of her childhood. She was the youngest of five siblings; her eldest brother Edward was born in 1879 (the same year as Albert Einstein) and emigrated to Chicago at age 11, to work for his elder half-brother. My mother’s formal education ended with elementary school, but she continued learning through her 90s. She came to Chicago in 1910 with her older sister Paula, and worked first as a governess, and then (after business college) as a stenographer. In the ’30s, she taught English to German refugees at the local Y, and typed Braille textbooks for blind students.

My parents settled in Hyde Park [2], a largely German-Jewish area, one mile from the University, near many parks (Jackson Park, Washington Park, Madison Park, Farmers Green, East View Park, East End Park) and the beach of Lake Michigan, and with excellent public transportation (train, bus, street car). Jackson Park had been the location of the 1893 Columbian Exposition, and still had its magnificent Palace of Fine Arts (converted by Julius Rosenwald’s philanthropy [3] to the Museum of Science and Industry, where I spent many happy hours) with its lagoon, the Japanese Garden on Wooded Island within the lagoon, the Iowa Building, and the beautiful gilded statue of Columbia.

2. My childhood
My earliest vivid memory was political, accompanying my mother to the polling booth in November 1932, to vote for FDR. He was inaugurated on March 4 (“We have nothing to fear but fear itself”), but what I remember is the previous day, my Dad telling me that it was 3/3/33. Intimations of interest in math.

By then Hitler had become Nazi dictator, and my parents decided to take their first trip back to Europe, to visit relatives in Frankfurt, Hamburg, and Vienna. At this impressionable age, I gloried in the trans-Atlantic voyage on SS Manhattan and Washington (later to be troop carriers) and the sights of Paris (Eiffel Tower), Rome (Castel Sant’ Angelo, to be seen again in Tosca, and the Colosseum), Venice (pigeons in St Mark’s Plaza, mosaics in the Cathedral, the Campanile, the Rialto Bridge), Vienna (the Ferris Wheel in the Prater), and Cochem. Nazi flags were everywhere in Germany; my parents told me to speak to them only in English.

I had been bilingual, speaking German to my grandmother, who had not mastered English. When someone told my mother that I spoke English with a German accent, she decided to stop communicating with me in German. So I lost my speaking knowledge of German, as well as of Italian, which I had learned from my sitter during our one week in Rome.

The 30s were stressful times for the Jewish community. My parents’ resources were devoted to helping our relatives escape from Germany, and getting them established in Chicago. My mother was religious, and our family attended services regularly at KAM Temple, the oldest synagogue in the Midwest. It had two successive liberal intellectual rabbis, Joshua Loth Liebman (author of the best-seller “Peace of Mind” [4]) and Jacob J Weinstein (who was a speech-writer for Adlai Stevenson’s campaign for president in 1952) [5]. At my mother’s request, Rabbi Weinstein trained me for my Bar Mitzvah, the first at this reform synagogue.

Returning to 1933, I loved the new World’s Fair, A Century of Progress (Chicago had become a city in 1833), especially the science exhibits. Then first grade began at Kenwood School (no public kindergarten in those days), with a succession of elderly, devoted, mainly Irish, spinster teachers, who instilled my love of learning. Of course, I inherited this primarily from my parents, nature and nurture. I devoured their collection of books: H G Wells’ “An outline of History”, van Loon, Shakespeare, Einstein.
3. Hyde Park High School

Fast forward to 1940, when I entered Hyde Park High. The first year was at the Annex, where I had an inspiring Latin teacher Miss Kirby (‘... cum libertate institiaque omnibus’) and English teacher Miss Slocum (Julius Caesar: ‘you blocks, you stones, you worse than senseless things’; Herve Riel; Evangeline; Ivanhoe). In Algebra, I learned that pi equals 22/7 exactly.

Next year to the main building, where all my (scientifically-oriented) friends enrolled in ROTC to avoid Physical Education and to prepare for military service in the war, and learned math from the greatest teacher of them all, Beulah I (‘for isosceles’) Shoesmith. Our plane geometry was strictly from Euclid, and taught us mathematical logic and beauty. Then came log tables (not even slide rules), trig, advanced algebra, solid geometry, but no calculus. Our other superb teachers were Miss Rubetta Biggs in English (Macbeth: ‘when shall we three meet again’, Pride and Prejudice, Silas Marner, Twelfth Night: ‘if music be the food of love’), Miss Baumgartner in Zoology, and Miss Lyons in US History. My chemistry teacher taught us about local politics, and so I learned the elements of chemistry on my own. In physics, we were strictly classical: Archimedes to Faraday, no Einstein or atomic structure. At our wonderful public library, I found Eddington, Jeans, and Heisenberg, but couldn’t understand how AB could be unequal to BA.

4. University of Chicago

At age 17, I attempted to enlist in the Navy, as many friends had done, to take the Eddy Test and become radar technicians. But since I wore glasses, my induction was delayed for a year. Thus, in June 1944 (shortly after D-Day), I started college at the University of Chicago, as did almost all my classmates, living at home. For several weeks, I was simultaneously finishing my high-school classes, a challenging situation. But even more challenging was introductory Physics, taught by young Mario Iona (who years later won the Millikan Award for his teaching excellence). This class of course used calculus, which I was learning at the same time. In addition, the College required a set of masterly presented survey courses, instituted by Robert Maynard Hutchins & Mortimer Adler, and based on the Great Books [6]. For these, attendance and exams were optional; one’s grade was determined solely by an eight-hour exam at the end of the school year. These classes included Humanities (Herodotus, Thucydides, Aristotle, Plato, Antigone, Hamlet, The Alchemist, School for Scandal, Addison & Steele, Tom Jones, Hume, Locke, The Brothers K, The Trial, The Spoils of Poynton, James Joyce), Social Sciences (Durkheim, Max Weber, Schumpeter), and Biological Sciences (botany, zoology, physiology; no DNA). They led to the PhB degree, my first of four at UC.

My walks between home and the University took me past Stagg Field, surrounded by barbed wire. Of course I had no idea what was happening at the Metallurgical Lab inside. Then in August 1945 the world was transformed, and a few days later, the war ended, with many lives saved. A few weeks before, I had turned 18, and awaited my draft notice. In October, I was inducted into the Navy, that service being chosen because of my one-year college education. After boot camp at Great Lakes, just north of Chicago, I was sent to BuPers (Bureau of Naval Personnel) in Arlington, VA, near the Pentagon. Because I had taken calculus, I was assigned to perform arithmetic for a captain who prepared budgetary estimates for Congress. With demobilization, I had served only 9 months, and then returned to college.

As my math courses (e.g., from Halmos & Kaplansky) became more advanced and abstract, I realized that math would not be my profession. Chemistry was ruled out by a course in quantitative analysis, which required my full time one quarter. Thus physics became my career choice by elimination. At Chicago, there was no separation between undergraduate and graduate courses; the physics curriculum was otherwise structured logically. So after my one-year introductory course, I found myself in competition with graduate students, mostly returned veterans, from other institutions. Taking effectively graduate courses in my second year, I felt
woefully unprepared, so decided to take a year off, to catch up on my own by self-study.

The physics courses were taught by all the faculty, some of whom were superb teachers (Fermi, Wentzel, Zachariasen, Chandrasekhar, Platt, Goldberger, Sachs), while many others were hopeless as teachers, though outstanding in research. My first course from Enrico Fermi was mathematical physics, wisely offered by the physics department because of the abstractness of the math courses. My fellow students and I quickly realized that Fermi was unique in his teaching ability, so we enrolled or audited every course he taught. (One summer he taught three courses!) Just listening to him made one feel that one had complete understanding; his insights were amazing. He taught almost every course offered: solid state, thermodynamics, field theory, nuclear physics. Being exceptionally modest, he never used his own name for the many concepts named for him: thus fermions became Pauli particles.

My fellow students Art Rosenfeld, Jay Orear, & Bob Schluter asked Fermi for permission to publish their notes from his course on nuclear physics [7]. They asked me to write the chapter on cosmic rays. The diagrams I drew, based on Vallarta’s theory, showed regions in the Earth’s field where particles could be trapped. But no one foresaw the existence of the van Allen belt, discovered over a decade later.

While taking courses, I was interacting with the older cadre of graduate students, some of whom were being directed by Fermi and Teller. They were clearly a brilliant bunch, and an inspiration: Yang and Lee, Chew and Goldberger, Marshall Rosenbluth, Mel Gottlieb (my lab TA), Owen Chamberlain. In my own classes were (in addition to Rosenfeld, Orear, & Schluter) Nina Byers, John Firor, Bob Frauf, Burt Fried, Maurice Glicksman, Chas Goebel, Bob Keyes, Steve Moskowski, Moe Scharff, Ron Sladek, Frank Solnitiz, Ludwig Tannenwald, Sam Treiman, George Wetherill, and Gaurang Yodh.

As an experimental Research Assistant, in 1949 I was assigned to Jim Tuck, who with Lee Teng was designing an extractor for the proton beam of the newly built UC synchrocyclotron [8]. The theory involved an iterated mapping, a beautiful technique new to me. My role was to design the iron shims, mill them in the machine shop, and crawl between the pole pieces to measure the magnetic field distribution, using a Dicke flip-coil fluxmeter. When Jim left for Los Alamos (he was the leading expert on high-explosive lenses, and had been the major British contributor to the Manhattan Project), John Marshall became my supervisor. John too later became prominent in Sherwood.

After passing the 32-hour departmental comprehensive physics exam (four 8-hour days) in 1951, I applied to Enrico Fermi for my PhD thesis research. He told me that he was sponsoring only experimental theses, and that I would first need to take an informal course on experimental technique from Dick Garwin, his postdoc. In spite of Dick’s brilliance and patience, I was all thumbs, a danger to myself and others. So I was finally destined for theoretical research, and was accepted by Murph Goldberger (only 5 years my senior), who had recently completed his own thesis under Fermi’s direction. Murph told me that you don’t have to be a genius to be successful as a physicist. His first suggestion for a thesis topic was an extension to scattering of a variational principle developed by David Saxon (later to become president of the University of California system). When I reported my lack of progress to Murph, he said that he had no other thesis ideas, let’s ask Gregor Wentzel. (I had taken Wentzel’s wonderful course on quantum mechanics, in which he had modestly been unable to identify the W of the WKB method.) He told us that yes, he had a lowest-order result for meson-nucleon scattering in his strong-coupling theory of nuclear interaction (of 1940), and wanted someone to calculate the next-order contribution. This theory was the antithesis of the currently fashionable weak-coupling theory, based on Feynman diagrams. The fundamental technique was the use of noncanonical Poisson brackets, to be of great importance in my future plasma research. I studied Wentzel’s original paper, found a serious error, corrected it, and proceeded with a lengthy and complicated calculation until I got stuck. Neither Murph nor Wentzel was able to resolve my difficulty, so Murph took me to
young (2 years my junior) Murray Gell-Mann, who had just joined our faculty. Murray of course suggested the solution.

Toward the end of my thesis research, I faced the dreaded oral exam, covering current advanced topics. My committee consisted of Fermi, Wentzel, Murph, and Murray. (Murph wisely scheduled my exam when its experimental member, Herb Anderson, would be out of town.) To prepare, I asked my good friend Art Rosenfeld, who was working with Fermi, what were Fermi's current research ideas. Art provided me with his four latest preprints, none of which had yet been published. I studied them thoroughly, and thus was able to brilliantly answer Fermi’s questions, to Murray’s amazement. (I confessed to Murph afterwards.)

I was occupying Edward Teller’s office and desk, as he was on leave to Los Alamos and setting up the Livermore lab. Murph recommended that I apply for a position in this new laboratory. Dave Judd and Joe Lepore came to Chicago to interview me, and soon I received an offer to join the weapons program, which I did in June 1953.

5. Weapons Research at Livermore

On arrival in Berkeley, I was greeted by Burt Fried and his wife Sally, who got me settled. Burt had recently completed his thesis with Wentzel, and was now a postdoc at the Rad Lab (UCRL), working with Bob Riddell. (Three years later, Bob would make a rare foray into plasma physics with Steve Gasiorowicz and Maurice Neuman, producing one of the first plasma kinetic equations.) I spent several weeks at UCRL, awaiting my security clearance. When it finally arrived, I was introduced to my new nuclear weapons colleagues, and was asked how I would design an atomic bomb. Of course I had no idea, and proceeded to learn the background theory in Courant & Friedrichs’ wonderful textbook of 1948 [9]. My group leader was Johnny Foster, and my colleagues included Jim Wilson, Ed Chupp, Larry Germain, Chuck Godfrey, Jack Trulio, Wally Selove, Bryce DeWitt, Wally Birnbaum. I also interacted with Harold Brown, Hans Mark, Bob Jastrow, Sid Fernbach, Larry Wilets, Steve Brush, Mike May, Chuck Leith, Herb York, and of course Edward Teller. We felt that the Soviet threat was very real, and so we worked hard, often around the clock. Our computer was the Univac, utilizing the octal system and a punched tape input. (One motivation for its use was that it occupied the only air-conditioned room at the lab. For human brains, it was felt that Livermore summer temperatures would not affect productivity.)

One needed empirical datum was the high-pressure equation of state of the substance we used. The only person with the required instrument for reaching a megabar was Percy Bridgman of Harvard, who had received the Nobel in 1946 for this work. So I was appointed as the courier for a large sample of this radioactive pyrophoric substance, as carry-on baggage on a commercial flight to Boston. Prof Bridgman greeted me in his lab, and allowed me to participate in his measurements; he then flushed the sample down the drain. To determine the shock Hugoniot of this substance, I later designed an experiment using high explosives, which Wally Selove then carried out.

On a visit to Los Alamos, I was greeted by Jim Tuck, who showed me his Perhapsatron, utilizing the pinch effect, which again was totally new to me. (Somewhere Anperian attraction of parallel currents had not been included in my E & M course at Chicago.)

While working at Livermore, I was living in Berkeley (with an 80-minute commute, before the introduction of freeways). So I could participate in evening activities on the campus, especially the weekly Physics Dept journal club. Here were presented reports on important new developments, and I was able to interact with Bill Nierenberg, Ernest Lawrence, & Luis Alvarez.
6. Sherwood at Livermore

My transfer to the Sherwood program in 1955 represented a sudden reduction in pressure. (Our theory group leader, Ted Northrop, called our area Happy Valley.) My first task was to educate myself in this new field of high-temperature plasma physics. There were no textbooks yet published (Spitzer’s excellent book came out in 1956 [10]). Because the program was secret (until 1958), there were few papers in the open literature. My main resource was a set of lectures given by Conrad Longmire in Los Alamos the year before.

There were good interactions among our group, which included Cliff Gardner (later known for his stability theorem and for his co-authorship of the inverse scattering method), Marvin Mittleman (whose wife Sondra arranged my introduction to my future wife Louise), Lew Tonks (who had collaborated with Irving Langmuir in discovering Langmuir waves in 1928), Howard Greyber, Bill Newcomb, John Killeen (our mathematician), and later Nick Christofilos (inventor of the alternating-gradient or strong focusing cyclotron), Harold Furth (who was working with Stirling Colgate), and Bernie Lippmann.

My first publication for the Sherwood project was on collisional particle loss in the mirror machine, taking account of the electrostatic field caused by the excess of electron loss over ion loss. This was presented at a conference of 350 Sherwooders in Gatlingburg, in June 1956. Shortly thereafter, I spent a month at Los Alamos, having been invited by Murph and Ken Watson.

At the public library there, I found a book on how to select a spouse. This was a serious research topic, as I was emotionally committed to Louise Lazarus, whom I had met on Cinco de Mayo, thanks to my colleague Marv Mittleman and his wife Sondra. After a whirlwind courtship, I found in this book a checklist for marital compatibility, and confirmed my feeling that we were meant for each other. Louise accepted my invitation to travel to Mexico with the Mittlemans; in Mexico City, on Aug 19, at the romantic restaurant Rincon de Goya, I popped the question, and we were engaged. Our wedding took place in April 1957, at Temple Emanuel in San Francisco, by our rabbi Alvin Fine (author of “Life is a journey”, cousin of Jacob Weinstein, and another inspiring liberal intellect). Louise quickly learned about life with a physicist, and has provided me with the moral support I needed throughout my career.

In early 1957, Stirling Colgate suggested that I examine the collisional transport of plasma mass and energy due to a temperature gradient perpendicular to a strong magnetic field. (The effect of density gradient was discussed in Spitzer’s textbook.) The most important result of my calculation was that ion-ion collisions dominated thermal transport. After I had finished, Stirling confessed that he had suggested the same problem to Marshall. So I consulted Marshall, found that our results coincided, and wrote up our work for joint publication [11]. I followed this with a study of the effect of charge separation [12]. My next foray into transport theory was stimulated by Nick Christofilos’ question as to the microscopic explanation for the formulas of magnetized-plasma viscosity. I found that shear flow implied elliptical gyro-orbits, and led to the desired explanation [13].

In the summer of 1957, and newly married, Louise and I spent some weeks in Los Alamos. I worked with Ken Watson and Chandra, formulating a fluid theory for collisionless plasma. Our major success was the discovery of the instabilities due to anisotropy of the pressure tensor [14].

In the early summer of 1958, we went to San Diego, where I worked at Convair (General Dynamics) with Ed Hones & George Stewart, and with Bob Vik, Bob Karplus, and Ken Watson, on the dynamics of an artificial belt of electrons in the Earth’s magnetic field. The electrons would be injected by a nuclear explosion. This had been proposed by Nick Cristofilos, as a defense against Soviet ICBMs; his proposal was later tested in Operation Argus. While at San Diego, I was interviewed by Roger Revelle, who showed me the site he envisioned for the new UC campus, to open in 1960. But I opted to stay at the Livermore lab.

In September 1958, we proceeded to Geneva, for the Atoms for Peace Conference, at which
the US, British, & Soviet fusion programs were first put on display and declassified. (Because the AEC had strangely decreed that only experimentalists could attend, I was given a crash course in operating one of the lab’s plasma devices. Of course, at the conference itself, I was not allowed to touch it.) We met the leading Soviet theorists: Igor Tamm (who gave an after-dinner speech about his Menshevik days during the Revolution), Roald Sagdeev, Boris Kadomtsev, Evgeny Velikhov, Vitaly Shafranov.

On the day we returned to the states, we visited my father in the hospital, and he died a few hours later. My mother had protected us from the knowledge of his terminal illness.

In the summer of 1959, I was invited by Cecile Morette to lecture at the Les Houches summer school, near Chamonix & Mont Blanc, on the subject of plasma dissipative processes. My fellow lecturers included Leon van Hove & Elliott Montroll. The students, half French and half foreign, were very bright; one of them was Donald Lynden-Bell.

7. Teaching at UC Berkeley

In the fall of 1959, the need arose for someone to teach the one-year graduate course in electromagnetic theory (Physics 210, E & M) on the Berkeley campus, as the professor assigned to it had left to take a sabbatical. With one week’s notice, I was offered this position (a one-year contract, at one-third time, as Lecturer) by Carl Helmholz, the chairman. Bob Karplus, who had taught the course the previous year, gave me his lecture notes to use. And so my teaching career began.

Of course, I had to prepare the exams on my own. For the first mid-term exam, the class average was 55 out of 100; that seemed quite satisfactory. On the final exam, the class average was again 55, but this time out of 300. (A nice example of invariance.)

In using Panofsky & Philips as my textbook, I was dissatisfied by their treatment of dielectric energy. I expressed my discomfort to Ken Watson on a Friday; the following Monday, he came in with an extensive set of notes, laying out a statistical mechanical approach to this subject. The problem posed was the thermal equilibrium of an imperfect gas in an applied electric field. For an ideal gas (of identical molecules), there was a known result, the Clausius-Mossotti formula, relating the dielectric constant to the single-molecule polarizability. The challenge was to deal with both the short-range interaction between molecules and their long-range dipole-dipole interaction induced by the electric field. Of course one had to define the macroscopic field clearly, as opposed to the random microscopic field. Working with Ken on this problem was a great experience [15, 16]. Later we discovered a similar treatment in the Handbuch der Physik article by Wm Fuller Brown, as well as a sophisticated macroscopic theory by Landau, in terms of thermodynamic free energy. It was natural to then study the analogous magnetostatic problem, which I conducted with Ken’s postdoc Toshio Soda [17]. Whereas the electrostatic problem allowed a classical treatment, a quantum formulation was required for the magnetic problem, since the classical magnetic susceptibility vanishes identically, by the Bohr-van Leeuwen theorem. We utilized the Darwin Lagrangian, which we had studied in earlier work [18] in connection with the definition of magnetic energy. Later, with Pete Rostler (Wulf Kunkel’s student), we proposed the Darwin Lagrangian as an appropriate tool for plasma dynamics simulation [19].

Other papers resulted from perceived deficiencies in standard textbook treatments. On the relation between microscopic and macroscopic electric field, I found a subtle correction to Leon Rosenfeld’s treatment, involving molecular quadrupole density [20]. For Maxwell equations in moving media, I was surprised that it had always been assumed (e.g., in Tolman’s treatise) that the motion was spatially uniform and temporally steady, and that non-steady motion required a general-relativistic treatment. Accordingly, I developed a statistical approach based on local inertial frames, to define the macroscopic fields [21]. This was later generalized by DeGroot and Suttorp. Another question concerned the equation of motion of a dielectric medium, which Landau & Lifshitz had left open [22].
In the fall of 1961, Wulf Kunkel initiated and organized a series of technical lectures throughout the state on the extensive new developments in plasma physics, theoretical, experimental, and applied. He invited me to present the lecture on dissipative effects. He then edited the written versions of these lectures, in a book published in 1966 [23].

During the 1961-62 school year, the Livermore lab asked me to offer the same course 210 that I was teaching in Berkeley (MWF in Berkeley, TuTh in Livermore). My prize student was Gerry Schubert (now an eminent geophysicist), who commuted in from his full-time job at the Navy base in Vallejo.

When Lou Klauder requested that I serve as his PhD thesis advisor, I proposed that he extend our theory of linear polarization to higher order in the field, to exhibit electrostriction. His thesis [24] was completed in 1964. After Lou, my next three students all did plasma-physics theses: Shalom Fisher [25], Gary Pearson [26], and John C. Price [27].

By 1963, it was apparent to me that a graduate course in plasma theory was needed, to supplement the undergraduate course (Physics 142) that Wulf Kunkel was teaching. To prepare myself in modern plasma theory, I took a leave from the Livermore lab, to spend some time at the Princeton lab (PPPL), where many of the leading theorists worked: Ira Bernstein, John Dawson, Ed Frieman, John Greene, John Johnson, Martin Kruskal, Russell Kulsrud, Andy Lenard, and Carl Oberman. We rented Murph & Mildred Goldberger’s beautiful house on Fitzrandolph Road. By then we had our two children: Joel, born in 1960; Janet, born in 1962. From our neighbor Evelyn Patterson, Louise acquired her classic “Recipes for elegant but simple dining”.

Thus, in the fall of 1963, I offered the first semester of the graduate course Theoretical Plasma Physics 242A, limiting the material to unmagnetized plasma. Ted Northrop then presented the magnetized case, 242B, underlining B, in Spring 1964. This course was eventually expanded to four quarters or three semesters, and was offered every other year. While this physics course was analytic and based on kinetic theory, my colleagues in EE presented MHD theory (Allan Lichtenberg and Mike Lieberman) and numerical simulation (Ned Birdsall).

8. UCLA interlude
In early 1964, I received an offer to join the physics faculty at UCLA. Its two theorists were Burt Fried (my fellow graduate student) and Alfredo Banos (who had visited our group at the Livermore lab), and the chairman was Dave Saxon (whose work I had studied in my thesis research). Before accepting, Louise and I thought it prudent to try living in the LA area for a year. So I took a leave from the Livermore lab, and we moved to Sherman Oaks for the 1964-65 school year.

At UCLA, I taught the plasma theory course; among my students was George Johnston, who would be working with me some years later. My research there, stimulated by Al Wong’s experiments, was with Burt and his postdoc Dave Sachs [28].

9. On the faculty at UCB, 1965-1971
While I was at UCLA, the Berkeley campus, in the throes of student riots, came up with a counter-offer, which I accepted. So in Fall 1965, I joined the regular faculty as a tenured Associate Professor, after five years as Lecturer. Part of my salary came from the Rad Lab, where I inherited Ted Northrop’s office (he was moving to Goddard Space Flight Center), his desk (which I still have), and his thesis student Chuan Liu.

Ann Palm got us interested in the aurora, and Chuan proposed that he develop a theory of its dynamics for his thesis [29]. He was followed by Norman Albright [30] and Blake Patterson [31].

Ted provided me with a NASA grant to hire a postdoc, and I was fortunate to acquire Ed Frieman’s prize student Ron Davidson in 1966. Ron’s thesis was most impressive, and led to his publication output averaging one significant paper per month. Toward the end of his two-year
stay, he presented a series of lectures on nonlinear plasma theory, which he then converted to his famous textbook.

The one paper that Ron and I co-authored, on weak turbulence [32], used a standard formula for wave entropy, derived by taking the classical limit of the formula for the entropy of a boson gas. The need for a quantum detour bothered me for years, and finally I discovered a purely classical derivation by Jaynes’ maximum-entropy principle [33]. A few years later I met Rafi Levine, who told me of his prior use of Jaynes’ method.

With Dave Baldwin, I developed a simple inverse method to determine the density profile of a plasma slab by external impedance measurements [34]. The extension to cylindrical geometry was mathematically challenging, so I referred that problem to Tony Degasperis [35].

When Toshio Nakayama became my postdoc in 1969, I proposed that we extend quasilinear theory from the well-studied spatially uniform case to the more challenging case of one-dimensional spatial variation [36]. This led to studying a cylindrical model of magnetized plasma [37], and finally to quasilinear diffusion in a tokamak [38]. That work utilized an action-angle formulation, based on Brian Taylor’s guiding-center Lagrangian [39].

In September of 1970, we lost our home to a fire, set by an arsonist in dry grass on Fish Ranch Road. He was an employee of the regional park district, hired to fight fires, and paid by the fire, we were told. The strong dry winds spread the fire to our neighborhood, where a grove of eucalyptus, with loose bark and oily leaves, further spread the flames. Phone calls to the Oakland fire department were misinterpreted by their dispatcher, so their trucks came late. They requested help from Berkeley, whose hoses were of different diameter from Oakland’s, and so did not fit the hydrants. Many volunteers, including students from the campus, came up the hills to assist the firemen. In all, 37 homes were totally destroyed; fortunately, no lives were lost.

Since the eucalyptus forest was now gone, we (wrongly) believed that our area would be safe from future fires, so we immediately decided to rebuild on-site. During the prior year, we had been remodeling our house, so that our children could have separate bedrooms. Thus our talented architects, Kolbeck and Christopherson, and builder, Al Heffley, were available to design and build our new home. We decided to take another leave at PPPL during Spring 1971, while our home was being rebuilt. In February 1971, we learned about the San Fernando earthquake, with its dreadful loss of life. Our architects’ engineer went there to learn about reducing seismic risk, and advised appropriate strengthening of our incipient house.

10. 1971-1980

Shortly after our return in 1971, I acquired three new talented thesis students. To find suitable thesis projects for each of them, I turned to the prolific work of Marshall Rosenbluth. For Bruce Cohen, we examined Marshall’s idea of beat heating, whereby the beat difference frequency of two lasers would be tuned to the local plasma frequency, and nonlinearly excite Langmuir waves, which would heat the plasma by Landau damping. We recognized that Marshall had overlooked the constraint of action conservation (Manley-Rowe relations), so that the primary effect would be the transfer of energy from the higher-frequency laser to the lower frequency laser. Joined by Ken Watson, we proposed instead a cascade process [40]. (At that time, conservation laws were not yet derived from variational principles and Noether’s theorem. Instead, they were discovered by examining a set of coupled evolution equations, and so were often overlooked, being sensitive to relations among the nonlinear coupling coefficients.) Bruce examined the beat resonance in detail [41], including a simulation with my postdoc Claire Max [42].

For Dwight Nicholson, I proposed examining the question of absolute vs convective instability in three-wave interaction in a plasma gradient. This was a controversial and important question, as the convective case would saturate, while the absolute case would become fully nonlinear. Previous studies had used smooth background profiles, and led to convective growth resulting
from phase relationships. We felt that a random perturbation of the density profile would disrupt the latter, and lead to absolute growth. This was confirmed in Dwight’s study [43]. He then went on to show, in his thesis, that randomness was irrelevant, and that the same effect was produced by a sinusoidal perturbation [44].

For Mike Mostrom, I suggested the problem of Raman side-scatter instability, where the validity of WKB approximations made by Marshall and his co-workers was uncertain. His detailed analysis was incredibly complicated and sophisticated [45, 46].

By the time that Gary Smith joined us as my fourth student, I was cognizant of the newly discovered phenomenon of chaos. The idea that a deterministic system could exhibit random behavior was startling to the scientific community. I had been introduced to the subject in a colloquium at Princeton by Joe Ford, during my 1971 stay there. We read his papers (from 1969), studied the Henon-Heiles Hamiltonian (1964), were amazed by the revelations in Robert May’s 1974 review in Science, and learned about Chirikov’s resonance overlap studies. We had been unaware of Ed Lorenz’ seminal paper of 1963, until pointed out by Ruelle & Takens.

At the time, there was discussion of the validity conditions for quasilinear diffusion. The standard derivation assumed a set of waves with neighboring phase velocities, and with random phases. Thus the randomness of diffusion would result from a random input. In contrast, I proposed that a nonrandom coherent set of neighboring resonances could produce diffusion, if the resonance islands overlapped. Such a set could be produced by the field of a single wave, with Doppler-shifted gyroharmonics. Gary tested this idea numerically, and it worked beautifully [47, 48, 49]. When I presented this work at the next APS meeting, it was met with incredulity. (When I told Tom Dupree that he had assumed such behavior in his own work, he said, “Yes, but I didn’t believe it.”) We searched for a term to describe this phenomenon, and came up with “intrinsic stochasticity”; not surprisingly, “chaos” was the term that stuck (Yorke 1975).

We presented this work at a major international conference in Cernobbio, in the summer of 1977: Stochastic Behavior in Classical and Quantum Hamiltonian Systems (VOLTA Memorial Conference, Como, organized by Giulio Casati). Here we first learned about quantum chaos, which would later become part of Steve McDonald’s thesis [50].

In 1972, I proposed a new formulation of quasilinear theory, which had been plagued by the phenomenon of “fake diffusion”, a second-order reversible change in the velocity distribution due to interaction of the wave field with nonresonant particles [51]. I showed that this effect represented the particle contribution to wave momentum and wave energy. This idea was clarified further and formalized by Bob Dewar, who introduced the concept of “oscillation center”, and obtain its Hamiltonian dynamics by a canonical transformation [52].

In 1974, Hans Wilhelmsson convened an international workshop on plasma theory at Chalmers University in Goteborg, and in later years Nobel Symposia in Aspenasgarden. These meetings provided valuable interactions with Vladimir Karpman, Vadim Tsytochich, Lennart Stenflo, and many others. Lennart was a master of nonlinear wave interactions: we extended the Manley-Rowe action-conservation laws to the presence of a background high-frequency field [53] and we studied upper-hybrid solitons [54]. Later Lennart came to visit me at LBL, and produced an extension of 3-wave interaction to essential plasma nonuniformity [55].

Shayne Johnston became my postdoc in 1974, as a master of Bob Dewar’s oscillation-center theory applied to nonlinear problems. We recognized that the widely-used heuristic ponderomotive potential was actually the oscillation-center Hamiltonian. We used these ideas to study nonlinear wave interactions [56], and, with George Johnston, we generalized this concept to allow for a ponderomotive vector-potential [57].

A major improvement in classical perturbation theory was the introduction of the powerful Lie transform, replacing the clumsy use of mixed-variable canonical transformations. We were made aware of it by Bob Dewar and by Alex Dragt and John Finn, and enthusiastically adopted it for all our studies [58]. By now I had three bright new students: John Cary, Robert Littlejohn,
and Harry Mynick; we each produced a review of the Lie transform. John called his review “A Pedestrian’s Guide”, so Robert’s became “An equestrian’s guide”, and Harry’s was “for the jet set”. My own review was personal and historical [59].

John’s thesis research was devoted to developing the Lie transform, and applying it to a self-consistent nonlinear plasma-wave problem [60]. We discovered the remarkable identity between the linear susceptibility and the quadratic ponderomotive Hamiltonian, the K-χ theorem [61].

Harry Mynick chose his own thesis topics [62]: motivated by Mort Levine’s experiment at LBL, one was a guiding-center dynamics when the gyro-orbit covers a large variation of magnetic field. Another topic was Harry’s rigid-bar model for wave-trapped particles [63], based on a paper of O’Neil, Winfrey, & Malmberg.

For Robert Littlejohn, I selected an important problem whose solution had baffled me (as well as Bob Dewar) for some years: developing a rigorous and physically oriented Hamiltonian theory for guiding-center motion. By this time all three adiabatic invariants were known [64], Ted Northrop had published his comprehensive treatise using Hamiltonian ideas (1963), and yet a satisfactory systematic Hamiltonian theory was clearly needed. (I had used an unsatisfactory one in my 1972 quasilinear diffusion paper.) The sticking point was the universal belief among physicists (based on the standard textbooks) that a Hamiltonian formalism was necessarily based on canonically conjugate variables. The breakthrough came when Alan Weinstein gave us the galley proofs of his translation of Vladimir Arnold’s new textbook on classical dynamics (published in 1978). Here was the exposition of symplectic geometry, and Hamiltonian dynamics with noncanonical Poisson brackets. Another new world opened for us. Robert was able to use these concepts to developed the needed guiding-center theory [65, 66, 67].

My next postdoc (1976-8) was Nino Pereira, an expert on solitons. With our visitor Lennart Stenflo, he generalized the nonlinear Schroedinger equation, to allow for soliton growth or damping [68]. With Gary, he studied chaos among trapped particles [69]. And with Ken’s student Jim Meiss, he studied internal wave solitons, based on the Benjamin-Ono equation [70].

In 1978, my former student Chuan Liu (now a professor at Maryland) sent me his own student Celso Grebogi to postdoc with me. Our first project, with Robert, was to use the Lie-transform technique to extend the ponderomotive force formulas to magnetized plasma [71]. (The controversy was whether the gradient of the second-order potential acted also on the magnetic-field nonuniformity.) Then, with John Cary and Jim Meiss, with Henry Abarbanel and his student John David Crawford, and with Jin-Soo Kim (Andy Sessler’s student), we began investigating various aspects of chaos [72, 73], as well as iterated maps for gyroresonance crossings. When Celso returned to Maryland in 1981, joining Ed Ott and Jim Yorke, he became eminent in the chaos field. (And a few weeks before my own Fest80, Louise and I journeyed to Aberdeen to celebrate his Fest60!)

Our entry into quantum chaos was stimulated by a paper of Wersinger, Ott, & Finn [74], examining the chaotic rays of lower-hybrid plasma waves. This led me to wonder what the corresponding wave field would look like. We needed a simple model to investigate the relation between chaotic rays and wave fields, or correspondingly, between classical orbits and quantum wave functions. Such a model was the stadium-shaped billiard, whose classical chaotic properties had recently been determined by Benettin & Strelcyn [75]. For a free particle, the corresponding quantum problem was the Helmholtz equation. A computer code for finding its eigenfunctions and eigenvalues had been written by our colleagues Bob Riddell & Joe Lepore, for the study of waves on Lake Tahoe [76]. My student Steve McDonald undertook a detailed study of the quantum billiard, and obtained its Wigner eigenvalue spectrum and its eigenfunctions’ chaotic nodal lines. Our first paper [77] portrayed the world’s first chaotic eigenfunction, and has become my most cited paper by far. In later work, Steve studied the Wigner function for a quantum chaotic billiard, testing Michael Berry’s theories [78].

What an exciting decade, with all these new concepts to revel in: Manley-Rowe, classical and
quantum chaos, oscillation center dynamics, Lie transform, noncanonical Hamiltonian dynamics for guiding-centers. To master them, we had a weekly dynamics seminar, in addition to our long-standing plasma seminar. We worked our way through important review articles and textbooks, especially Arnold’s two and later Sinai et al. on ergodic theory.

11. 1980-1987
Steve had taken a math course from Andrew Majda, on pseudodifferential operators. He then developed the Wigner-Weyl symbol calculus as a tool for plasma theory. Our first important result was a derivation (as opposed to a statement) of the linear wave kinetic equation for the action density in ray phase space [79]. With Celso, we then combined the wave evolution with that of the oscillation centers [80], including resonant interaction. Steve later prepared an important and extensive review paper on the symbol calculus [81].

In late 1981, Bob Dewar sent me his student Eliezer Rosengaus to be my next postdoc. At the Quantum Chaos Conference in Como, in 1983, we presented our ideas on classical wave chaos.

Shortly after Robert’s discovery of the Poisson brackets for guiding-center dynamics, Phil Morrison & John Greene [82] presented a noncanonical Hamiltonian formulation of MHD. With my student Rich Spencer, we applied these ideas to the two-fluid model of plasma dynamics [83]. After Phil found a Hamiltonian structure for the Vlasov equation [84], I worked with him to find such a non-Hamiltonian geometric structure for the quasilinear equations [85]. Such concerns led me to a dissipative bracket formalism [86], found independently and simultaneously by Phil [87] and by Miroslav Grmela [88]. I then generalized the formalism to a covariant form [89], and further with Lukasz Turski [90].

In 1983, Steve Omohundro became my thesis student with broad interests in plasma physics and in math, so Alan Weinstein was his co-advisor. To a large extent he developed his own ideas, and published his thesis [91] as a book.

In 1983 Philippe Similon became my postdoc. One of our first projects concerned the stabilization of an unstable plasma by the ponderomotive effects of an applied rf wave, which had been demonstrated by Noah Hershkowitz’ group at Madison. From my work with John Cary, Shayne Johnston, & George Johnston, we expected a number of new subtle effects, which Philippe obtained by formulating the problem by a variational principle [92, 93, 94]. This success confirmed my gradually growing conviction of the power of variational formulations. Earlier evidence of this power had been Robert’s Lagrangian reformulation of guiding-center dynamics, Herb Berk & Rene Dominguez’ variational approach to systematic approximations, and the pioneering work of Fred Crawford, John Dougherty, and Bob Dewar, demonstrating Noether’s method for obtaining conservation laws from continuous symmetries. Robert had converted the Lie transform to a Lagrangian setting, working with John Cary. Building on these ideas, Darryl Holm and I derived a variational (and relativistically covariant) approach to the dynamics of an unmagnetized oscillation-center plasma interacting nonresonantly with an eikonal wave [95]. The needed extension to a magnetized plasma was a major undertaking, so I assigned it as a thesis topic to my student Bruce Boghosian. The first steps in this project were developed by Philippe, who produced a covariant guiding-center plasma dynamics, with a complete set of conservation laws [96]. Then Bruce extended the formulation to the covariant oscillation-center plasma, interacting ponderomotively with an eikonal wave. (His important thesis was never converted to a journal publication, but is now available on the Web [97].) The next extension, to include resonant interaction, and the consequent reaction of the background fields, was accomplished by Huanchun Ye in his thesis [98, 99].

In 1985, a semester-long workshop on plasma theory was held at the Santa Barbara Institute for Theoretical Physics, organized by John Krommes, Tom O’Neil, and Ed Ott. One of my projects was to work with Celso, to complete what Steve McDonald and I had begun on
obtaining coupled equations for interacting particles and waves [80]. The other was suggested by Carl Oberman, who proposed using the methods of my group (variational principles and Lie transforms) to deal with a problem that John David Crawford had formulated, the coupling of modes by beat resonance. After we had finished, I discovered to my delight that Shayne had done the same calculation in unpublished notes in my files [100].

Huanchun Ye started as my student in 1985. My first project for him was to apply our Lie transform methods to determine a three-wave coupling coefficient under kinetic (as opposed to fluid-model) conditions. As a comparison, we had the mysterious (to us) powerful methods developed by Lennart Stenflo and his former student (now colleague at Umea) Jonas Larsson. Their approach led to an expression with six terms, while ours had 192 terms, as I recall. More about Jonas later.

12. Mode conversion, 1987 on

In 1986, Lazar Friedland came to spend a sabbatical year working with me. I had invited him after seeing his incisive approach to linear mode conversion, presented at an APS meeting. The first of his chief ideas was a systematic reduction of a multicomponent wave equation to a two-component form, representing the two waves involved in the conversion process. After linearizing the two wave operators with respect to their \( x, k \) dependence (and treating the coupling as a constant), he cleverly solved the equations for the transmission coefficient of the incident wave. Examining his expression, I recognized a Poisson bracket hiding there; so I felt that there must be a far simpler and more fundamental approach to this problem. My main tool was the metaplectic transform, a powerful generalization of the Fourier transform, which Robert had recently developed. This was the field/operator transformation representing an arbitrary linear canonical transformation of \( (x,k) \) phase space. Indeed this approach made the solution of the coupled equations trivial, as they became a first-order ODE.

Toward the end of Lazar’s stay, I returned to Chicago to look after my mother during her terminal illness. She passed away at age 97.

In 1987, Walt Sadowski convened a Science Court, to provide guidance on the problem of rf heating in the ion gyrofrequency range for tokamak plasmas. One team, called the “defense”, presented their approach, while another team, called the “prosecutor”, critiqued it. The three “judges” (myself, Charles Karney, and Ira Bernstein as chief justice) then prepared a report with recommendations. Reading through the literature, I realized that the mode-conversion analyses that Lazar and I had been doing as an academic study were immediately applicable to a practical problem in fusion energy.

Accordingly, Huanchun and I turned our attention to linear conversion problems. Our first example was the linear ion-cyclotron echo (which we decided not to abbreviate as LICE), based on a fluid model (including the anisotropic pressure tensor) which Lazar had introduced. We showed that the standard Budden model represented a two-step conversion, with the Budden resonance actually an eikonal wave at fixed \( x \), propagating only in \( k \)-space.

After our publications [101, 102], Tudor Johnston informed us that the delayed echo idea had been discussed by Muldrew years before, in 1969.

In August 1989, two visitors joined our group for a year: Maria Ekiel-Jezewska from Warsaw & Tor Fla from Tromso. Maria & Tor derived a generalized vector nonlinear Schrodinger equation, with resonant particle effects included. In addition Tor joined a number of our other projects, including multiple mode conversion [103].

Alain Brizard began his collaboration with me in October 1989, arriving, as my new postdoc, two days before the Loma Prieta earthquake. His Princeton thesis had demonstrated his expertise in Lie-transform and variational methods. Since then, we have worked together on many problems associated with mode conversion and with variational principles, publishing 13 papers as co-authors, so far.
At William & Mary, Gene Tracy started his collaboration with me at about the same time, beginning with a generalization of mode conversion theory to incoherent waves [104]. He then put my heuristic analysis on a sound mathematical footing [105]. Over the years, Gene, Alain and I have been kindred spirits, with a remarkably similar approach and philosophy about plasma physics.

In July 1990, a conference on “nonlinear and chaotic phenomena” was held in Edmonton, organized by Rozmus and Tuszynski. Here I presented an extensive review of our group’s work on “linear and nonchaotic phenomena”, with historical details.

In October 1991, fire struck the Berkeley-Oakland Hills again. On a Saturday, there had been a minor fire, caused by debris from a remodeling project near our home. After putting it out, the Oakland Fire Department left the scene, with embers still hot. The next morning, strong winds revived the fire, which was quickly out of control, spreading as a fire-storm. Some 25 lives were lost, and 3,000 homes. We now knew it was futile to rebuild, so purchased a house in Orinda. For a week we stayed with Wulf & Erika Kunkel, and then for a month with Don & Lynn Glaser. While there, we were horrified to learn that Dwight Nicholson and colleagues had been murdered. Recognizing Dwight’s exceptional qualities of character, Marty Goldman & Herb Berk initiated an APS medal in his memory, to be awarded annually.

Dan Cook was my last thesis student, his topic being the ray-based analysis of minority heating by mode conversion [106]. Lazar had shown that Landau resonance and gyroresonance could be quantitatively interpreted as conversion to ballistic waves. In minority heating, there is a combination of conversion to a collective wave (a minority quasi-Bernstein wave) as well as Doppler-broadened gyroresonance (interpreted as conversion to a continuum of Case-van Kampen modes). Accordingly, it was necessary to incorporate the (damped) collective wave within the complete set of eigenfunctions of the linearized Vlasov equation. This was accomplished by analytic continuation to complex-valued velocity, the technique developed by John David Crawford and Peter Hislop. The reflection of the incident magnetosonic wave then resulted from a second conversion process [107]. The analysis was then extended from the slab model to two-dimensionality [108].

Jonas Larsson, a frequent visitor, introduced a number of radical new ideas into plasma theory, which we appreciated and utilized. One was the concept of pseudo-Hilbert space, and the corresponding idea of a pseudo-Hermitian operator. (The former has an inner product with a nonpositive norm.) Jonas showed that the linearization of any Hamiltonian produces a Hermitian or pseudo-Hermitian operator. Since the Maxwell-Vlasov and Poisson-Vlasov systems are examples of this, their linearization can then directly be represented by a quadratic variational principle. This enabled us to extend action-conservation laws to non-eikonal situations [109].

My last postdoc was Yi-Ming Liang, with whom Gene and I initiated a study of wave emission as a conversion process [110].

After Robert Littlejohn was appointed to our faculty in 1983 and joined our research group at LBL, he continued providing powerful new tools for our research, such as the metaplectic transform. In addition, his thesis students often showed great interest in our studies, and made valuable contributions. Chief among them was Jim Morehead, who continued as a postdoc. He provided crucial insight in our work on multiple mode conversion [103], and was a valuable co-worker in some five other papers. Also, Robert’s students Greg Flynn [111] and Yukkei Hui [112] joined our studies. And Kevin Mitchell patiently taught me how to use my computer.

In 1997, Darryl suggested that our mode-conversion methodology might be transferable to ocean waves, which he had been studying at LANL. In Philander’s treatise on El Nino, we found a beautiful diagram of a mode-conversion process (an avoided-crossing dispersion diagram), from work by Cane & Sarachik [113]. We proceeded to apply our methods to the problem of a spatially variable background (the depth of the thermocline), and published our first oceanography paper [114], involving energy transfer from an equatorially trapped Rossby-Kelvin wave to a coastaly
trapped Kelvin wave. Later, Brad Shadwick produced simulation movies, to test our theoretical approximations. Our work was brought to the attention of oceanographers by Vanneste [115], and then applied by Rene Tailleux to Rossby waves [116].

Having become interested in geophysical fluid dynamics, I gave a physics course on the subject, using Cushman-Roisin’s excellent textbook. We then became aware of Eckart resonance [117], involving mode conversion among internal waves in the ocean. A similar resonance occurs among Doppler-ducted internal waves in the atmosphere, studied by Dave Fritts [118]. To deal with linearization about an atmosphere with sheared flow, we needed Jonas’ generalization of Hamiltonian dynamics [119]. He had trained his graduate student Krister Wiklund, whom he sent to work with me on this problem [120].

Andre Jaun, a Swiss computational plasma physicist working in Stockholm on full-wave modeling, recognized the value of our ray-based methods. In 2000, he joined our team, proposing the implementation of our approach for realistic tokamak geometries.

I retired from teaching in 1998, having started in 1959. Preparing my lectures and homework assignments was a wonderful stimulus to deeper understanding. Each time I taught a course, I would try to dig deeper. Without teaching, my research would surely have suffered.

13. Colleagues

Wulf Kunkel was group leader of the plasma program at LBL from 1971 to 1991, and was my plasma colleague on the Physics Department faculty. Over these many years, this good friend was a continual source of valuable advice, as we discussed problems in physics and in the real world of people.

Andy Sessler was my theoretical colleague for many years at LBL, and director for part of that time. As a leading accelerator theorist, he called my attention to Chirikov’s pioneering work on chaos. His students Jim Hammer, Paul Channell, Jonathan Wurtele, & Jin-Soo Kim were active members of our group.

In our campus EE department, Ned Birdsall trained all my students in the plasma simulation techniques which he and Bruce Langdon pioneered. One homework project for Dwight Nicholson, the motion of a charged particle in an electrostatic wave packet, led us to the concept of oscillation-center velocity. His colleagues Allan Lichtenberg and Mike Lieberman taught a valuable course on MHD, and wrote a definitive textbook on Hamiltonian dynamics.

Oscar Manley for many years provided our grant support from Washington, both in fusion and in basic energy science. He was most remarkable in his deep understanding of our research, based on his strong physics and math background. After retirement he co-authored incisive work on turbulence, with Foias and Temam. His mathematician friend Yvain Reve helped us make valuable contact with the math community, in particular with Alan Weinstein and Jerry Marsden.

Over the years, short-term visitors provided valuable insights and knowledge. Chief among them were John Krommes, with his sophisticated approach to turbulence and broad understanding of plasma theory, and Jonas Larsson, who introduced powerful new mathematical tools.

I feel very fortunate to have found a career in a field where I could grow with the field itself. I have had exceptionally bright students, congenial stimulating supportive colleagues, and a stable and secure institutional home. My wonderful family have fulfilled my life: my wife Louise, our son Joel, our daughter Janet, our son-in-law Tom, and our grandsons Alexander and Aaron.

It has truly been a happy time.

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