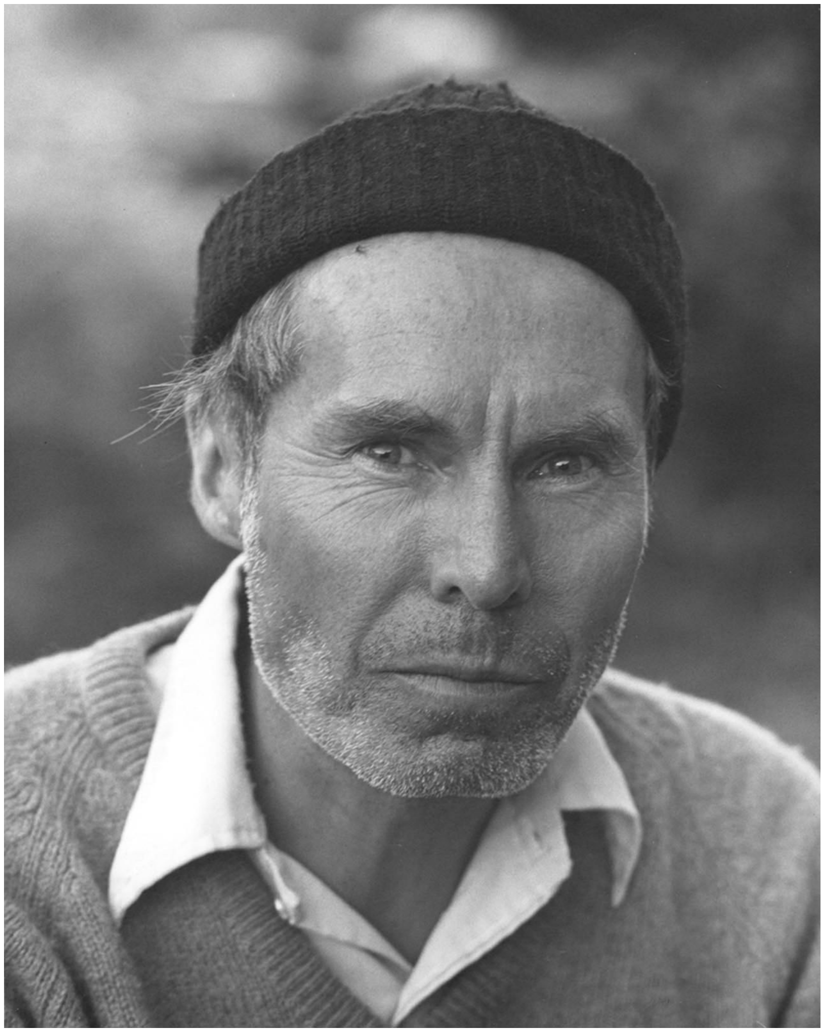

PHOTO CREDIT: KURT GOTTFRIED

JS Jackson

Annu. Rev. Nucl. Part. Sci. 1999. 49:1–33

SNAPSHOTS OF A PHYSICIST'S LIFE

J. David Jackson
Department of Physics, University of California and Lawrence Berkeley National Laboratory, Berkeley, California 94720; e-mail: jdj@lbl.gov

CONTENTS

INTRODUCTION

My career as a professional, card-carrying physicist has spanned the golden years. At my beginnings, right after World War II, physics was a calling, not a profession. The wartime accomplishments of physicists brought public recognition and hastened change. Military hardware, especially radar, was turned to effective civilian use and exciting discoveries in many fields followed. Science in general boomed, students came, and physics became a profession of tens of thousands, not hundreds.

In the following decades, my main chosen fields of first nuclear physics and then particle physics flourished. Bigger laboratories, more specialized accelerators were built. Experimental and theoretical discoveries jostled for preeminence. Both fields are now mature and in some sense musclebound. Accelerator complexes and experiments/detectors demand huge investments of time and money. For me, the death of the Superconducting Super Collider (SSC) in 1993 was a signal that times have changed, at least in particle physics. The golden days of rapid and spectacular progress on a broad front are over. Isolated brilliance will flash, no doubt, but for most of us the bloom is off the rose. You may view my pessimism

0163-8998/99/1201-0001$08.00

as the biased opinions of a spent old man. I hope, for your sake, you are right. In any event, I am grateful to have participated in a most exciting time in the history of physics.

My somewhat unusual background (growing up in Canada, attending a college that taught me classical physics and mathematics well, but little else, and doing graduate work at MIT) gave me a broad grounding that served me in good stead. It may again be an oldster's lament, but I believe the narrow specialization of our graduate and even our undergraduate training today diminishes a physicist's, especially a theorist's, ability to respond to the new and unexpected.

In this prefatory chapter, I present a number of "snapshots" from my career. Some are intended to give the reader a glimpse of what it was like in the "good old days" (which seem always to be in the youth of the writer). Some describe exciting, if brief, moments of particular significance to me. Going along with the "snapshots" are actual snapshots intended to illustrate the text.

BEGINNINGS

Born in London, Ontario in 1925, I attended local public schools and then the University of Western Ontario (1942–1946). In high school, I had an interest in chemistry and physics and in mathematics. Physics was badly taught by a teacher with a PhD, but chemistry was made fascinating by a fine teacher, Mr. McCallum, and analytic geometry had another fine teacher, Mr. Hall.

Western is located on what was then the northwestern edge of London, in a beautiful setting of low rolling hills, surrounded by a golf course (now largely consumed by the campus). In those days it had three buildings, a stadium, and 1800 students. First-year science students took four sciences and two or three math classes, plus humanities. Chemistry had a kind but boring lecturer and disappointing, qualitative labs; physics, taught enthusiastically by the department head, was at least more quantitative and mathematical. Botany and zoology were endured. By the end of the year I determined to major in Honours Physics and Mathematics.

The second World War was raging during three of my five years in high school and three of my four undergraduate years at Western. Science students were deferred from military service but participated in the Canadian equivalent of the Reserve Officer Training Corps (ROTC). I was in the University Air Training Corps. As I recall, we met after classes for an hour or two each week in uniform for drill and instruction in navigation and aircraft recognition. Summer camps were held at nearby air bases, where we trained with regular recruits and flew for hands-on navigation training in ancient Avro Anson aircraft.

In addition to military camps, my summers involved various temporary jobs. My most vivid memories are of work at the refinery of the International Nickel Company, applying layers of foam rubber sheeting to the exterior of Inconel-metal fuel tanks for PT boats (to retard leakage when penetrated by bullets). The fumes from the rubber cement were horrendous. More than a month or two at the job would have damaged one's health. We also substituted in servicing the acres of electrolytic tanks where the raw nickel sheets were grown. Despite wooden floorboards, rubber boots, and gloves, electric shocks were routine as we replaced ruptured canvas frames that surrounded the sheets and cleaned the buildup of nickel sulphate crystals from the tubes circulating the electrolyte.

The physics curriculum at Western was almost entirely classical. Freshman or sophomore physics lab included precision of measurements and the use of a planimeter for measuring areas. Our lab planimeters were simple brass rods bent into the three sides of a rectangle, with a point at one end and a flattened, axe-like tip at the other. There were, of course, a few professional instruments with wheels and a calibrated scale. I recall my initial amazement at the displacement of the wedge-shaped end of the planimeter on the paper as the point at the other end was made to trace out the perimeter of the area being measured. Such a simple gadget, based on such a simple principle! Not seen by students today, I am sure.

Laboratories were old-fashioned, with ballistic galvanometers and quadrant electrometers. Figure 1 shows the author measuring hysteresis in Professor Allen's lab. The frustration of trying to get a finicky quadrant electrometer to work gave me immense admiration for early researchers in radioactivity, for whom it was a major tool. Some of the faculty were doing wartime research on radar. There was a strong emphasis on electricity and magnetism. One optics and spectroscopy course touched on "modern physics" via the Bohr atom, but that was all. In mathematical

Figure 1 The author at the lab bench peering into the telescope of a ballistic galvanometer, taking data on magnetic hysteresis, 1943.

physics, there was a fine junior-year course taught by Gar Woonton, who later went to McGill before returning to Western in retirement. Most of the course was on Fourier series and their applications, using an ancient book by Byerly. I recall the fascination of the Gibbs phenomenon in a series at points of discontinuity and the satisfaction of solving with Fourier series a heat conduction problem with arbitrary initial conditions. The use of orthogonal expansions has been a part of my life ever since! As a senior (1945–1946), I took graduate electromagnetism from Woonton with a group of returning air force veterans who were working for MAs. I did well enough in my courses that going on to graduate school was an easy decision, thwarting my mother's earlier hopes that I would rise to the presidency of a local insurance company by becoming an actuary there.

Not surprisingly, the emphasis on electromagnetism and the radar research of the faculty (which became openly known in my senior year) pointed me in the direction of the Massachusetts Institute of Technology (MIT), because of the fame of its Radiation Laboratory and because Julius Adams Stratton's book was then my bible. Stratton was a professor of physics at MIT and director of the Research Laboratory of Electronics (RLE), the successor to the MIT Radiation Lab. He later became President of MIT. In contrast to present practice, I applied only to "Tech" for graduate study. By some miracle, I was admitted. In later life, I learned that prominent universities occasionally take a chance on admitting a student from an obscure institution. Evidently, I was one of those.

GRADUATE SCHOOL

MIT asked me to come down in June 1946 to take two undergraduate summer courses to fill in my background—"Atomic Physics," taught by Hans Mueller, and "Thermodynamics," taught by nuclear physicist William W. Buechner. At MIT, every graduate student is a Research Assistant right from the start. I was assigned to RLE (perhaps on the basis of my stated ambitions) and showed up on a very hot day in June to check in with Director Stratton. I was ushered into the great man's office and told to sit down. Stratton was a big man with a round head, chubby cheeks, and a very warm and friendly manner. The RLE building was a "temporary" wartime structure, part of the MIT Radiation Lab, without air conditioning. It was hot as Hades in Stratton's office. He asked the secretary to bring two Cokes from the machine in the corridor. We sat and drank our Cokes and chatted about what I hoped to do. He then sketched a small problem on radiation pressure for me to work out and sent me away. I came back the next day with the solution. He seemed satisfied and assigned me to work with his Electrical Engineering colleague, Lan Jen Chu. Chu set me to work on a field theory of traveling wave tubes, newly invented at Bell Labs. Thus, my first publication was a paper with Chu in the *Proceedings of the Institute of Radio Engineers*.

During the academic year 1946–1947, I took, among other courses, mechanics and electromagnetism from Slater and Frank (one for each semester). This was a course for MIT seniors, but it was taken by most incoming grad students. Frank was a typical absent-minded professor type, slightly disheveled, and rather casual in his lectures. In contrast, Slater was organized, meticulous, precise, always in suit and tie. He lectured without any notes. He would end a lecture as the bell rang and would pause to take a file card from his suit coat pocket to jot down the last equation on the board. Next lecture, he would enter the room, glance at the file card, and begin his lecture. Slater was Chairman of the Department and Frank the Executive Officer when I arrived. Slater was the author of many textbooks. He was legendary for writing them at the typewriter without notes. I have a memory of seeing him in his office at the typewriter. His books were criticized for being far too "talky," surely a result of his methods—a failing, I fear, of all of us who use word processors.

The course I enjoyed the most was Victor F. Weisskopf's quantum mechanics. Viki had been hired from Los Alamos. He arrived at MIT sometime during 1945–1946; the quantum course in 1946–1947 was his first teaching assignment. For me, it was a wonderful introduction to modern physics. My inadequate background in modern physics had been filled partially during the summer, but I had a lot to learn and soaked it up from Viki. I was an efficient note taker and was often able to correct for myself Viki's numerous trivial blackboard mistakes as I went along. Several of my fellow students were quite unhappy with Viki's slipshod board work and eager to see my notes after class to fill in the gaps. In retrospect, the course was undoubtedly a fairly standard course in nonrelativistic quantum mechanics at the level of Schiff's book, but for me it opened new vistas.

By the end of academic year, I had decided to leave RLE and Chu and transfer to Viki's nuclear theory group in the Laboratory of Nuclear Science and Engineering (LNSE) if he would have me. I had done well enough in his course that he agreed to take me on. When I broke the news to Chu, he was annoyed and tried to dissuade me. He told me that theoretical nuclear physics was very competitive and that, while I was a bright young man, he doubted that I would succeed in that competitive field. He said that he knew that I was good enough to succeed in electronics and could be assured a good job with companies such as Raytheon after my PhD. I ignored Chu's advice.

During my three years of graduate studies, I lived in the MIT Graduate House on the banks of the Charles River, directly across Massachusetts Avenue from the main MIT buildings. I do not recall how much I paid for room and board, but I know my stipend from MIT was $125 per month. All was not study and research. I soon fell in with other Canadians in the department. John A. Harvey and Douglas M. Van Patter were classmates, close friends, and for a time co-owners of a decrepit second-hand car that took us (with difficulty) to the Cape beaches. Harry E. Gove and Bruce French (a Newfoundlander, not a Canadian, he always made clear) were older and married. Jack, Doug, and Harry (Figure 2) all worked at the cyclotron lab, and, as is well known, went on to distinguished careers in nuclear physics. Bruce French became prominent in nuclear theory, but in those days he was calculating the Lamb shift for Weisskopf and in combat with Schwinger and Feynman.

Figure 2 Four of the Canadian physics graduate students at MIT in the 1945–1950 period. Three experimenters and one theorist. From left to right, John A. (Jack) Harvey, the author, Douglas M. Van Patter, Harry E. Gove; Boston, 1948.

Once I had joined Viki's group of theorists, I was entrusted for day-to-day supervision to John Blatt, then a postdoc and working with Viki on a book on nuclear theory that eventually became Blatt & Weisskopf's *Theoretical Nuclear Physics*. John and I and Lawrence Biedenharn, another of Viki's students, shared a large, sunny office next door to Viki's. John, who went from MIT to Illinois and then to Australia, was an unusual man. He was disliked by many because of his bluntness and lack of social graces, but he was a kind and helpful mentor to this greenhorn. My research work was on nucleon-nucleon scattering at energies up to 20–30 MeV in the lab. Blatt had audited Julian Schwinger's lectures on nuclear physics at Harvard in 1946–1947 while I was taking Weisskopf's quantum mechanics. He wished to apply Schwinger's variational methods to nucleon-nucleon scattering, particularly the effective range expansion for the phase shift δ in terms of the momentum k,

$$k \cot \delta = -\frac{1}{a} + \frac{1}{2} r_0 k^2 + P r_0^3 k^4 + \cdots,$$

as a model-independent way of describing the data (P is the first shape-distinguishing parameter). Blatt set me to work on the *n-p* s-wave scattering problem, calculating the scattering lengths a, effective ranges r_0, and shape parameters P for a series of potentials $V(r)$ and comparing them with the data. The idea was to demonstrate that many potential shapes were equivalent descriptions of the low-energy data, provided the strength and radial scale parameters were suitably chosen (1).

Some years earlier, Gregory Breit and colleagues analyzed the low-energy *p-p* scattering data numerically with certain potential shapes. Breit asserted (tentatively) that the data preferred some shapes over others. John Blatt would have none of it. I recall a rather hostile meeting between Blatt and Breit at MIT, with Breit claiming that there was nothing new in the effective-range approach.

A few months after the encounter at MIT, I was attending the annual meeting of the American Physical Society in New York. By chance, Breit and I were staying at the same hotel. One morning he joined me at the breakfast counter. He knew me as a grad student who had the unfortunate circumstance of being associated with Blatt but was otherwise blameless. We chatted about approaches to nucleon-nucleon scattering. I think it was I who raised the topic of the "zero-range" paper of Landau & Smorodinsky, published in 1946 in the short-lived English-language *Journal of Physics*. Breit looked pained and said that his 1937 paper had contained their result (true, but it had been rather disguised and its significance unclear). He then went on to say, "And you know, at that time they were our allies!" (For the younger readers, I need to point out that Breit was a White Russian, and staunch anticommunist. Breit's remark is particularly bizarre since he must have known that Landau was opposed to the Stalin regime.)

After the work on *n-p* scattering, I wrote my PhD thesis on a painfully thorough analysis of s- and p-wave *p-p* scattering at low energies using the Schwinger variational method, also establishing the connection with the simpler derivation of Bethe. All 157 pages of the original (and three carbon copies) were typed in the spring of 1949 by my fiancée and now wife of over 50 years, Barbara Cook. (I did write in the equations, though.) With some revisions, the thesis was published in the *Reviews of Modern Physics* (2). The computations for our *n-p* paper and my thesis were done on Marchant calculators over a period of months by Barbara (Siegel) Levine and Hannah Paul, members of the computation group serving RLE and LNSE. These computations, solutions of the radial Schrödinger equation, now might take a day or two of programming and an hour of running on a Macintosh or PC.

In my time at MIT, Weisskopf was occupied mainly with the book with Blatt and with French's calculation of the Lamb shift. He supervised the rest of us with benign neglect, but he was conscientious about regular evening gatherings in which he and his students would go out to eat supper (the Window Shop on Brattle Street was a favorite) and return to his office for an hour's discussion, mostly of physics. I recall that postdocs were not encouraged to join but were tolerated provided they did not speak. I cannot for the life of me remember specifics of our discussions, but I do have a general impression of Viki as a wise, honorable, humane mentor with concerns beyond physics, shared with us on occasion (Figure 3).

Viki's involvement, if any, in the ongoing nuclear weapons program at Los Alamos was not visible to us graduate students. It is now known that, at that time, Edward Teller and others were concerned that the staff at Los Alamos had been too drastically depleted. In the fall of 1948 or spring of 1949, Teller came on a recruiting mission. Although I imagine Viki did not approve, he turned his office over to Teller for interviews. I sat on Viki's couch or walked the corridor with Edward, as did others, while he made his pitch. Fortunately, I had an easy out—I

Figure 3 Professor and pupil, 35 years later. Victor F. Weisskopf and the author, Woods Hole, 1983.

was then a Canadian citizen (and remained so for many years after)—Los Alamos was not in my future.

After getting my PhD in June 1949 and marrying Barbara, I remained at MIT as a Research Associate until the end of the calendar year. That spring I received three job offers. Bob Sachs in Wisconsin offered me a one-year postdoc position at $3600/year. Hans Bethe at Cornell made a similar offer at $3200/year. The foolishness and chauvinism of youth is evident in my acceptance of the offer by Philip R. Wallace of McGill of an Assistant Professorship in *Mathematics* at $4000/year, beginning in January 1950. With no insult intended to Phil Wallace or McGill, the best career move in 1949 was obviously Bethe and Cornell!

McGILL

It was thanks to Ernest Rutherford that when I joined the McGill faculty it was as a member of the Mathematics Department. In the Cambridge tradition, Rutherford held that theoretical physicists were not physicists but applied mathematicians, and so belonged outside a Physics Department. McGill held Rutherford's legacy dear. The North American practice of theorists in physics departments has since taken hold at McGill. But not in my time.

Phil Wallace, who had worked during the war on neutron diffusion at the Montreal labs, was a theorist with wide interests including condensed matter. Now retired in British Columbia, he was an important figure in Canadian physics. Phil was an excellent teacher and inspiration to students and junior colleagues alike. As our leader, he protected the three, then four, of us quite well from the difficulties of being citizens without a country, sometimes in very peculiar quarters—in the attic of a Victorian mansion housing the Arctic Institute and later in a corridor of offices added to a new building as an afterthought, with access initially only through the building's fan room.

I began at McGill in the second half of the 1949–1950 academic year, teaching graduate theoretical nuclear physics while still wet behind the ears. We theorists taught our share of partial differential equations for engineers, but more important, we also taught the theoretical physics courses in the upper-division and graduate physics program. Classical mechanics, electromagnetism, methods of mathematical physics, quantum mechanics, and nuclear physics were among my teaching assignments. Teaching loads were heavy by present standards—two courses one semester and three another was typical, albeit most were two lectures per week. With such teaching loads, there was little time for research except in the summers. With a budding family, we spent several summers at Chalk River, the Canadian atomic energy laboratory. A mixture of service to experimenters and independent research proved pleasurable, particularly in the verdant surroundings of Deep River, the "company" town, and the Ottawa River.

The theory group under Wallace attracted a number of MS and PhD students. Kurt Gottfried was Wallace's MS student before going to MIT for a PhD, but he took quantum mechanics and electromagnetism from me. Among my MS students were Seymour H. Vosko, who became a condensed-matter theorist, ultimately at Toronto, and Hubert Reeves, now a prominent astrophysicist in Paris. My first PhD student was Harry Schiff, later a professor at the University of Alberta, as was Donald Betts, my second.

Two incidents in my research at McGill illustrate the small joys and pains we all have felt in our careers. The joy, really quite small (but it did not seem so at the time), occurred one night at home when I was attempting to do a rather complicated calculation of atomic charge transfer by protons in hydrogen. I was fighting with an intractable integral when it dawned on me that I could Fourier-transform it and use a three-dimensional version of Feynman parameterization to do the integrals! I still recall the thrill and the intense work until 2 or 3 AM, when I accomplished my goal and had a cross section in explicit, if involved, closed form. What a feeling of pleasure and accomplishment! The feeling was reprised some months later when I found that workers in Britain had independently attacked the same problem but had settled for numerical computation of the integrals. Such are the small pleasures of theoretical physics!

The pain occurred a few years later when, stimulated by Robert E. Bell (a nuclear experimenter, co-inventor of the early Bell-Petch fast coincidence circuit, and later Vice-Chancellor of McGill), I attempted a calculation of weak radiative electron capture, a process that allows determination of the transition energy from the end point of the photon spectrum, not possible without the radiation. I had done a nonrelativistic second-order perturbation calculation with plane-wave intermediate states and had prepared a draft of a short paper when the latest issue of the *Physical Review* arrived. In it was a brief but impressive Letter to the Editor on the same subject by Roy Glauber and Paul Martin, full of relativistic Coulomb Green's functions, far superior to my miserable effort. What a letdown! At least I had chosen to investigate a topic of some interest.

Although I was in Mathematics, my interest in experiment led to close association with parts of the Physics Department. In the 1950s, John Stuart Foster (whose son of the same name was at one time Director of Livermore and served in the US government) built a 100-MeV cyclotron, copied from the Harvard machine and housed in a separate building called the McGill Radiation Laboratory. Gar Woonton's McGill Electronics Laboratory was built next door. Foster gave me an office in his lab, where I spent most of my non-teaching hours. Foster was a vigorous, gruff, no-nonsense individual who, as a graduate student at Yale in the 1920s, had made the first measurements of the Stark effect in helium, and with the aid of Heisenberg's matrix mechanics had given the theoretical interpretation. He had not much followed the developments of quantum mechanics since those days. His quantum mechanics lectures began with, "We take these matrices ...." It was unfortunate that in those days there was very little support in Canada for basic research in universities. Foster had spent most of the available funds on the cyclotron. There was virtually no money and no space for experiments. Some nuclear physics got done, but lack of resources and facilities meant the program was never at the frontier.

Lack of resources, even travel funds, caused a feeling of isolation from the centers of physics activity south of the border. My connection with the nuclear physicists led one year to an invitation to the Gordon Conference on Nuclear Chemistry in New Hampshire (Figure 4), where I gave a talk on a simple theory of (p, xn) reactions at energies up to 100 MeV and first learned of the nuclear chemists' addiction to poker.

PRINCETON, MUON-CATALYZED FUSION, AND TIME-REVERSAL INVARIANCE

In 1956–1957, I spent a sabbatical year in the Department of Physics at Princeton University on a Guggenheim Fellowship. It was a year that changed my life. In retrospect, I appreciate more than ever that my wife, Barbara, with four very small children to look after (the youngest born in Princeton a month after our arrival), did heroic familial service while I did physics enthusiastically at the Palmer Lab. Despite the family ties and the physics, we managed often to take advantage of the theater and concerts in New York, thanks to the excellent train service and to the babysitting services of the young women at a local choir college.

My proposal to the Guggenheim Foundation envisioned nuclear physics research with John Wheeler and Eugene Wigner. When I arrived in September to occupy the absent Arthur Wightman's office, I had an interview with Wheeler. He

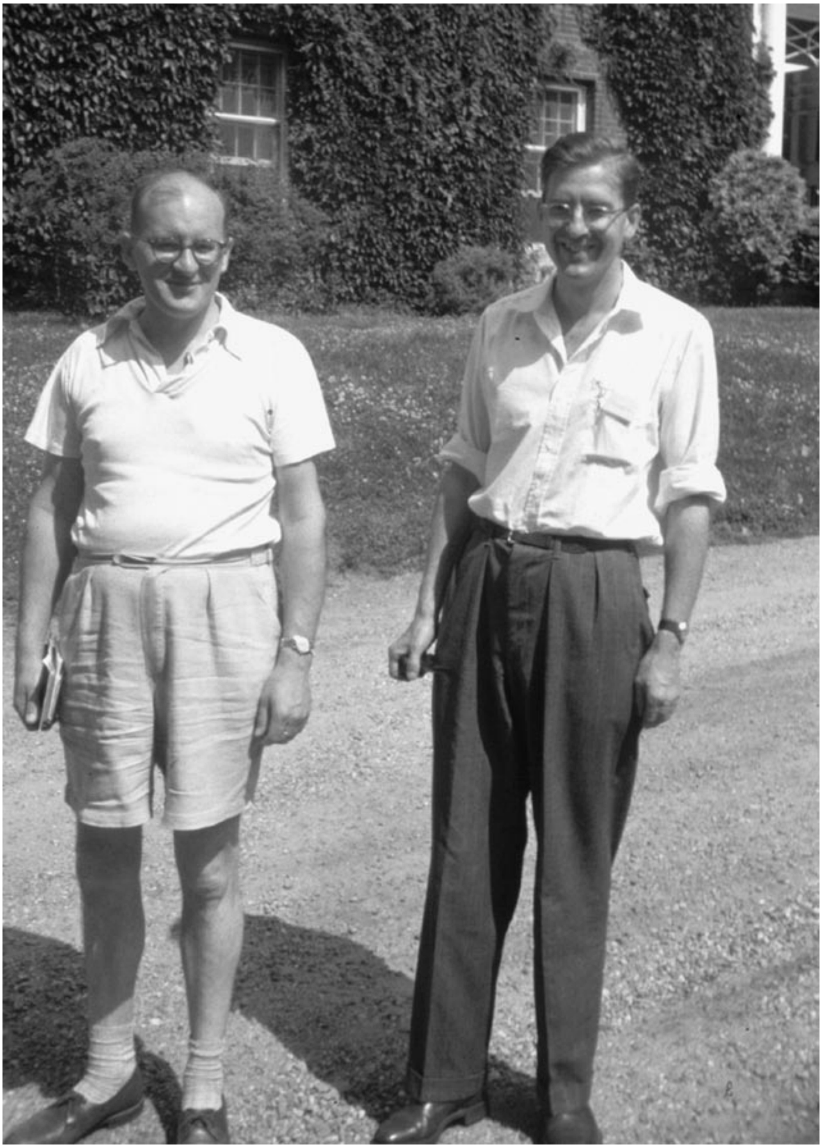

Figure 4 Anthony Turkevich, University of Chicago, and John Robson, Chalk River Laboratories, at the Gordon Conference on Nuclear Chemistry; New Hampshire, 1955. Robson made the first measurements of the beta spectrum from neutron decay.

suggested a number of nuclear physics topics, including a prescient suggestion of the collisions of heavy nuclei to produce states of very high angular momentum. During the fall, I worked on some of these suggestions without visible progress, but I soon got diverted into neighboring areas.

In December 1956, two rumors and more began to circulate. One was that Luis Alvarez and colleagues had discovered a new particle, a "mu primed" meson, in their bubble chamber. The other was that Madame Wu and her National Bureau of Standards colleagues, pursuing the suggestion of Lee & Yang, had observed parity nonconservation in the beta decay of polarized ^{60}Co nuclei.

One of the virtues of living in Princeton then was the opportunity to have the *New York Times* delivered to your doorstep daily. At breakfast on December 29, I was fascinated by Arthur Sullivan's account in the *Times* of the west coast meeting of the American Physical Society at Monterey the day before. Sullivan featured the discovery of muon-catalyzed fusion of $\mu^- pd \rightarrow {}^3\mathrm{He}\,\mu^- + 5.5$ MeV by Alvarez et al—the true explanation of the rumored "mu primed" meson. As science reporters often do when encouraged by the scientists, Sullivan waxed eloquent about possibly limitless production of energy by this new fusion process, which potentially recycles the muon over and over again. The whole idea was captivating. I wanted to know more, to do some quantitative estimates of key aspects, to see whether the speculations made sense.

Over that Christmas–New Year's period, I went to Palmer Lab and worked happily, if feverishly. The lab was deserted. No colleagues to talk to, but also no distractions. I investigated the nuclear fusion rates in muonic diatomic molecular ions, the capture of the muon by the moving helium fragments after fusion, the possibility of liberation during the slowing down of the fragment, as well as speculations on energy production. My most important conclusions for d-t fusion were that after molecular ion formation the nuclear reaction rate is extremely fast ($\Gamma \geq 10^{12}\,\mathrm{s}^{-1}$), and that, whatever the rates of molecular processes, there is an upper limit of the order of 100–130 on the number of possible fusions caused by one muon (because of its capture by the produced alpha particle, the "sticking probability"), independent of the muon's mass or lifetime. The latter conclusion negated the remarks in Sullivan's story about the efficacy of a possible longer-lived lepton. My files show that by January 5 I had the draft of a paper ready and had mailed a copy to Luis Alvarez. He replied on January 8; my paper was mailed to the *Physical Review* on January 9 (3). This story is told in somewhat more detail elsewhere (4). Unknown to me and to Alvarez until the remark of a referee was the earlier work on muon-catalyzed fusion by FC Frank (1947), Andrei Sakharov (1948), and Ya. B. Zel'dovich (1954). In the late 1950s, Semen S. Gershtein and Zel'dovich made careful calculations of energy levels of the various molecular ions.

In the subsequent forty-some years, a sizable industry has developed for the study of muon-catalyzed fusion, mostly in Europe and Russia. Much fascinating atomic and molecular physics of muonic systems has been discovered, the most impressive being the serendipitous presence of an excited bound state in the d-t molecular ion very near threshold, which causes a very rapid molecular formation rate, far faster than the muon's decay rate. This circumstance permits the observation of many cycles of fusion, in excess of 100, under the right conditions. My estimate of the "sticking probability" was refined by others to give a theoretical upper limit of about 150–170 for the number of d-t fusions per muon, still insufficient for practical energy production, despite ingenious schemes of Yuri V. Petrov and others. In the 1970s and 1980s, extensive, precise numerical computations of bound and scattering states were made in Russia by a team led by Leonid I. Ponomarev. I have enjoyed following and occasionally contributing to this cross-disciplinary field. Figure 5 shows four of the Mu-sketeers at a meeting in Sweden in 1992.

Muon-catalyzed fusion was just the beginning of my exciting year at Princeton. By January 1957, the rumors about parity nonconservation in beta decay had turned into hard fact, followed rapidly by evidence from pion and muon decay. With one discrete symmetry fallen, it was natural to ask about others and think of tests for them. Sam Treiman and Bill Wyld, who had written about pion decay, began to examine tests of time-reversal invariance (TRI) and invited me to join them. In their 1956 paper, Lee & Yang had treated the parity-violating signatures in beta decay (e.g. coefficients of $\boldsymbol{\sigma} \cdot \mathbf{p}$, $\mathbf{J} \cdot \mathbf{p}$). We decided to compute the important TRI-testing signatures [e.g. $\mathbf{J} \cdot (\mathbf{p} \times \mathbf{q})$] for allowed beta decay with the most general form of the beta-decay interaction. This was an exciting, sometimes frantic, time. We each worked independently—a good thing, too, given the algebraic complexity of the formulas and the chance of error. I recall small triumphs when I would

Figure 5 Four Mu-sketeers in Uppsala, 1992. Leonid Ponomarev, Semen Gershtein, the author, and Yuri Petrov.

get a correct answer first, using my "brute-force" approach with two-component Pauli spinors and explicit representations, while Sam and Bill were using elegant projection operators to achieve the same ends. Of course, sometimes the shoe was on the other foot. It was a fruitful and satisfying collaboration. We later had several arguments with other authors who thought we had made mistakes. To the best of my knowledge, there are no errors in our two published papers on TRI, one without (5) and one with the Coulomb corrections (6).

The unfolding story of parity nonconservation in all its profusion and confusion (because of initially conflicting evidence on the exact form of the interaction) occupied us through the spring and summer of 1957. I was invited to lecture on high-energy physics and weak interactions at a summer school in Jasper run by the Canadian Association of Physicists. Lecturers included Phil Morrison, Julian Schwinger, and Eugene Wigner (Figure 6). I recall an unusual conversation with Wigner. The Cold War was in full swing in 1957. We must have been having a political discussion and I must have made some remark appropriate for a fuzzy-minded liberal academic. Wigner said dramatically, "If they get control over here, you know intellectuals will be the first to go!"

My revised and augmented lectures appeared a year later as a book solicited by Wigner for the Princeton University Press (7). The contents reflect the state of the field at the time—details of s- and p-wave pion-nucleon interactions up to 500 MeV, the (3,3) resonance, dispersion relations, photoproduction; the phenomenology of strange particles, hypernuclei, pre-SU(3) models; discrete symmetries, beta decay, muon decay, the universal Fermi interaction, pion decay. The strong dynamics were treated in semiclassical fashion equivalent to tree graphs for the most part. The little book served a useful purpose—Martin Perl says that it played a role in his transition from atomic to particle physics—but it had a lifetime of a few years at best before the field moved on.

Memories from Princeton abound—the warm hospitality of the physics faculty and their wives, especially to Barbara and the children; the Fine Hall teas with the mathematicians; the Frisbee tossing at noontime among the trees on the sward in front of the Palmer Laboratory [introduced, if I recall correctly, by Marvin L. (Murph) Goldberger, who had come at midyear], the excursions to New York, the family trips to beaches, the excellent local doctors and Princeton Hospital (an uneventful birth and a scary illness of a child). All in all, it was a good year!

My highly successful year at Princeton had consequences. I now had some visibility in the United States, absent during my six years at McGill. That year, two stars at the University of Illinois in Urbana-Champaign, Geoffrey F. Chew and Francis Low, had decided to leave, Geoff back to Berkeley and Francis to MIT. Fred Seitz, newly named successor to Wheeler Loomis as Head of Physics in Urbana, was recruiting theorists. I was recommended to Seitz, who offered me a job. It seemed too good an opportunity to turn down. We returned to Montreal for the summer months and then emigrated to the United States in September 1957. Actually, I was the only immigrant—Barbara and the four children were all US (or joint) citizens.

Figure 6 Tea on the lawn in Edmonton, 1957. Eugene Wigner, Julian Schwinger, Ernest S. Keeping of the University of Alberta, Clarice Schwinger.

ILLINOIS AND THE GREEN, RED, AND BLUE BOOKS

Geoff Ravenhall, Bill Wyld, and I joined the faculty in Urbana at the same time, three "replacements" for Chew and Low. We were joined soon by Rudolf Haag and Kazuhiko Nishijima. My ten years at Illinois were productive and enjoyable as I completed my switch from nuclear to particle physics. The Physics Department, created largely by Wheeler Loomis in the 1930s and 1940s, was strong in nuclear physics, with a cyclotron lab and a 300-MeV betatron built by Donald W. Kerst. Condensed matter was another strength, with Charles Slichter, John Bardeen, and others. Unusual among departments, Illinois was congenial and collegial. Fred Seitz, then Gerald Almy, provided firm and fair leadership. Being in the College of Engineering was a real advantage.

Among the well-known who had been at Illinois were Maurice Goldhaber, later scientist at and director of Brookhaven National Laboratory (BNL); Willi Jentschke, later director of DESY and then CERN; and Gilberto Bernardini, director of research at CERN in the 1960s and then at Scuola Normale Superiore in Pisa. In the mid-1950s, Bernardini and Edwin L. Goldwasser and others were busy studying pion photoproduction at the betatron. James Allen was studying angular correlations in beta decay. With the parity revolution, Hans Frauenfelder and colleagues did important work on the longitudinal polarization of beta rays. Initially, my research was on aspects of beta decay, then on K-meson–nucleon interactions at low energies, K-meson decays, and final-state interactions in collaboration with Geoff Ravenhall, Bill Wyld, and Roy Schult. Two summers in Los Angeles led to work on plasma oscillations. My second sabbatical, at CERN in 1963–1964, proved very productive, as I describe below.

I taught nuclear and particle physics, quantum mechanics, and electromagnetism at Illinois. The last was taught initially by Bill Wyld, using some typed notes of mine from McGill. There I had been given the job of teaching the graduate E & M course almost as soon as I arrived. Panofsky and Phillips did not exist. The Landau & Lifshitz book, though newly available in English, was not really suitable for North American teaching methods. I therefore developed my own set of lecture notes. By the time I left McGill, they had evolved into typed sentences and paragraphs, bound and looking a bit like a telephone book. At Illinois, Bill Wyld, faced with the task of teaching the same course, did not like the existing books. When he saw my notes he decided to use them as a text, if copies could be had. The department purchased the last 50 copies from McGill.

Within a year or two, I was assigned the course and took the opportunity to revise and expand the notes, including numerous problems. As many readers know, once you have a set of lecture notes for a course, publishers' agents begin thrusting contracts in front of your nose. By 1960, having fought off all the contracts, I felt I had the notes finished enough to permit publishers to assess their suitability for publication. That year I signed up with John Wiley & Sons, Inc. and began the two-year process from manuscript to bound books. During that time, I largely deserted my graduate students to work on the book, but I gave my courses and fulfilled my other departmental duties. The first edition of *Classical Electrodynamics*, the Green Book, appeared in 1962 (8).

Classical Electrodynamics proved much more successful and durable than expected. It became a standard text in US graduate programs, its problems the bane of many a graduate student's existence. After a few years, publishers always want a new edition of a text to stymie the used-book market. Wiley was no exception. In 1970, I took a six-month sabbatical to Cambridge, England with the intent of revising the whole book for a new edition and doing some research as well. I had a grand time at the old Cavendish Laboratory with the ghosts of Maxwell, Rayleigh, JJ Thomson, and Rutherford in the corridors, but after six months had revised only two chapters! It was another five years before the second edition (the Red Book, 1975) saw the light of day. At Berkeley I have taught the course rarely—not wishing to read from my own book, I must work hard to find new material. I did keep collecting new problems and ideas for new or different treatments with the vague idea of another edition someday. Finally, in July 1997, I completed a third edition, which appeared one year later (the Blue Book, 1998).

The book's longevity is the origin of the following story from Kurt Gottfried. Kurt was teaching the graduate E & M course at Cornell ten or more years ago. He had gotten to the density effect in energy loss and found the discussion a bit opaque. He went to Hans Bethe ("Mr. Energy Loss") to ask him whether there was a simpler, more intuitive explanation. Bethe said no. Kurt then telephoned me to ask me whether I could help. I said it was in my book. At his next lecture, Kurt told the story of going to Bethe and getting no help and then calling me. At that point he noticed strange looks on some of the students' faces. He stopped and said, "What is wrong?" The class replied, "Jackson? We thought he was dead!"

SUMMER SCHOOLS: Scotland, 1960

In 1960 I began my decade or more of lecturing at summer schools—Edinburgh (1960), Brandeis (1962), Les Houches (1965), Edinburgh again (1973). The 1960 Scottish Universities' Summer School (Figure 7), held at Newbattle Abbey near Edinburgh, was the first of that continuing series. It was on dispersion relations. I was asked to give the introductory lectures. Other lecturers were Geoffrey F. Chew, William R. Frazer, Sergio Fubini, JM Jauch, John Polkinghorne, Michael Moravcsik, and Walter Thirring. Bill Frazer was a last-minute replacement for Murph Goldberger, and the local committee of Scots had not been able to complete all the preliminaries of travel expenses, etc with him. The committee had budgeted $600 to cover Bill's airfare from California. When he arrived the day before the school began, they found that Bill had received an NSF travel grant and did not need their NATO money. The committee held an emergency meeting and decided to use the windfall to provide wine at dinner for all the participants for the three-week school. The tradition begun by happenstance in 1960 continues, as far as I know, to this day. Because the wine was being delivered by an Edinburgh merchant once a week, we

Figure 7 John Polkinghorne and Michael Moravcsik, Scottish Universities' Summer School, Newbattle Abbey, Scotland, 1960.

lecturers were given the opportunity to purchase and have delivered libations of our choice. I began my introduction to single malt whiskies with the advice of a Scot.

Newbattle Abbey was actually a Victorian mansion built on the remains of an old abbey. The entrance hall led down into the basement area and up to the ground floor and the bedrooms above. At the foot of the downward stairs was a grandfather clock, with the dining room and kitchen on the left and a lounge called the crypt on the right. The Scottish women serving the meals allocated the same two or three bottles of wine to each table at dinner, regardless of the composition of adults and children at a table. After the meal, they retrieved whatever wine was unused for future use. A group of "students," including Sheldon L. Glashow, and I soon realized that we should sit at a dinner table with children, often the Polkinghornes, surreptitiously lower a bottle of red wine to the leg of a chair during the meal, and on the way out hide it in the bottom of the grandfather clock. At the 10 PM signal of clanging keys as the big front door was locked for the night, the group would descend from their rooms to gather in the crypt for wine and conversation. A night or two after we began this secret ritual, a plate of biscuits and a set of glasses began to appear each night on the fireplace mantle—secret ritual, indeed! It was there that I met for the first time Martinus Veltman, a student at the school, and learned from him the pleasures of Médoc wines. Lest you think that the summer school was nothing but carousing, I hasten to say that Nicholas Kemmer, Director of the School and the Tait Professor of Natural Philosophy at Edinburgh, and his chief of staff, Mrs. Rae Chester, kept the lecturers' noses to the grindstone, writing lecture notes for distribution (and later publication) and giving lectures.

The lectures are collected in a book, *Dispersion Relations* (9). Nowadays dispersion relations are taught, if at all, only as a useful tool for some field-theoretic calculations. In the 1950s and 1960s, they occupied a much more central position. There was no real theory of strong interactions. Theorists attempted to exploit analyticity in the general S-matrix theory of scattering amplitudes to determine fundamental aspects of the interaction. Our dispersion relations were the generalization of the Kramers-Kronig relations of optics. For example, the real part of the forward amplitude of pion-nucleon scattering can be written as an integral over the π-N total cross sections, plus possible discrete "pole terms" below the physical threshold. In $\pi^- p$ scattering, there is a neutron pole term not far below threshold. Scattering measurements and dispersion relations permit an estimate of the residue at the neutron pole, the residue being related to $g^2/4\pi$, the pion-nucleon coupling constant. In those days, this strong coupling parameter was only crudely defined as the strength of the longest-range part of the nucleon-nucleon static potential. The residue definition gave the π-N coupling a well-defined meaning without off-shell ambiguities, albeit at an unphysical point in the energy plane. Readers who consult *Dispersion Relations* will find discussion of rigorous proofs based on causality and retarded commutators, application to scattering, and use of the Mandelstam representation, a generalization of ordinary dispersion relations, to discuss strong interaction dynamics and form factors—topics in the fore of particle physics 40 years ago.

ROCHESTER CONFERENCES: Kiev, 1959 and 1970

In addition to summer schools, there were "Rochester" Conferences to attend. I single out the two Kiev conferences (1959, 1970). The 1959 meeting was the second to be held outside Rochester. The USSR was anxious to show off its high-energy physics prowess—the 10-GeV Dubna machine had turned on (although not well) two years before. Western physicists went in great numbers, eager to see the Soviet Union and its scientists. Most of us from the United States flew via Greenland and Iceland to Copenhagen. The Los Angeles–Copenhagen trip took 25 hours. Figure 8 shows some US delegates and locals stretching their legs in Copenhagen. The meetings themselves were up to the usual standards of international HEP conferences; the milieu was different, full of contrasts. Shoddy civil construction and nineteenth-century technology jostled for our attention with advanced Tupolev 104 jet passenger planes, patterned after a military bomber, just as were our Boeing 707s. Not all Soviet aircraft were so advanced. On my flight from Leningrad to Helsinki on the way home, the plane was a copy of a DC-3. As we prepared to take off, the stewardess came down the aisle handing out small waxed paper envelopes for fountain pens; when I went to fasten my seat belt, one end came off from the wall!

Figure 8 Delegates Henry Kendall (*front left*), Sidney Drell (*front right*), Don Yennie (*rear left*), in transit to Kiev, with Copenhagen residents Sorel Gottfried, Kurt Gottfried (*front center*), and Frank Tangherlini; Copenhagen, July 1959.

Almost all the prominent Soviet theorists and experimenters were there—Andrei D. Sakharov was not in 1959 but was in 1970. We had the opportunity to hear them and talk with them, both during the sessions and on the day-long excursion along the Dnieper River. Figure 9 is a montage of seven familiar Soviet and Western participants in 1959 and another four in 1970.

Our 16-year-old daughter Maureen, who had studied Russian, accompanied me to the 1970 meeting. We were at CERN for the summer. After extensive correspondence over a year or more, at nearly the last minute I received a message from a Mr. Bojko of the Mir Publishing House that "payment awaits you in Moscow." The one-time payment of some unknown amount of rubles was for the translation of the first edition of my book, *Classical Electrodynamics*. I had not previously heard of Mr. Bojko, but on the chance that the amount would finance our trip, we made our hotel reservations through Cook's at CERN and obtained our visas as part of the CERN contingent, without the usual requirement of Intourist payment. (As insurance, we had enough travelers' checks to cover us in case the Mir well came up dry.) The hotel "Russia" in Moscow had record of our reservation, but we reached an impasse with the clerk when she found we had no Intourist vouchers! Impossible! After half an hour of waiting, we saw a middle-aged woman in mannish garb descend a staircase, stride across the lobby, extend her hand to me, and say, "Ah, an author!"

The next day I took a taxi to the Mir headquarters in the suburbs of Moscow in the midst of pasture land with cows grazing. A translator helped me persuade the taxi driver to wait while I conducted my business and directed me to Mr. Bojko's office. There I met Bojko and A. Sokolow, their physics advisory editor. It turned out that Sokolow spoke Russian and German. Bojko spoke Russian and French. My side was conducted in my rudimentary French. We had some polite conversation about the length of my stay and the advantages of opening a bank account for my rubles in Moscow rather than Kiev. At that time, withdrawals had to be made in person, and they observed that I was more likely to be back in Moscow than Kiev. Bojko then summoned his secretary, who produced a paper for me to sign and the payment in cash. It turns out that Mir pays by the page or fraction of a page, when it pays non-Soviet authors at all. Bojko ceremoniously pushed the stack of ruble notes and spare change in kopecks across the table to me after I had signed. I packed away the cash, thanked them profusely, told them I would think about the idea of the bank account while at the conference, and took my leave. Needless to say, on being delivered back to the "Russia," I tipped the taxi driver liberally.

In our hotel room, Maureen and I counted the notes and determined that we could finance our stay and more. In fact, we had difficulty spending because our hotel rooms, paid for in rubles, were at half the Intourist rate! Liberal purchase of souvenirs, loans of rubles to friends so that they did not have to cash their dollar travelers checks, and purchase of return air tickets allowed me to bring the excess funds home, despite the ban on exporting rubles. I did miss the chance of having a bank account in Moscow.

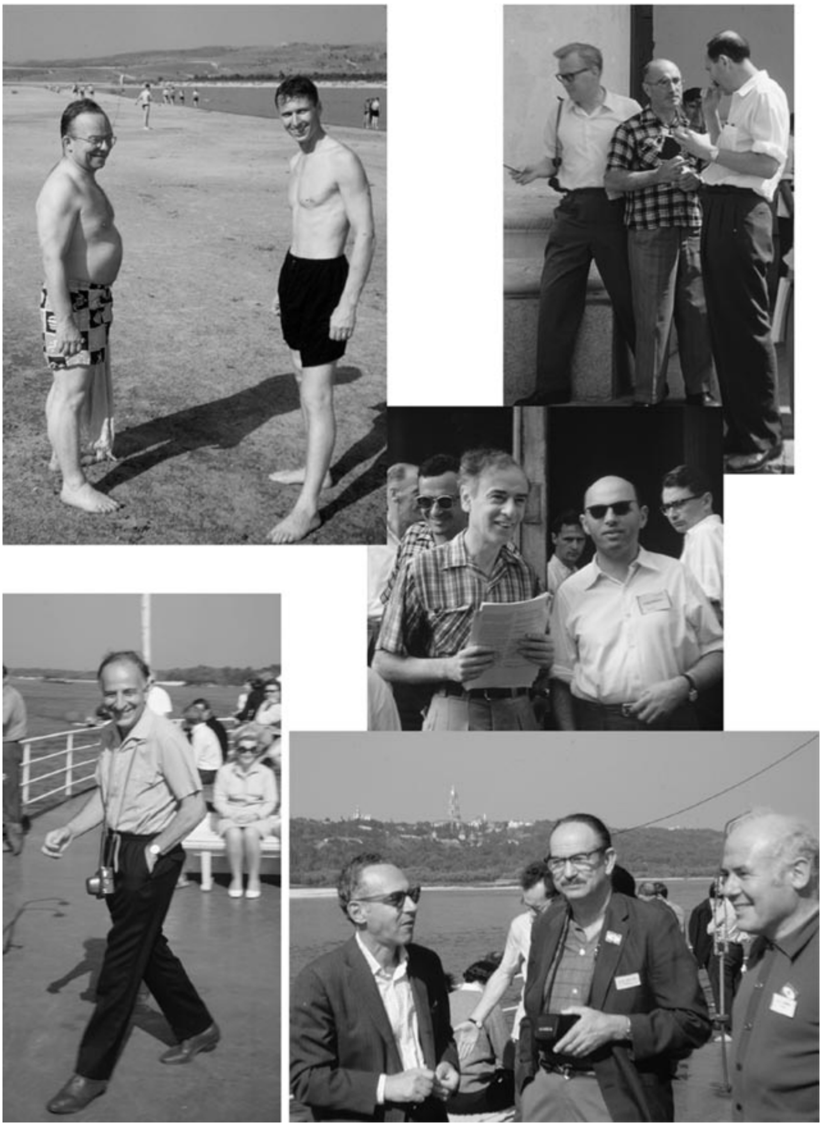

Figure 9 (*top left*) WKH (Pief) Panofsky and Roger Hildebrand on the shore of the Dnieper (1959). (*top right*) Gösta Ekspong, Ya B Zel'dovich, and Robert Marshak (1959). (*center*) Lev Landau and Isaak Khalatnikov at the Kiev conference hall (1959). (*bottom left*) Bruno Pontecorvo, his wife Marianna seated behind, Kiev excursion (1970). (*bottom right*) Maurice Goldhaber, Edwin M McMillan, and Harry Lipkin (1970).

Some comments about the Pontecorvos: Bruno Pontecorvo was someone I had known from afar at Chalk River in 1947. Handsome, flirtatious, very Italian, he was the heartthrob of all the single women in Deep River. He and his beautiful, vivacious Swedish wife Marianna were prominent on the tennis courts. At Kiev in 1970, "Ponte" and his wife were at the same hotel as we. We shared a breakfast table more than once. Ponte seemed much the same (we all grow older and don't notice each other aging), but Marianna was a shock. I had heard that she did not wish to go to the Soviet Union when they fled Britain and hated it once there. Ponte spoke animatedly about the days in Chalk River; Marianna said nothing. She seemed downtrodden and discouraged, not the vibrant Swede I remembered. Ponte had had his physics to sustain him amidst the difficulties of Soviet life. Marianna evidently had nothing.

SABBATICAL AT CERN, 1963–1964

I was granted a sabbatical leave from Illinois for 1963–1964 and chose to spend it at CERN, by then a vigorous international experimental research center with a distinguished theory group. It was the first of many productive visits. Thanks to CERN, we lived in a wonderful old house (42, route de Chêne) about a mile east of the center of town on the No. 12 tram line. Our family, with children aged 6 to 12, had a marvelous year in Geneva (a safe and simple city for children to navigate), in Switzerland, and throughout Europe. Professionally, I enjoyed the year, too.

Soon after arrival, Kurt Gottfried and I began to collaborate. Bubble chambers were busy all over the world studying hadronic interactions with beams of momenta up to 10 or 12 GeV/c. CERN had a strong group studying K^+p reactions. Production of resonances (ρ, ω, $K^*(890)$, ...) at small momentum transfers was a prominent feature of the data. The possible information available in the angular distributions of decay of these resonances seemed a fruitful area to explore. The paper on helicity amplitudes by Maurice Jacob and Gian-Carlo Wick (10) was generally known by then. A new paper on the crossing relations of helicity amplitudes (e.g. from s-channel to t-channel) by Larry Trueman and Wick (11) had just appeared in preprint form that fall. I recall Kurt bringing a copy of the Trueman-Wick paper to me and saying, "This seems important. We should try to understand it." So, with some initial difficulty on my part, we did. The key ideas for elucidation of the production mechanism in peripheral collisions are to (a) choose the momentum transfer direction in the rest frame of the decaying resonance as the z-axis, and (b) use the density matrix to describe the angular distribution of the decay. The choice of axes became known as the Gottfried-Jackson axes, and later, to my embarrassment, as the Jackson angles. Our *Physics Letter* in December 1963 (12) and a more detailed *Nuovo Cimento* paper (13) a month later established a technique that is still in use today.

Although the most prominent features of the decay angular distributions yielded strong evidence on the spin-parity of the t-channel exchanges, predictions of the lowest-order diagrams did not agree quantitatively with all aspects of the data. Pion exchange in particular was a problem. The Born amplitude,

$$\mathcal{M} = \frac{t}{t - m_\pi^2},$$

while possessing many s-channel partial waves that give a strong forward peaking of the cross section at modest t values, has a zero at $t = 0$, just outside the physical region, not present in the data. The Born amplitude can be rewritten as

$$\mathcal{M} = 1 + \frac{m_\pi^2}{t - m_\pi^2},$$

showing that the zero at $t = 0$ is caused by an extra $\ell = 0$ contribution in the s-channel in addition to the "normal" partial waves from the pion propagator. Once it dawned on us that the "bad" behavior was caused by one low partial wave, we realized that initial- or final-state interactions could modify the features of the basic exchange in the cross section and presumably in the decay correlations. From nuclear physics, Kurt was familiar with the optical model, with its complex potential to simulate the effects of competing channels, and the eikonal approximation of Glauber. We adapted them to incorporate absorptive effects into the peripheral production processes, using the high-energy elastic scattering data to parameterize our empirical gaussian absorptive potential (14). Removal of most of the lowest partial waves by absorption seemed to correspond to reality—the cross section shapes and the decay density matrix elements as functions of momentum transfer were predicted well, at least in situations with an obvious t-channel exchange. The absorptive peripheral model, in our version or modifications by others, together with the choice of t-channel axes and the density matrix, became a standard tool of both theorists and experimenters for many years.

I had numerous invitations to give seminars and talks at meetings during the year. One meeting, in Oxford in April 1964, was the occasion for a day of touring the Cotswolds, arranged by Dick Dalitz. Figure 10 shows the famous collaborators, John C. Ward and Abdus Salam, taken on that outing. Mention of John Ward (of the Ward identities of quantum field theory) leads to a story. My colleague from Illinois, Bill Wyld, was in Oxford on sabbatical leave while I was at CERN. It happened that Ward, Wyld, and I had dinner at The Trout, a historic pub near Oxford, famous as a stopping place for Henry VIII. I do not recall the meal, but I do recall the wine, a Gevrey-Chambertin 1958. It was superb. As we ate and drank and talked physics, John Ward, a brilliant, mercurial man, became very excited in telling us about a collaboration with Salam on electromagnetic and weak interactions. He thought they were on the verge of solving the whole problem, if they could only find the right matrix of transformation! He scribbled madly on a paper napkin and urged us to help him find the right matrix. We finished the wine but didn't find him the matrix. I still have the wine-stained napkin among my memorabilia. Salam & Ward's paper later that year (15), just before the Higgs mechanism, is an early milepost on the road to Salam's Nobel Prize, shared in

Figure 10 John C. Ward and Abdus Salam, Cotswolds, England, April 1964.

1979 with Glashow and Weinberg for the development of the electroweak theory. John Ward, then on leave from Johns Hopkins, soon based himself at Macquarie University, Sydney, Australia until his retirement.

Two incidents at CERN that year stick in my mind, each illuminating character in one way or another. My office was one of many along the east-west corridor that runs from the library to the Theory Division secretariat. Soon after my arrival, Nino Zichichi came by to ask my opinion of a short Russian paper on some aspect of lepton production and decay. I read the paper and told him it appeared plausible. I then promptly forgot about it. About two weeks later, as I was working at some calculation, a large, bearish man with a scowl on his face charged into my office and demanded, "Are you Jackson?" When I allowed that I was, he said, "How dare you! What business do you have interfering! Zichichi's proposal is nonsense." He then stormed out. That was my introduction to Carlo Rubbia. Zichichi and Rubbia had apparently presented competing proposals to a CERN program committee and Zichichi had quoted me as a supportive "authority." I don't know which proposal, if either, was approved, but Carlo was outraged at my "interference." Years later, when I told this story in his presence at a physics banquet, Rubbia's rejoinder was something like, "Jackson! I bought your book, but now I think I'll get rid it!"

The second incident illustrates how laboratory directors, with the best of intentions, may exacerbate an already difficult situation. Leon Van Hove, a very serious and proper Belgian, was head of the CERN Theory Division, and Viki Weisskopf was Director General (DG). CERN had at that time a policy, especially for theory papers, of requiring publication in European journals. I think the policy stemmed in part from an inferiority complex (not fully dispelled until Van der Meer and Rubbia shared the physics Nobel Prize in 1984) and in part from a desire to encourage European physics journals. Kurt Gottfried and I happily acquiesced, but some visitors did not. George Zweig, fresh from Cal Tech on an NAS-NRC

Fellowship and in the office next to mine, was one. Independently of Gell-Mann, Zweig had conceived of the frugal mnemonic (as it was then thought to be) of building up the particle states of Gell-Mann's "Eightfold Way" with three basic entities with fractional quantum numbers, called "aces" by Zweig and "quarks" by Gell-Mann. By January 1964, Zweig had a sizable paper written. He planned to send it to the *Physical Review* but ran up against the "European journals only" edict, transmitted to him ultimately by Van Hove. George was not shy about pushing his case. He argued, first with Van Hove and then, over his head, with Weisskopf, that he was an American who would be seeking a job in the United States and that he therefore needed to publish in US journals. Weisskopf was a DG with carefully chosen lieutenants and contacts that provided him with the means of very successful administration, but he was also a theoretical physicist with empathy toward young theorists. He failed to remain aloof; he overruled Van Hove. I was in my office just after lunch when Zweig, fresh from the DG's office, spotted Van Hove at the far end of the corridor and shouted for all to hear, "Viki told me I could do it!" Silence at the other end, but shortly after, a grim Van Hove stalked down the corridor in his overcoat, on his way home.

A month later, Zweig had a larger, revised version written. Ironically, he never published either paper. Gell-Mann's short paper on quarks appeared in a *European* journal, *Physics Letters* (16).

EARLY DAYS OF WINE AND CHEESE AT FERMILAB

In 1967, we moved to the University of California at Berkeley, where I began teaching on campus and doing my research in the Theoretical Physics Group at the Lawrence Radiation Laboratory. My appointment at Berkeley followed a summer as "house theorist" for the Alvarez Group, prompted no doubt by memory of my bolt out of the blue on muon-catalyzed fusion nine years earlier, as much as my more recent work with Gottfried. I continued work on peripheral interactions, influenced by the Berkeley emphasis on Regge poles, nuclear democracy, and then duality and exchange degeneracy, buzzwords of the late 1960s and early 1970s.

In 1972–1973, I served as Acting Head of Theoretical Physics at the National Accelerator Laboratory (NAL; later "Fermi" was added to the name and FNAL became known informally as Fermilab), then under construction in Batavia, Illinois. Sam Treiman had served the previous year, the first Acting Head of the infant group. I was in fulltime residence during the fall, but in the spring I taught my classes in Berkeley and came to NAL for one week in four. In the winter months, I made the transition between California shirtsleeves and Illinois woolies in a men's room at O'Hare airport. Much can be said about NAL and the theory group, but some of the ambience of those days can be conveyed by the story of the founding of the Wine & Cheese seminar. The account is adapted slightly from an article in the *Fermilab Annual Report 1992* (17).

Twenty-seven years ago, on September 29, 1972, the CK Mondavi Burgundy flowed for the first time, thanks to the initiative of the NAL Theory Group, in particular, Martin Einhorn. These were early days at the National Accelerator Laboratory. The 1972 Rochester Conference, held in Chicago and Batavia, had passed more or less successfully into history two weeks earlier. The buffalo roast and the bunting that camouflaged the raw concrete in the half-finished auditorium had done their work. The accelerator was running, sort of; results from the 30″ bubble chamber and the internal target at C0 had been reported at the conference. The high-rise and the meson and proton areas were nearing completion and particle beams were being coaxed away from the Main Ring. The Village (the houses of the expropriated town of Weston, Illinois) was still headquarters.

That year Marty Einhorn, along with Henry Abarbanel, Steve Ellis, David Gordon, Mannie Paschos, and Tony Sanda, formed the Theoretical Physics Section. This core was augmented by numerous visitors, some for brief stays and others for longer periods. These theorists led a simple but satisfying life, collaborating on the burning issues of hadronic and neutrino physics at 200–400 GeV. Visits and seminars by Bjorken, Feynman, and Low, among others, helped to provide the stimulating atmosphere of an established lab.

Director Robert R. Wilson and his troops in the field were straining to complete the experimental areas and to raise the energy and intensity of the machine. The early experiments struggled to be ready for whatever the machine would produce. Typically, work on the accelerator proceeded during the week; late on Friday, beam to the experiments was begun for the weekend. With luck, there would be some hours of running.

The contrast of the theorists "doing their thing" while the machine builders and experimenters heroically did the necessary spurred Einhorn to propose a weekly seminar to provide some sense of common purpose and intellectual food for the whole community. To avoid conflict with urgent meetings of one sort or another, 4 PM on Friday afternoon was chosen. Obviously there had to be a come-on to draw people back to the West Conference Room of the Director's Complex at the end of the week, especially since every week then was full of stress and setbacks, as well as small victories. Wine and cheese was the answer. The acting group leader and Marty cut a deal. Marty would do the shopping; the acting group leader would pay. All we needed was a name. We struck on "The experimental Theoretical Seminar," with a small E on experimental because it was just that. The West Conference Room was a modest-sized room that held 30–40 people, undoubtedly the whole ground floor of somebody's former residence. Veterans remember large wooden tables surrounded by government-surplus chairs, a portable screen for use with the overhead and slide projectors, and green chalkboards on the walls. I recall that the wine (in paper cups), Wisconsin cheddar, and bread lasted about 15 minutes at the beginning. Then the bar was closed and the talk began.

My 1972–1973 Pocket Diary for Physicists shows that Jim Sanford gave the first talk, on September 29, 1972, to about 40 people. My informal expense ledger for that date shows \$6.72 for bread and cheese (M Paschos; pd.) and \$9.43 for 2 gals. CK Mondavi Burgundy (MBE; pd.). A diary entry for October 12 reads,

> MBE owes me 93 cents (change on the wine) √

The item reflects my punctiliousness and Scottish blood; the tick mark demonstrates my successful tenacity!

In the first nine months there were 31 talks, over half on experiments, with theory and accelerator topics for the rest. (Paul Reardon gave “A Description of the Energy-Doubler Project” on February 2, 1973.) Clearly, we were off to a vigorous start. Einhorn recalls an occasion when Bob Wilson came in a bit late. The wine and cheese were gone and there was not an empty chair. Bob turned over a trash can and sat down—typical of Bob, and typical of the seminar, too. People did come. The room was normally packed to overflowing. One of my notations on the attendance had the addition, “2/3 ELG, 5/6 Jimmy W” (Ned Goldwasser and Jimmy Walker), indicating that even the bureaucrats came when they could. The wine gently loosened the tongues of otherwise inhibited questioners and even of speakers. Chris Quigg recalls Jimmy Walker coming in, helping himself to some wine, and departing, just as Henry Frisch was about to begin his talk. Henry remarked that Jimmy had been his senior thesis advisor at Harvard and he had met him there only once, for a similar period of 15 seconds. [As they say in the Congressional Record, (laughter ensued).] More seriously, Einhorn comments, “. . . it was an important civilizing physics event in the days before the high-rise. It also provided a focus for communication at a time when people were all spread out; I recall hearing Don Edwards talk about what was going on in the accelerator division.” A momentous pocket diary entry reads “December 14—2:45 AM, 400 GeV/c, 10^{11} ppp!” Nothing to do with wine and cheese, but indicative of the exciting times in late 1972.

While I recall vaguely the wine and the talks that year, my most vivid memory is of my encounter with Priscilla Duffield over the wine. Priscilla, a tall, imposing, no-nonsense woman, was Bob Wilson’s administrative “muscle.” I don’t know what her official title was but she was the majordomo, the enforcer, the person who ran the Director’s Office for Wilson and Goldwasser, protecting them from trouble and annoyance. If you had a problem about facilities or administration, you talked to Priscilla. One day, a month or so after the seminar’s debut, word about the Friday-afternoon goings-on had reached Priscilla. She stormed into my office, looking for my scalp. “What do you think you’re doing, serving wine at that seminar? Don’t you know it’s illegal to spend government money on such things?” I said that I wasn’t spending government money on the wine. She said, “Well, who *is* paying for it?” I said, “I am.” And she said, “Oh.” It was the one time I saw Priscilla just a little bit penitent.

“The experimental Theoretical Seminar” began as an experiment to fill a need. Right from the beginning it flourished. By June 1973 it was held regularly in the Village Curia, and its name was changed to “The Joint Experimental-Theoretical Seminar.” Not so long ago, its chief creator, Marty Einhorn, told me, “I also recall continually having to increase our allotment of cheese and wine to the point where the expense broke your budget and the Lab took the seminar over." The seminar continues. The wine, now banished, was paid for from some discretionary management account, not (*pace*, Priscilla) from DOE operating funds.

DAYS ONE AND TWO OF THE NOVEMBER REVOLUTION

I was in at the very beginning of the "November Revolution" of 1974. It was a specially exciting time. For about 24 hours, I knew something important about the J/ψ that nobody else in the world knew! I was at home on Sunday, November 10, 1974. My notes show that John Kadyk called at 4 PM to tell me the news of the discovery at SPEAR of a narrow resonance in e^+e^- annihilation into hadrons at $W = 3.105$ GeV, its observed width being consistent with the beam energy spread of about 1.5 MeV. He told me they were presently taking data to map out the peak. I immediately set to work to puzzle out what I could about this amazing resonance. At 8:30 PM Gerson Goldhaber called to give me some serious numbers: peak observed hadronic cross section, about 2000 nb; two-prong (not e^+e^-, probably mostly $\mu\mu$), about 100 nb. My notes for that evening and early on November 11 detail a very systematic attack on the problem, with inclusion of the single-photon amplitude in interference with the resonant amplitude, etc. I do not go into those details here. The big thing I found can be explained very simply. It involves ideas every nuclear physicist knows.

For a narrow spin-1 resonance of mass M formed by unpolarized beams of relativistic electrons and positrons of total cms energy W, the cross section for final state j is

$$\sigma_j(W) = \frac{3\pi}{M^2}\frac{\Gamma_e\Gamma_j}{(M-W)^2+(\Gamma/2)^2},$$

where Γ_e, Γ_j and Γ are the electronic width, the j^{th} width, and the total width of the resonance. The area under the resonance is

$$(\text{Area})_j = \frac{6\pi^2\Gamma_e\Gamma_j}{M^2\Gamma}.$$

Because of the energy spreads in the beams, the observed resonant line shape is the convolution of a true Breit-Wigner line and a resolution function,

$$\langle\sigma(W)\rangle = \int R(W-W')\sigma(W')\,dW'.$$

Here $R(W-W')$ is the beam resolution function, normalized to unity. It is easy to show that the area under an isolated peak is independent of the resolution function, provided it is narrow compared with the integration interval (which is assumed much larger than the resonant width Γ). This fact is the key to my simple analysis. Experimentally, the resolution-smeared cross section has an observed area.

$$(\text{Area})_j^{\text{exp}} = f\cdot\Delta W\langle\sigma_{j,\max}\rangle,$$

where ΔW is the full width of the observed peak at half-maximum (assumed to be independent of the final state j), $\langle\sigma_{j,\max}\rangle$ is the peak cross section, and $f = \pi/2$ for a Lorentz resolution function and $f = 1.0645$ for a gaussian. The theoretical and experimental areas can be equated to yield immediately results of the widths.

First of all, independent of resolution, the ratio of observed peak cross sections yields the ratio, Γ_h/Γ_μ. With the numbers I had that Sunday, $\Gamma_h/\Gamma_\mu \approx 20$. The large ratio meant that $\Gamma \approx \Gamma_h$. With this approximation and the assumption of μ-e universality, the integral over the observed hadronic cross section yields directly a value of Γ_e, according to the formula

$$\frac{6\pi^2\Gamma_e}{M^2} = X \cdot f \cdot \Delta W \langle\sigma_{h,\max}\rangle$$

and a value of the total width from

$$\Gamma \approx \Gamma_h = \frac{\langle\sigma_{h,\max}\rangle}{\langle\sigma_{\mu\mu,\max}\rangle} \cdot \Gamma_e.$$

Here I have inserted a factor X to account for the reduction in the observed cross section because of the radiative corrections. If we insert the preliminary values of $\langle\sigma_{h,\max}\rangle \approx 2000$ nb and $\Delta\mathrm{W} = 1.5$ MeV, we find $\Gamma_e \approx X \times f \times 1.3$ keV and $\Gamma \approx X \times f \times 26$ keV! With the radiative correction factor $X \approx 1.4$ (obtained at SLAC on November 11 from Roy Schwitters), these widths become

$$\Gamma_e \approx 2.0\text{–}2.8 \text{ keV}, \qquad \Gamma \approx 40\text{–}60 \text{ keV}.$$

The ranges reflect the uncertainty in the shape of the resolution function. My November 10 numbers were a factor of 0.7 smaller because I did not include the radiative correction. Despite all the uncertainties of my ignorance of the details of the experiment and the roughness of the numbers received over the telephone, before I went to bed that night I felt certain that the *total width of the* J/ψ *was less than 100 keV!* That seemed amazing on November 10, 1974 and still seems so today.

Wanting to share my excitement and newfound knowledge, I drove to SLAC on the morning of November 11 to talk to the experimenters. There I found an intense atmosphere. As has been told many times, Sam Ting had arrived with the news of the discovery by his group of a new particle at a mass of 3.1 GeV and a width of less than 5 MeV, seen in electron-positron pairs from 28-GeV proton bombardment of a beryllium target at BNL. Burton Richter and colleagues were putting the finishing touches on a brief *Physical Review Letter* to announce their independent discovery of the same resonance. I went to Richter to tell him what I had done, with its conclusion that the J/ψ was not just less than a few million electron volts in width but was less than 100 keV. I urged him to put this very significant information in their paper. I imagined that he would leap at the opportunity to publish something important about the resonance that Ting could not. It would take only two sentences! Richter said that their letter had already been cast in concrete and was being sent off, and furthermore his SLAC theorists told him that the photon propagator was more complicated than I had assumed. How could I argue against such experts? The SLAC theorists soon came around to my Breit-Wigner description of the resonance, but an important inference failed to make it into the initial publication.

The latest Particle Data Group values are $\Gamma_e = 5.3 \pm 0.4$ keV and $\Gamma = 87 \pm 5$ keV. My numbers are roughly a factor of two too small but are not bad for estimates based on very preliminary data on Day One of the November Revolution. Figure 11 shows my cartoon of a few months later, as it appeared on the cover of the *CERN Courier*, in celebration of the discovery of the J/ψ and 11 days later the ψ'. To refresh memories, I add a figure of the hadronic cross section from the paper announcing the discovery of the ψ (11).

STUDENTS

Earlier I mentioned some of my MSc and PhD students at McGill. When I came to Illinois in 1957, I "inherited" three students, two from Joseph Weneser and one from Francis Low. The best known is Icko Iben, Jr, who became a well-known astrophysicist at Illinois. Of my own students in Urbana, the most prominent are John T. Donohue of Centre d'Etude Nucléaires de Bordeaux-Gradignan; Gerald E. Hite, who spent many years in Germany and is now at Texas A & M—Galveston; and Gordon L. Kane of Michigan. In Berkeley, as thesis supervisor, I have had a part in the graduate education of Robert N. Cahn, who became a close colleague here at LBNL; Richard D. Field, of Feynman and Field, now at the University of Florida; and Chris Quigg, long-time theorist at Fermilab (and Editor of this series), among others.

I feel privileged to have had them all as my physics "children." Not surprisingly, my own inclination to stick close to experiment and our four-dimensional world has rubbed off on all of them, although Gordy Kane's belief that supersymmetry is hiding behind every tree lifts him somewhat off the real axis from time to time. Of course, events may prove him right and the skeptics wrong.

AFTERWORD

These glimpses into my education and the first 25 years of my professional life are but a sampling. There are many more stories to tell, some serious, some less so—of battles for free speech locally and human rights worldwide, of how I became an honorary woman, of decisions on the future of US high-energy physics, of leafleting CERN mailboxes with Jack Steinberger and Georges Charpak when US missiles were coming to Europe, of life in the SSC Central Design Group, and many more. Physics is a serious but joyful business. Although it has become a profession, it still has elements of a calling. I hope these vignettes have conveyed some of the excitement and joy, the sense of community and experiences shared. It has been and continues to be challenging and satisfying—in short, fun.

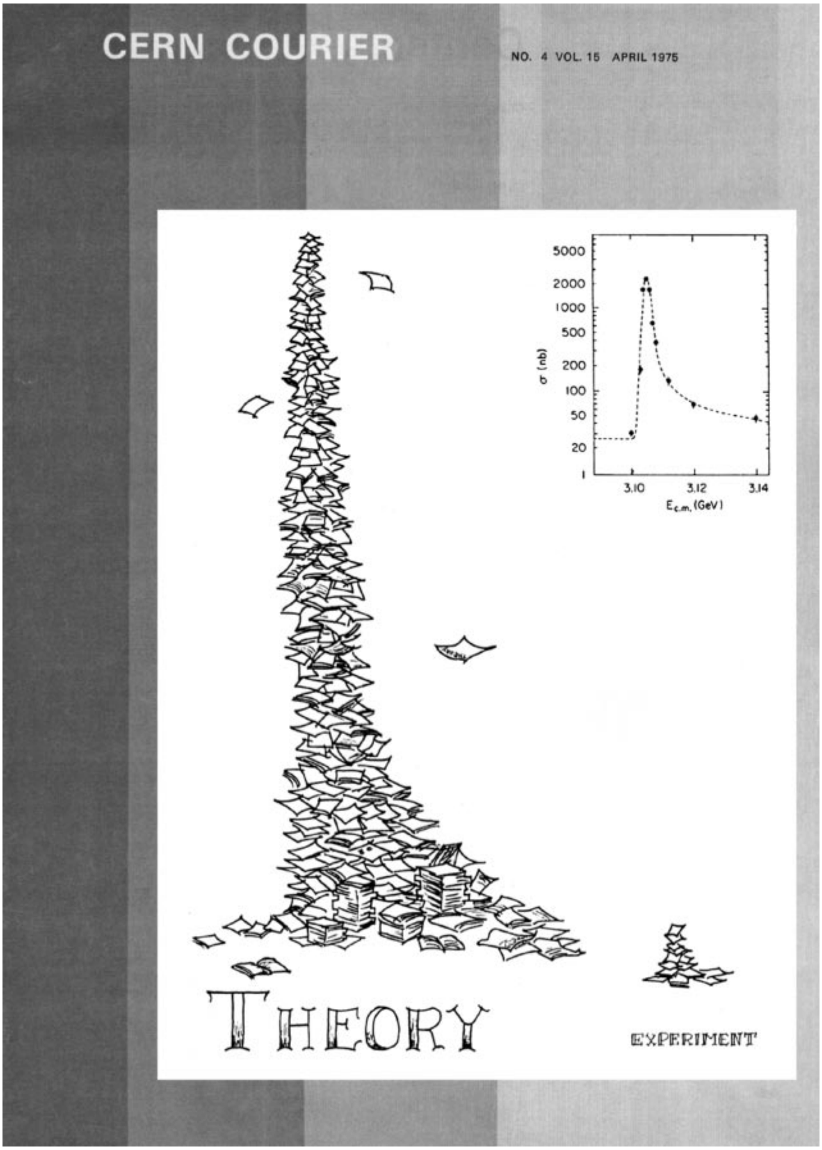

Figure 11 The author's cartoon commenting on the excitement generated by the discovery of the J/ψ and ψ', as it appeared on the cover of the *CERN Courier*, April, 1975. *Inset* (not on original), a figure from discovery paper of the ψ(3100) showing the hadronic cross section (11).

ACKNOWLEDGEMENTS

I thank Photo Services, Lawrence Berkeley National Laboratory, for expertly digitizing the photographs from my diverse originals. This work has been supported in part by the Director, Office of Energy Research, Office of Basic Energy Sciences, of the US Department of Energy under Contract DE-AC03-76SF00098.

LITERATURE CITED

1. Blatt JM, Jackson JD. *Phys. Rev.* 76:18 (1949)
2. Jackson JD, Blatt JM. *Rev. Mod. Phys.* 22:77 (1950)
3. Jackson JD. *Phys. Rev.* 106:330 (1957)
4. Jackson JD. In *Discovering Alvarez*, ed. WP Trower. Chicago: Univ. Chicago Press. (1987), p. 154
5. Jackson JD, Treiman SB, Wyld WW. *Phys. Rev.* 106:517 (1957)
6. Jackson JD. Treiman SB, Wyld WW. *Nucl. Phys.* 4:206 (1957)
7. Jackson JD. *Physics of Elementary Particles*. Princeton, NJ: Princeton Univ. Press. 135 pp. (1958)
8. Jackson JD. *Classical Electrodynamics*, 1st ed. New York: Wiley. 641 pp. (1962); 2nd ed. 848 pp. (1975); 3rd ed. 808 pp. (1998)
9. Screaton GR, ed. *Dispersion Relations. Scottish Universities Summer School 1960*. Edinburgh: Oliver & Boyd. 290 pp. (1961)
10. Jacob M, Wick GC. *Ann. Phys. (NY)* 7:404 (1959)
11. Trueman TL, Wick GC. *Ann. Phys. (NY)* 26:322 (1964)
12. Gottfried K, Jackson JD. *Phys. Lett.* 8:144 (1964)
13. Gottfried K, Jackson JD. *Nuovo Cimento* 33:309 (1964)
14. Gottfried K, Jackson JD. *Nuovo Cimento* 34:735 (1964)
15. Salam A, Ward JC. *Phys. Lett.* 13:168 (1964)
16. Gell-Mann M. *Phys. Lett.* 8:214 (1964); Zweig G. *CERN-8182-TH-401*, 17 January 1964; *CERN-8419-TH-412*, 21 February 1964
17. Jackson JD. In *Fermilab Annual Report 1992*. Batavia, IL: Fermilab. pp. 40–41 (1993)
18. Augustin JE, et al. *Phys. Rev. Lett.* 33:1406 (1974)