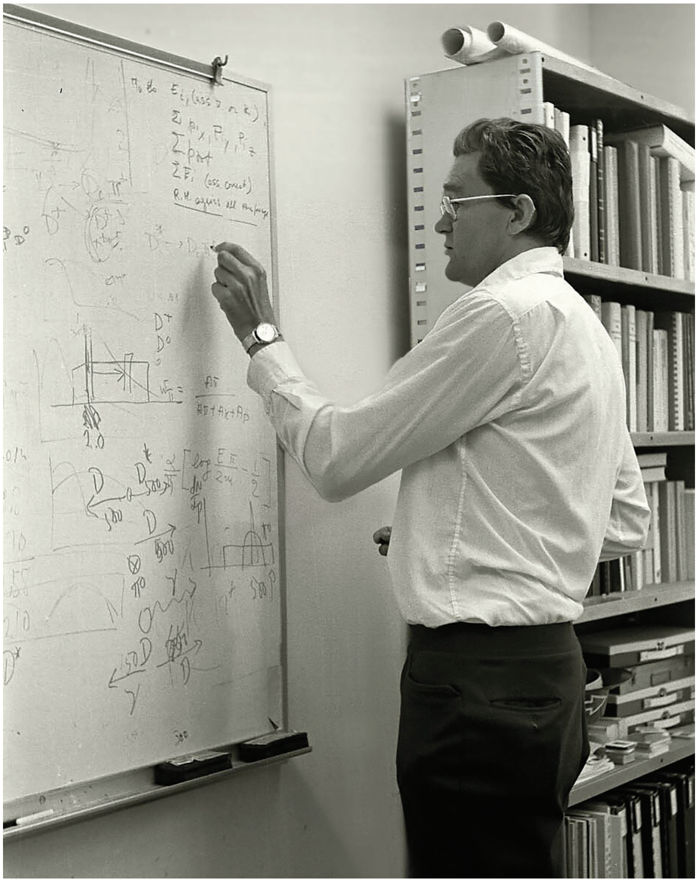

James D. Bjorken

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"Why Do We Do Physics? Because Physics Is Fun!"

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Abstract

In this informal memoir, the author describes his passage through a golden age of elementary particle physics. It includes not only his career trajectory as a theoretical physicist but also his excursions into experimental physics and particle accelerator theory. While his successes are highlighted, some unsuccessful efforts are included in the narrative as well. Those "losers" were arguably as pleasurable as the less-frequent "winners." Since retirement, the author has become interested in gravitation theory and cosmology—a new golden age. This activity is also briefly described.

Contents

1. PROLOGUE

I have chosen to begin this memoir with excerpts from a talk I gave just a few years ago. I had received the Wolf Prize (more about prizes later!), and friends of mine in Jackson, Wyoming, organized a celebration. I had to give a speech, and what follows is excerpted from that talk (1). The reason for including it here is that it epitomizes an idealized view of how science at its best works. And, in the remainder of this article, I would like to put my own personal history within that context:

> This part of the talk has to do with belief systems in science. I recall that in the 1960s people would ask me, "Do you believe in quarks?" These people were physicists! As best as I can recall, my reply was, "That is a religious question, not a scientific one. What do you mean?"
>
> For me, a relatively young researcher, and schooled year after year in the principles of the scientific method, such a question felt a little bit silly. I could—and did—"believe" in quarks and "disbelieve" in them at the same time. Or I could lay odds. No problem! That was what I was taught as science itself.
>
> But now, from a perspective a full half century removed from those days, I see belief as playing an important role in how science is conducted—for better or for worse. At the individual level, a belief that a certain line of thinking, or that a certain program of experiments will be highly productive, is a motivating force. If you believe, you will think harder, you will work harder, and you will not give up until hard evidence forces you to give up. Given a mix of talented believers who cover the full range of possibilities, both theoretical and experimental, it would seem that the science would move forward at optimal speed. This is contingent on the requirement that, at the social level, the scientific method is the lingua franca. And in this idealized scenario, most of this diverse set of believers will by definition be losers. The winners must not gloat. They must simply regard themselves as fortunate, and maintain full respect for all of those losers.
>
> I went back and had a look at my own track record. I identified a dozen or so memorable topics in my physics career. Each one of them gave me a great deal of personal satisfaction. Roughly four I regard as "winners," with twice that many as definite "losers," and with a small number still undetermined. Personally, I feel very happy in having even one of those dozen turning out to be a "winner."

> In practice, the idealized scenario I have sketched doesn't work that way. We are naturally competitive. We are territorial, especially with respect to ownership of intellectual property. And individual belief systems tend to morph into collective belief systems. And I need not elaborate at all—especially in these days—on the dangers that ensue. . . . (1)

For better or worse, I would like the above words to provide the setting for this memoir. In what follows, I will not spare you, dear reader, some accounts of the failures as well as those of the successes.

2. THE EARLY YEARS

The name Bjorken is, not so surprisingly, Swedish. In the old country, the name was decorated with two dots over the o and an accent over the e. These embellishments were dropped into the Atlantic Ocean by my father en route to the United States in the mid-1920s. He grew up on a small family farm on the shores of Lake Siljan, a region rich in folklore located in the center of the country. He showed enough talent as a child that his parents sent him off to college, where he obtained a degree in electrical engineering. En route to the United States, he met my uncle, also an electrical engineer. They eventually found themselves in Chicago, where they met their wives in the Swedish district, thanks to my uncle's family connections.

My father's career was as a superintendent in a shop that repaired and rebuilt industrial electrical motors and generators. He was a superb troubleshooter and overall handyman, who would find creative ways of fixing things that broke. As a child, I clumsily tried to follow his lead, and I enjoyed building models, especially model railroad gear. In school, I naturally gravitated toward the sciences, with my favorite high school subjects being mathematics and chemistry. Physics was less attractive. But my career preferences at that time were very vague and unfocused. My interests outside of science included classical music (French horn) and a love of baseball. Because I was hopelessly poor as a player, this evolved into being a frequent spectator, watching the Chicago Cubs at Wrigley Field.

I graduated from Main East High School, Park Ridge, Illinois, in 1952. Where to go to college soon boiled down to the University of Chicago versus MIT. My parents argued that the U of C was too close to home and that I needed to be farther away from the nest. In hindsight that was good advice, even though it created considerably more financial hardship. At MIT I was offered a long-term loan, while at U of C I was offered a rather generous scholarship.

3. UNDERGRADUATE PHYSICS AT MIT

For me, entry into MIT was somewhat daunting. There were all those valedictorians from top schools like the Bronx High School of Science, already versed in calculus and relativity theory. Thanks to the heritage of World War II and the Bomb, physics was clearly the sexiest subject. But our common nerdhood created something of a bond, and friendships were easily formed.

By the end of my freshman year, it was clear that physics would be my major. Part of this was due to the aforementioned social pressure. But the main reason was that Physics 8.01 was taught by a master, Hans Mueller. He was from Zurich, and his research specialty was classical optics. In the lecture room, he made classical Newtonian mechanics a work of scientific and pedagogical art. A beautiful, compelling logical development of the mathematical description was linked to the real-life world of observations and measurements. His visage was a bit Einsteinian, and his delivery was in a thick Swiss German accent.

As evidenced by the title of this memoir, I still remember vividly the opening of his first lecture: "Why do we do physics? Because physics is fun!" At the time it was a bit of a groaner. But as the years wore on, it gained more and more significance for me. He then took aim at the novice relativists from Bronx High: "Vot is space? Vot is time? Space is vot ve measure mit a ruler. Time is vot ve measure mit a clock." He then launched immediately into Newton's Laws. To this day, I have not encountered a better definition of the nature of space and time.

The quality of the undergraduate education was especially high. There was a high level of personal attention. Before long, I was introduced to my mentor and second father, Sidney Drell, via courses he taught. And not too long thereafter, he took notice of me. It is a story he loved to tell. There I was in the back of his class not paying attention to his lectures. So one day he wandered back there while lecturing to see what was going on. I was doing homework for other classes. Nevertheless, I did just fine on his quizzes and exams.

What was happening? As best as I can recall, the reason probably had to do with my discovery of the mountains and rivers of New England. The MIT Outing Club ran weekend trips up to the White Mountains and other such venues. And I became a regular. That meant I had to get homework out of the way during the weekdays. Sid's class was a good opportunity because he kept quite close to the textbook in his presentations, allowing me to do homework without missing too much fresh information.

Sid and his close colleague Fred Zachariasen actually ran an evening seminar for undergraduate physics majors that took place in Sid's living room. This initiated an especially close and warm relationship with the remarkable Drell family, a relationship lasting more than 60 years.

In my senior year I made contact for the first time with the research frontier. MIT had on its campus a 300-MeV electron synchrotron, and I became connected to it via Al Wattenberg and Bernie Feld. A hot topic at that time had to do with puzzling decay properties of the newly discovered K mesons. Eventually, Lee and Yang (2) provided the answer: parity violation. I was in the perfect position to discover this myself—an "innocent fool" who knew nothing about the symmetry called parity. But I blew the opportunity. Instead, I did calculations for Bernie on the decay distributions, which supplemented experimental work by Dave Ritson going on in the lab. Dave had acquired nuclear emulsion data from Brookhaven on stopping Ks. I was invited to look through the microscope at these precious events. After a few minutes of eyestrain misery, I vowed to remain in theoretical physics. If that was experimental physics, I wanted nothing to do with it!

4. THE EXODUS TO STANFORD

As my graduation from MIT in 1956 approached, I needed to decide on a grad school. Two candidates stood out: Stanford and Harvard. Harvard offered the most generous scholarship aid, but again the choice ended up being in the opposite direction. Two factors loomed large. One had to do with mountains: The Sierra Nevada beckoned. The other was a conversation with Fred Zachariasen after one of Sid's evening seminars, on the way back to campus. When he heard of the Harvard option, he reacted in mock horror: I should not become a Schwingerian! By then, Julian Schwinger was already legendary. His public persona was formal: lectures difficult to understand and papers difficult to read. Fred did not mention his private persona as a superb mentor of a huge number of students who went on to have very distinguished careers. I often wonder what would have happened had I sat at his feet. I think the outcome would have been not bad at all, but quite different nevertheless.

It was not only me that chose to leave MIT in 1956 and move to Stanford. It was in fact a mass emigration, including Fred Zachariasen himself, Sid Drell, Burt Richter, Henry Kendall, and nuclear physicist Charlie Schwartz, among others. A few years later, Dave Ritson joined the crowd. Along with these emigrants, my nickname bj also emigrated. It had originated in the dormitory in my freshman year. The only telephone on the floor was located in the hallway. When someone answering the phone called out, "Hey, Jim, it's for you," I was not alone in running for the phone.

My next-door neighbor, Jim Hughes, would be racing with me. We quickly tired of this routine. My neighbor suggested using initials—he was already known as J.S. I did not like the sound of J.B.—too much like a banker—so I permuted it to bj. The moniker spread from the physics majors in the dorm to the physics faculty, via Sid and Fred. And once it got exported to Stanford, it became permanent.

For many of the emigrants, the attraction of Stanford had to do with its recently commissioned 500-MeV linear electron accelerator. Bob Hofstadter was already using it to measure the size and shape of atomic nuclei. And it was clear that there were many experimental opportunities beyond his program. For me, that was less of a factor. Having real mountains nearby was at least as important.

Burt Richter, who did his thesis at the MIT synchrotron, came to Stanford eager to use the Stanford linac to test quantum electrodynamics (QED) at short distances. While Tomonaga, Feynman, and Schwinger had created the very successful modern formalism, there was still a strong feeling that there were fundamental shortcomings (the divergent values of the basic input parameters in the theory) that were only wallpapered over. Viki Weisskopf, a QED veteran as well as a beloved guru at MIT, shared these feelings as well, and I suspect that much of his concern rubbed off onto Sid and Burt.

Once at Stanford, Sid suggested a program of experiments that would be sensitive to a breakdown of QED occurring at short distances (3). One of these was taken up by Burt: photoproduction of wide-angle electron–positron pairs. And it wasn't long before I was drawn into the program. The necessary theoretical calculations supporting Burt's experimental program had not been done. There were several Feynman diagrams that had to be calculated fully relativistically, with inclusion of proton structure and relativistic recoil of the proton. This occurred long before such calculations became computerized. They ended up being done in triplicate by Sid, me, and my fellow student Steve Frautschi (4). Half of Steve's PhD thesis and half of mine consisted of these tedious, miserable calculations.

The other half of my thesis had to do with the fashion of the day: dispersion relations. In those days, the aforementioned QED skepticism was generalized by a majority of the theorists into skepticism regarding whether local quantum field theory was the basis for a theory of everything. The candidate alternative was to concentrate on the direct observables, in particular the S-matrix, and set aside the field theory Lagrangians. Locality properties required by causality and relativistic invariance could be achieved by much weaker assumptions about the analyticity properties of the Green's functions. This latter question was a specialty of the very best theorists on the planet. But the more mundane question of how individual Feynman diagrams behaved had only begun to be addressed, most notably by Yoichiro Nambu (5). I got interested in the problem, which turned out to be the other half of my thesis. But it never got published in a journal because I got scooped; the great Russian physicist Lev Landau got there first (6) (my version of the solution can be found in Reference 7). I did not at all feel bad about this. I got scooped by Landau! Not bad!

Those years at Stanford were special. The department was small, and there was plenty of interaction between us students and the faculty, including distinguished visitors such as T.D. Lee.

5. WANDERJAHRE

After receiving my PhD in 1959, I stayed on at Stanford for a couple of years as a postdoc. I was very happy to be able to stay there, and the department evidently was happy as well. But this was not a typical career path. Everyone felt that travel at this stage of a career was very beneficial and perhaps essential to grow into the complete physicist. So, with the help of fellowships, I hit the road for 3 years in a row without breaking my fundamental ties with Stanford. The first year was on the East Coast, with the goal of "sitting at the feet of the Great Masters." The Great Masters that I targeted were Lee and Yang at Princeton and Gell-Mann, Goldberger, and Low at MIT. My plan was foiled. Lee and Yang "divorced," with Lee moving to Columbia University and Yang becoming incommunicado. And at MIT, the other Great Masters kept to themselves in a cigar smoke–filled room and were rather inaccessible to outsiders like me. But it wasn't all that bad a year. My physics focus was Regge theory, but my own contributions to the subject were at best modest and at worst totally forgettable.

The next year was spent at CERN. The dollar was strong compared with the European currencies, so life was very good. There were the Alps to explore and enjoy. And there was a very good ambience at CERN as well. As for the physics, spontaneous symmetry breaking was a hot topic. I entertained the very speculative idea that maybe the photon is itself a Nambu–Goldstone boson (8). Although this remains a very speculative idea to this day, the possibility still exists. The citations to my paper are spread rather uniformly across the half century following its publication. And I still rather like the idea.

The third year was at the Niels Bohr Institute in Copenhagen. The ambience again was wonderful. Actually, I had been given a preview the previous spring. Thanks to the connections between Niels Bohr and Russian physicists, a tour to the Soviet Union took place during the spring break. I learned about it from a colleague and jumped aboard. It was an extremely memorable event—one that opened my eyes to the very high quality of physics being done there despite the severe Cold War constraints under which our Russian colleagues worked.

It was in the following springtime, while I was officially at the Bohr Institute, that Shelly Glashow showed up. I had been working on clumsy constituent models of hadrons, a subject of interest to Shelly as well. We started working together. One day he showed up in a state of high excitement, with not only a new model but a half-completed paper describing the idea. I was ordered to sign on. And I did (9). His suggestion of the fourth, charmed quark (including the name) of course turned out to be the right answer. To this day I am not very clear on what if anything I contributed to the story; Shelly was for sure generous in sharing that idea with yours truly.

6. THE BOOKS

In the early 1960s Sid was encouraged by department chair Leonard Schiff to write a textbook on QED and quantum field theory. Sid asked me to be a coauthor. I was cautioned by colleagues that it was not appropriate for me to get involved in such a project—I should instead be concentrating on research. But I signed on anyway. One motivation was the feeling that I needed to master the subject, and this was a very good way to do it. The other motivation was the pleasure of working closely together with Sid. The project began shortly before my Wanderjahre. Neither Sid nor I anticipated the magnitude of the undertaking, one that resulted in two books (10, 11), not one. Our working method was to collectively write each chapter. A first rough draft by me would then be passed on to Sid for revision, which would usually be major. The reverse was also true. In the resultant back-and-forth we converged on a common jargon and writing style. To this day I cannot look at a typical piece of these books and claim that I wrote it or that Sid wrote it. Together we wrote it all.

Writing the books took years. Most of that was my fault. I am a very good procrastinator. This, plus my absence from Stanford (except for summers) during the Wanderjahre, slowed things down. Finally Sid laid down the law and imprisoned me in the tower of the Drell House, only to be released on the completion of The Books. The strategy worked. And for me it is a happy memory to have been in such close proximity to the Drell family.

7. PROJECT M

In the fall of 1964 I returned to Stanford. By then, the 2-mile linear accelerator complex now known as SLAC had been proposed. Both Sid Drell and I faced the issue of whether to stay on with the Physics Department or to move over and join Pief Panofsky in the new laboratory. Both he and I independently opted for SLAC, at almost the same time. The SLAC theory group at that point was minuscule. It was led by Pierre Noyes, a well-known S-matrix theorist who had emigrated from Berkeley. The temporary home of Project M (M for Monster) was a warehouse on the Stanford campus. The front half of the warehouse consisted of cubicles for everyone from the director down to us lowly peons. The rear half of the warehouse housed a machine shop, which manufactured prototypes of the components of the linac.

For us theorists, a major task was hiring. Sam Berman came in early on. One of his many skills was as a talent scout. Two candidates stood out. One was John Bell, and the other was Tini Veltman. During their stay, John produced his celebrated "Bell's Theorem" (12), and Tini produced his pioneering computer program Schoonschip. All this happened amid the din that permeated the Project M workplace. I like to tell this little story to those theorists who think that good theory can only be created in a totally silent, monastic setting. I myself prefer some background noise within my workplace environment.

Alas, we failed in hiring John and Tini, who returned to Europe. But the group did grow and of course evolved very positively down through the years. Sid had a lot to do with this. His informal style and high standards were instrumental in creating the group's special personality. Sid had a way of asking "dumb" questions to seminar speakers. Actually, most of his dumb questions concealed some subtleties that only emerged after a round of discussion. But the questions were sufficiently dumb that they encouraged students to engage the speakers with their own questions. So the resulting ambience was both inclusive and informal. It was hard to feel left out or intimidated.

One of the challenges facing SLAC was its experimental program. The short microsecond beam pulses emerging from the accelerator every few milliseconds posed a major challenge to experimenters used to working at synchrotrons in Berkeley, Brookhaven, or elsewhere. So we theorists had some obligation to the lab in that regard. Pierre specialized in neutrino beams, and Sid invented the "Drell process," which opened up research opportunities using photon beams (13). It was during this period that I became increasingly interested in using electron beams not only for elastic scattering experiments à la Hofstadter but also for inelastic scattering. That of course turned out to be a very productive direction.

8. PARTONS

Before long, construction of the SLAC linac and the laboratories had proceeded enough that we made the move to the SLAC site. My interests focused more and more on the electron scattering program. Gell-Mann's current algebra ideas stimulated the creation of sum rules somewhat akin to those created in the golden age of atomic physics. Steve Adler in particular created one for neutrino interactions (14), and that influenced me to look at the implications for electron–nucleon interactions. In the years before the experimental program, I came up with a corollary of Steve's sum rule (15). It implied rather strongly that the yield from inelastic electron scattering at large momentum transfer could be large, characteristic of scattering from pointlike constituents. But there were loopholes in the argument.

Additional motivations for thinking about this general problem came from elsewhere. A former Stanford colleague and friend, George Williams, took a position at the University of Utah. Through him, I was introduced to the cosmic ray group there led by Jack Keuffel. Jack was commissioning a huge neutrino experiment deep underground, beneath the Park City ski area. I was soon paying frequent visits to Salt Lake City, not only to learn the physics but also to learn how to ski in deep powder. Jack and his colleagues were experts in both. On fresh powder days, we would head off to Alta. On the chairlift, I would get a tutorial from Jack on cosmic ray physics. On the way down, the subject matter would of course change somewhat.

It turned out that Jack's experiment could be sensitive to the deep-inelastic physics all the way up to the 1-TeV scale, a huge number for those days. This would be the case if the total neutrino–nucleon cross section rose linearly with laboratory neutrino energy. The rough prediction was equal numbers of events per factor of 10 in incident neutrino energy (16). And the energy resolution of Jack's detector extended up to hundreds of GeV. But, even given Adler's sum rule, this behavior was not guaranteed. One needed additional assumptions to make it happen. The simplest way to make it work turned out to be the hypothesis that to this day bears my name: Bjorken scaling (17, 18). I was acutely aware that such an assumption was sufficient, but far from necessary.

Meanwhile, back in California, there were similar influences. Thanks to my introduction to the mountains gained at MIT, I became active in the Stanford Alpine Club. Rock climbing in Yosemite Valley, as well as mountaineering in the High Sierra, was a strong draw (see **Figure 1**). That was also the case for experimentalists Henry Kendall, Hobey DeStaebler, and Dave Coward. All three were members of the main SLAC electron scattering team (Group A). On the 5-hour drive to the Sierra, there was ample time to talk shop. My obsession with the "deep-inelastic" regime (Henry Kendall's name for it) was of course present in such discussions. The main complication with pursuing that program had to do with the electromagnetic radiative corrections, which were anticipated to be very large. This was a frequent topic.

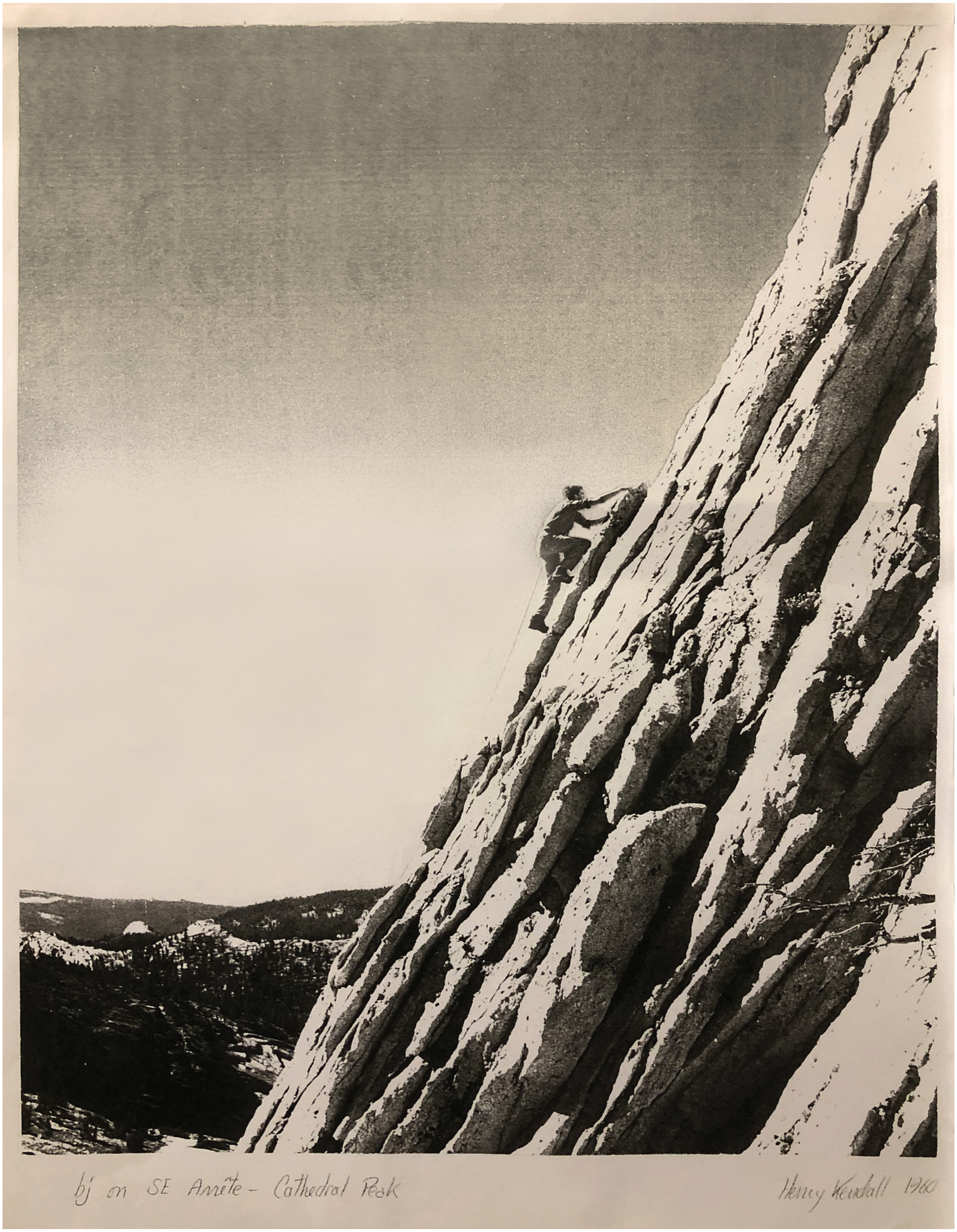
The net result of these encounters was that I became a Group A groupie and was more than ready to respond to the data when they arrived. This story is often told, so I will be brief here. Henry Kendall visited my office and showed me the first round of data, with radiative corrections applied. I asked him to replot the data in terms of the scaling variable. He came back the next day in a state of great excitement: All the data lay on a single curve. Shortly thereafter, Richard Feynman, who was in the neighborhood visiting his sister, dropped by SLAC to learn what was new. He was shown the scaling data. Overnight, he interpreted the data in terms of his parton model. Up to that point he had concentrated on the application of his ideas to hadron–hadron scattering (e.g., to the trendy Regge pole theory). But here was a much simpler and cleaner application, and it took him no time at all to get up to speed.

When Feynman arrived at SLAC, I was elsewhere (the mountains, maybe—but I really don't remember). But I returned before he left, and we had a one-on-one session where we compared notes. I had used arguments for scaling that were new to him, while his version of pointlike constituents was different from my own. He used infinite-momentum techniques that I should have been using. Instead, I retreated to the rest-frame description, which was clumsier.

The bottom line was that, with the involvement of Feynman, the scaling idea took hold almost immediately. At SLAC, I teamed up with Manny Paschos (19). He was more wired into the goings-on at Caltech than I was. But even so, both he and I awaited some Feynman opus emerging from there.

Nothing appeared, and after some time I got on the phone to Feynman to find out why not. He told me that nothing was in the pipeline. When I objected, he simply said, "I'm rich."

For me, the next major development belonged to my students John Kogut and Dave Soper. They created, totally on their own, the light-cone quantization formalism (20, 21). Thereafter, John and I, together with Sam Berman, applied the parton ideas to hadron–hadron collisions (22). Those were truly heady days. But for some reason we missed the description of high-mass dilepton



(*Caption appears on following page*)

Figure 1 (*Figure appears on preceding page*)

"Bjorken scaling" up the southeast arête of Cathedral Peak, Yosemite National Park (1960). Photo by Henry Kendall.

production via quark–antiquark annihilation. That was quickly supplied, in-house, by Sid Drell and Tung-Mow Yan (23). As best as I can recall, I felt at that time that the theoretical description of that process had some subtleties and that the "Naïve Drell–Yan Formula" was indeed overly naïve. Wrong!

9. RUSSIA

Being an elementary particle physicist provides many opportunities for travel. I have had the privilege of visiting several dozen countries during my career. Many of the most memorable such visits occurred in the former Soviet Union during the 1970s. Some of the highlights deserve mention in this memoir. It was Stanford colleague Marshall Baker who alerted me to the quality of the science there; he was one of the first Western physicists to experience science on the other side of the Iron Curtain. Before long I was attending conferences in Moscow, which opened the door to visits to ITEP and the Lebedev Institute in Moscow and to Gatchina (Ioffe Institute) in Leningrad. The leadership in those institutes included Lev Okun and Boris Ioffe at ITEP, Andrei Sakharov in Lebedev, and Volodya Gribov at Gatchina. Ioffe was especially expert in deep-inelastic scattering, Okun was expert in almost everything, and Sakharov was of course Sakharov. Gribov was a driving force in understanding the dynamics of the strong interaction. In those days the Soviet theory community suffered from the isolation from the West. High-energy physics of course was evolving rapidly, and the information flow from the West trickled in too slowly for them to respond. All too often they would get scooped. So a visitor like me was warmly welcomed.

In the mid-1970s I was invited to ITEP for an extended stay. My wife and children accompanied me, and we spent almost a month living in the ITEP apartment complex. The final week was in Leningrad, where we were housed in an elegant Academy of Sciences apartment located within the palace that houses the great Hermitage Museum of art. Memorable indeed!

10. THE NOVEMBER REVOLUTION AND THE RISE OF THE STANDARD MODEL

One fateful day in 1974, I was at home at the dinner table when the phone rang. It was Burt Richter, telling me that SPEAR had discovered a narrow resonance in electron–positron annihilation at 3.1 GeV in the center of mass (24). I went back to the table in a daze. The meal happened to be roast beef with a very sharp horseradish sauce. After I had devoured the horseradish with no ill effects, my wife, Joanie, understood; she quietly suggested, "I think you had better go to the lab now."

The subsequent tumult was memorable. Marathon sessions, attended by theorists and experimentalists, went on every day, with minutes taken by one of the grad students (25). There was full sharing of the intellectual property. We all just wanted to understand what was going on. I am told that the same thing happened elsewhere, in particular at Cornell, Princeton, and Caltech.

Charm was almost immediately the leading candidate. But I did not sign on. There was not enough strangeness in the hadron final states. And the total cross section was too large. The excuse given by the believers was that, at essentially the same threshold energy, the tau lepton was being produced. This in turn diluted strangeness yield and raised the total cross section. At that time, there was independent evidence for the tau production, but it was far from conclusive. Anyway, the necessary fine-tuning troubled me. In addition, it was well known at SLAC that Marty Perl really wanted to discover a heavy lepton and devoted great energy to the search. So there was that suspicion of bias as well. To his credit, Marty was a very careful experimentalist, and he had a superb, no-nonsense lieutenant (Gary Feldman) backing him up (26).

My alternative was an idea of Harry Lipkin's called frozen color (27). In principle, the quarks were given integer charges, in concert with an idea created earlier by Han and Nambu (28). But in practice, only the fractional charge was to be observable in the absence of a low-energy spectrum of hadrons possessing net color. This idea actually lingered on in the literature for some time before finally fading away. And I stayed with it until convincing evidence for open charm finally appeared. Wrong!

Both in standard quantum chromodynamics (QCD) and in the maverick Lipkin version, the issue of quark and gluon confinement loomed large. I never involved myself in the basic problem of *why* they are confined. But I became interested in *how* they become confined during the evolution of a high-energy quark–antiquark pair-creation event. This clearly involved looking at the space-time geometry of such collisions, which in turn involved dealing with large spatial and temporal intervals. The solution was not immediately self-evident. I was not alone in worrying about this question. John Kogut and Lenny Susskind were doing the same thing. We all came up with the right answer at about the same time: the so-called inside–outside cascade (29, 30). This description was refined and extended by the Lund group, which was led by Gösta Gustafson and Bo Andersson (31). It is a piece of physics that gave me a special amount of pleasure. Perhaps that is because almost no equations were needed to figure it out.

The presence of the fourth quark stimulated development of the electroweak theory. When Steve Weinberg wrote his pioneering paper (32), he titled it "A Model of Leptons" because the three known quarks did not provide a description that was as user-friendly as the leptonic description. The fourth quark allowed the Glashow–Iliopoulos–Maiani mechanism (33) to take place. This in turn has made the theory on the quark side even more user-friendly than for the leptons.

Together with this development, the relevance of non-Abelian gauge theories emerged, not only for the weak interaction but also for the strong. The emergence of QCD provided stimulus for the $SU(2) \times U(1)$ electroweak gauge theory as well. On both fronts, I dragged my feet. I followed the lead of J.J. Sakurai and opted to treat the weak-interaction description in terms of what now is called effective field theory (34). With this more general version, the estimate of the gauge boson masses was softer. I adhered to this version for quite a long time. Wrong!

All through the 1970s, I was enamored of the concept of rapidity and what is known in the trade as the lego plot. It was Feynman in the early days of the parton model who popularized the concept (35). I think I learned about it from Jack Keuffel, because it was in common use in the cosmic ray community for a long time. The properties of the "central region" were connected to diffraction scattering and the energy dependence of the total cross section. After QCD became accepted as the correct strong interaction theory, multijet final states invaded the central plateau region. I responded by creating a language to describe this. The simple lego plot has the topology of a cylinder. Suppose a hadron–hadron collision leads to a dijet in the final state. The hadronic content of each jet can also be described in lego terms by a cylinder. Each such cylinder gets attached to the original cylinder. The topology becomes fractal, similar to that of a plumber's tree. When I submitted this idea to *Physical Review D*, the word "plumber" appeared in the title of the preprint (36). Unfortunately, it was excised by the editors. I especially like the language described in that paper. But, alas, it never has gained any traction with the army of theorists who create QCD simulations of hadronic final states in high-energy hadron–hadron collisions.

11. THE MOVE TO FERMILAB

In 1979, Leon Lederman took over the directorship of Fermilab. He soon invited me to join the lab. Along with the invitation came a remarkable offer: My family could live in the Director's House (Site 29), located in the northwest corner of the laboratory itself, because Leon preferred another residence, which was also on-site. I was into my midlife years, and this change of venue was not at all unattractive for me personally. The biggest difficulty was clearly to persuade my wife, Joanie, to leave California for Illinois. She was a fifth-generation Californian with deep family roots in the San Francisco area. We scheduled a visit to the lab in January, just to make the test as severe as possible. It so happened that our visit coincided with the Great Blizzard of 1979, with all the mundane features of the Batavia area buried in 3 feet of snow. Joanie was entranced by the beauty of it all, and we opted to make the move. It lasted for a very memorable and bittersweet decade, after which I returned to Stanford and SLAC.

My title was associate director for physics, and the job description mainly consisted of working with the program office as ombudsman between laboratory staff and experimentalists proposing—or on occasion setting up and running—experiments. I became especially involved with one of the small groups working in the neutrino area, led by Luke Mo, Al Abashian, and Tom Nunamaker. They were looking for a successor to their neutrino–electron scattering experiment (37). Axions were a new hot topic, and we explored the possibility of a beam dump experiment at Fermilab that might produce low-mass axions, which then could decay in flight into electrons and/or photons. Luke's big electromagnetic calorimeter would then serve as a very good detector given such a scenario.

It turned out that the SLAC beam dump was potentially a superior source for our kind of axion. So I joined forces with Luke and colleagues, and we submitted a proposal to SLAC. It was approved, which led to the curious situation of me commuting back and forth to California for the next few years as a novice experimentalist on SLAC Experiment E137.

The experiment was a shoestring operation, but we got very good support from the SLAC infrastructure. Our detector was located 400 m downstream of the dump. The first 200 m was a hill, through which our axions had to penetrate. The last 200 m was a valley, where our axions would be above ground and able to decay into quanta visible to the detector. We eventually got some running time dedicated to our experiment (not easy to obtain!), with 20 Coulombs of electrons delivered to the beam dump for us.

We observed a rather clean zero for axions with momenta larger than about 1 GeV. This allowed us to claim an exclusion region in the two-dimensional axion parameter space (axion mass, and axion coupling to photons) (38). At the time of the proposal, we anticipated that we would be sensitive to several square orders of magnitude in that parameter space. But in the interim the astrophysicists invaded our territory. By the time our experiment was finishing up, most—but not all—of our territory was covered by astrophysical limits.

By the end of that effort, I had a much sharper understanding of what it means to do even the smallest of experiments. The amount of work, and the depth and breadth of the many social interactions needed to get the job done, is huge compared with what it takes for a theorist to come up with a publishable piece of science. Experimental physics is in its way an extremely rich and satisfying way of life. But I was left wondering whether the very modest scientific benefits of E137 were worth the cost in time and effort.

What I did not anticipate at all was that, more than 20 years later, E137 would be relevant in the search for dark matter and, in that context, would claim an exclusion region consisting of several square orders of magnitude within a totally different two-dimensional parameter space (39).

Meanwhile, back at Fermilab, life was not at all uneventful. My wife, Joanie, was offered a job in the guest office, which she accepted. She was responsible for the well-being of visiting physicists and their family members. She was perfect for that job. In a very short period of time, she became a beloved member of the Fermilab family thanks to her outgoing personality and her linkages to most if not all of the departments in the lab.

Alas, this did not last for very long. Joanie contracted an aggressive form of salivary gland cancer. It was battled via surgery, radiation (from the Fermilab neutron therapy beam), and chemotherapy. But the battle ended in sudden failure in the autumn of 1983. The entire laboratory mourned her passing, and a memorial tree was planted outside the Hi Rise. For me, this entailed a lifestyle change, which for the rest of the decade kept me much closer to the lab and to my daughters, who were about to enter high school.

12. LATE FERMILAB

During the 1980s, the future of the national high-energy physics program was hotly debated. CERN already was building LEP, and it was evident that the LEP tunnel could eventually house an LHC, which would be much larger than the Fermilab ring. In addition, Brookhaven was promoting a heavy-ion collider program.

I was introduced to the Brookhaven idea by Bill Willis, by visiting postdoc Larry McLerran, and by promotional material created by T.D. Lee. Larry was working on the properties of the heavy-ion fragmentation regions, and we often discussed the issues (40). After I gave a short journal club talk on T.D.'s ideas (41), I got hooked. Nobody had yet looked at the properties of the central plateau region of phase space. Dealing with that problem was for me both natural and very simple. Long before, I had worked with Stan Brodsky on a hydrodynamical model of particle production in electron–positron annihilation (Wrong!) (42) and thereby learned the fundamentals of Landau hydrodynamics. That was easily linked with my experience with the central rapidity region. The result was a piece of theory much simpler than what Larry was dealing with. A paper was quickly and easily produced (43). By now that paper has thousands of citations. In hindsight, I wish it had been jointly written with Larry.

It was not only Brookhaven that had to worry about its future. Fermilab had to plan for a future beyond the Tevatron. Russ Huson and others in the accelerator division promoted a huge ring called the Desertron. Leon liked the idea and signed on. The Desertron idea soon became more refined, and it evolved into the Superconducting Super Collider (SSC) initiative, with Leon leading the way (44).

As usual, I became a contrarian. Together with others from the lab, I argued for a relatively modest site-filling proton–antiproton "Dedicated Collider" (DC) ring to be built quickly and to be on the air ahead of any LHC that CERN came up with. Leon was no enthusiast, but he did not suppress our efforts. In fact, he helped us put the proposal in good order for the national committees to consider (45).

Of course, the DC proposal went nowhere. And I eventually got personally involved in the SSC experimental program (more on that later). In 1991, I gave a talk at an SSC conference, where I confessed that I had been wrong about the DC (46). A major motivation for my choice of the DC option was the political risk associated with the SSC option. Now, with a site selected and with a tunnel about to be dug, it was clear that I was wrong. (Wrong! Wrong!)

Back in the Fermilab program office, I became interested in the spectroscopy and decay properties of hadrons containing heavy quarks. I believed there were opportunities in the fixed-target program and worked as best as I could with the experimental community to flesh these ideas out. The heavy quark effective theory created by Howard Georgi (47) was especially helpful and provocative. Baryons containing more than one heavy quark became an interesting subfield, although at that time the production and detection of such objects appeared to be very challenging. I put together a "Rosenfeld table" for them with estimates of their masses and decay properties (48). By now there is actually a considerable database. Someday I may, just for fun, dig out my old predictions and see how well (or badly!) I did.

All through the Fermilab years, I maintained a liaison with the theorists in the accelerator division. My interest in that subject already had been triggered by Burt Richter at SLAC. Accelerator theory is in fact a beautiful subject. I learned about it via a classic tutorial written by Matt Sands (49). By the time I left for Fermilab, I was eager to learn more. During the design of the Tevatron, Alvin Tollestrup alerted me to a research problem regarding intrabeam scattering. The mutual Coulomb repulsion of the electrons or protons within a bunch leads to beam growth, and the study of this problem for strong focusing storage rings and accelerators was in its infancy. The guru was Anton Piwinski at DESY (50). As I was working on this, a theory postdoc, Sekazi Mtingwa, came to me looking for a problem. Intrabeam scattering was all I had to offer. Sekazi and I joined forces, and in the long run he took over the subject from me (51). It launched a very successful career for him in accelerator science—something that has given me a great deal of personal satisfaction.

A little later on, one of the hotter research topics was filling out the entries in the Cabibbo–Kobayashi–Maskawa (CKM) matrix. As mentioned above, I was a promoter of high-precision, fixed-target experiments featuring the b quark. In the Fermilab theory group, there was a very good postdoc named Isi Dunietz, who was interested in heavy quarks as well. We joined forces, and one of the by-products was the creation of what is now known as the unitarity triangle (52). [We were unaware that this construction had originally been discovered years before (53).] It is one of those examples of how a choice of descriptive language (in this case, it was the introduction of a single picture that supplemented the many words and equations used up to that point) can by itself make a significant difference. By now, the importance of linguistics in science has become for me, for better or worse, a mantra.

13. THE RETURN TO SLAC

In 1989, Leon Lederman stepped down as director of Fermilab. In addition, my daughters had left the nest and were enrolled in college. It was clear that I should leave Site 29 for a different residence. The only question was whether it would be in Illinois or in California. I opted to return to Stanford, where I took a job as permanent staff, not faculty. SLAC director Burt Richter wanted to keep as many faculty billets open as possible—something that made perfect sense to both of us.

But my linkage to hadron colliders was not broken by that decision. At Fermilab, I had already become a convert to the SSC project. I was also enamored of the idea of an "electronic bubble chamber" that could look at complete events. Event by event, all the charged particles, no matter where they were produced in phase space, were to be detected, with momentum accurately measured. To do this required a novel detector design—long and narrow, like the SLAC linac. I produced a single-author letter of intent to the SSC (54). Director Roy Schwitters treated it kindly. A small working group coalesced around the idea, and soon we were meeting in Waxahachie and fleshing out a physics agenda.

Unfortunately, other things were happening at the same time. In particular, the SSC project was abruptly canceled by Congress. Our little working group by that time had come up with some novel physics ideas. One of them—very speculative—was the production of "disoriented chiral condensate." The idea was that in a high-multiplicity, high-energy hadron–hadron collision, the interior region of the arguably large "fireball" might cool down enough to create something close to the QCD vacuum state, which, according to QCD, possesses a slightly broken chiral symmetry. Our presumption was that the orientation of the vacuum expectation value inside the fireball was uncorrelated with its orientation on the outside (the sigma direction). After the fireball burst, this false vacuum would then decay semiclassically, with large event-by-event fluctuations of the ratio of neutral pions to charged pions (55).

Our eyes turned to the Fermilab proton–antiproton collider as an appropriate venue. We submitted a proposal to the laboratory, which was now led by John Peoples. He was not very happy with a small maverick experiment that would be located in interaction region C, halfway between the major detectors CDF and DO, which were located in collision regions B and D. Those big experiments were hot on the trail of discovering the top quark. We were merely sand in the gears. Nevertheless, in a weak moment, John grudgingly gave us approval as T864 (the T stood for test). Our group included Dick Gustafson (University of Michigan), an aficionado of small experimental enterprises and scavenger of discarded experimental apparatus, especially electronics. In addition, we had an ex-string theorist, Cyrus Taylor, who joined the group early on as a way of getting closer to real-life physics. Cyrus in turn enlisted fellow experimentalists at his home institution (Case Western Reserve). They were led by Tom Jenkins, who quickly provided us with tracking chambers.

We quickly established friendly diplomatic relations with the accelerator division and the safety division. We needed a custom-made beam pipe with a window through which our detector could view the collision region. It was a rather delicate object that, if ruptured, could shut down the entire collider program. All this created angst in the director's office. John Peoples was not amused.

Despite the presence of a lot of politics (some of which originated within our group, which at times disagreed on the design of the tracking system), we succeeded in installing the experiment, learning by doing, rebuilding when necessary, acquiring dedicated running time, and removing the apparatus from the tunnel in a scant 3 years. By the end of this short adventure, Cyrus had taken over the leadership as well as the data analysis. No disoriented chiral condensate was found. But the data were good, and the results publishable (56).

In the collaboration we had a computer expert, Jon Streets. One day, he came in and told us that he had installed some new software. It was something called the World Wide Web. It was, of course, of great use to us. Personally, I was totally clueless regarding its potential importance. Our website was actually one of the first hundred in the United States. It is still there: Just google "MiniMax Fermilab."

While the T864 experience was for me personally rewarding, the physics results were quite modest. Again, just as for E137, I worried about the benefit–cost ratio of the enterprise, especially for Cyrus Taylor. Were those years of use to his career? When I recently expressed this concern to Cyrus, he responded by saying that those years were in fact quite valuable. He learned social skills in dealing with the politics inside and outside the collaboration, which he has put to great use in the interim. Cyrus moved up through the administrative ranks from department chair to dean of the College of Arts and Sciences. (At present, he has retired from those responsibilities and has returned to the research world.)

14. EARLY RETIREMENT

In 1999, after 10 years back in California, I reached retirement age. Without hesitation, I retired. But since then I have retained a desk and computer terminal within the SLAC theory group, and I remain somewhat wired into the SLAC enterprise. However, my physics interests have shifted toward gravitation and cosmology. Although I am quite an amateur in these fields, I am finding a lot of enjoyment learning the trade.

My initiation into the field came via contacts with graduate school classmate Ron Adler. In the interim, he had coauthored a textbook on general relativity (GR) (57) and had become involved in the Stanford-based Gravity Probe B experiment headed by Francis Everett (58). Even after completion of that experiment, their theory group continues to meet weekly in an informal format. I have become a regular at those meetings. In addition to being an excellent way to keep up with what is happening in the field, I also can vet my own crazy ideas to the group and obtain very useful feedback.

One of the crazy ideas was not my own invention but instead a favorite of Stanford colleague Bob Laughlin (59). It is now called a gravastar (60). The premise is that the interior of a black hole consists of dark energy. The challenge is to understand what happens near the horizon and also to understand the formation history. Together with Ron, Bob, and others—in particular Emil Mottola—I learned black hole and cosmology lore and was soon speculating that Standard Model parameters might be correlated with the magnitude of the dark energy. This in turn led to my own speculations regarding the anthropic principle and multiverses, which made the string theory versions look conservative (61, 62).

By now I have set most of those thoughts aside. But what has been central all along is the challenge of understanding dark energy itself. I consider it a perfect problem for a retiree like me. It is of course extremely important and fundamental. And, since I presume that dark energy is best described as a cosmological-constant contribution to an effective field theory for gravitation and for the Standard Model, the data are already in place.

Those early retirement years were not dominated by black holes and inflation. Two SLAC postdocs, Natalia Toro and Philip Schuster, together with Rouven Essig, were working out the consequences of assuming that dark matter consisted of a light spin 1 particle that was mixed with the photon. After looking at the constraints from collider physics, they turned to fixed-target physics, including beam dump experiments. Michael Peskin informed them that I had a history in that field. After they contacted me, I got hooked on the idea as well, and I worked with them on the constraints from existing experiments and also on how to do better (39). As mentioned above, it turned out that our old E137 experiment was an important contributor.

Another early retirement distraction was due to Hong-Mo Chan and his collaborators. They described the parameters of the CKM matrix in terms of what they dubbed "the rotating mass matrix" (63). I created my own version of their idea, which can be found in one of my publications (64) (see also the Rotating Mass Matrix section of my website at **http://bjphysicsnotes.com** for more details). It "predicts" numbers for all the lepton and quark masses and mixing angles. The arguments that produce these numbers are admittedly very fragile. What is most interesting to me is that no one has bothered to query me as to how I got those numbers. I also put the table of results on the blackboard in my SLAC office. I would sometimes point it out to a visitor. The typical reply would be "Oh." And then the visitor would quickly change the subject.

I regard this subject as, at best, a possible solution to an important physics problem, the proper statement of which remains to be discovered. I have returned to it every now and then but have not made much progress. But I am nowadays giving it another try.

15. LATE RETIREMENT

In the last few years, I have continued to struggle with the dark energy problem. I have received much help from members of the loop quantum gravity community—especially at Perimeter Institute, Penn State, Cambridge, and Marseilles. These include, in particular, Lee Smolin, Laurent Freidel, Abhay Ashtekar, Martin Bojowald, Anthony Lasenby, Chris Doran, Carlo Rovelli, and Alejandro Perez. I thank them all for teaching me so much. I have developed a preference for the version of GR called gauge gravity (for an excellent survey, see Reference 65). The formal structure is a non-Abelian O(3,1) gauge theory. The basic building blocks, instead of the 10 components of the metric tensor, are the 24 degrees of freedom of the gauge potential (spin connection) plus the 16 components of the vierbein (a vector with respect to the substrate variables x, y, z, t as well as a vector with respect to the gauge group). While this formalism is well known to the cognoscenti, it really is not the lingua franca of the majority of the GR theory community. However, it is more spinor-friendly than the Riemannian language. And, being a Yang–Mills theory, it has a closer kinship with the world of Standard Model particle physics. Since everyone looks for unification of gravity with particle physics, I am surprised that this version is not more popular.

Anyway, this is what I have been playing with. And I cannot help but add here my present interest. It consists of a beautiful, challenging problem in pure theory. I am studying generalizations of a solution to the vacuum Einstein equations that has been around for nearly a century. It is the Kasner metric (66), and it describes a cosmology in which the expansion is anisotropic but homogeneous. The Friedmann–Robertson–Walker scale factors evolve as fractional powers of time. The Einstein equations require that the sum of the powers adds to unity and that the sum of the squares of the powers also adds up to unity.

What seems to be, as far as I can tell, unexplored territory is to generalize this idea to (*a*) inclusion of a cosmological constant; (*b*) the presence of extra compactified dimensions à la string theory, which are also allowed to expand or contract with cosmological time; and (*c*) generalizations of Einstein gravity, which allow actions that are polynomial in the curvature tensor.

For this latter point, there is an especially user-friendly class of candidate gauge–gravity Lagrangians. They consist of polynomials in the field strength F and generalized vierbein ("vielbein") variables e, with all gauge indices and all substrate indices antisymmetrized via the Levi-Civita tensor. I am deep in the middle of this problem, which is still tractable at the level of hand calculations. It is too early in the game to anticipate whether it will be useful in elucidating something about real-life cosmological history. But I am rather sure that it will occupy me until I no longer am able to continue doing physics. After all, at my age that may be in the near future.

16. PRIZES AND ALL THAT

Although I have been blessed with some prizes and honorifics, I have mixed feelings about them. The negative side began early on with the Nobel Prize. Early in my career, it was evident that there were people out there more motivated by The Prize than by the simple desire to learn the answers to the scientific problems of the day. For me, The Prize was an invention of the devil.

Since then, my negativity has softened. The Nobel Prizes are among the most effective ways of communicating physics results to the public at large. I certainly personally benefit by learning, via the Nobels, about the progress in fields other than physics. However, I would prefer to see fewer prizes out there that go to tenured old-timers holding endowed chairs at major universities, and more prizes (or other forms of support) for talented untenured youngsters who would not only appreciate the financial windfall but also appreciate the addition of such a prize to their resumes. After all, talent alone is not sufficient nowadays to guarantee a permanent job within the field, especially for those who choose topics that are not politically correct. In my opinion, too many mavericks are at present being forced out of the field.

Nevertheless, I do appreciate the honorifics that have come my way. The Lawrence Award was presented in the West Wing of the White House with my parents in attendance. This was especially satisfying to my mother, who grew up in Washington, DC, very close to the Capitol. The deep-inelastic physics was recognized (appropriately!) by a Nobel Prize awarded to my Group A SLAC-MIT colleagues. As a guest of Henry Kendall, I got to go to the festivities in Stockholm.

While Swedes have a reputation for being rather stiff and formal, this was, to my surprise, not the case there—it was one blast of a party.

I have already mentioned the Wolf Prize, awarded in the Knesset in Jerusalem. A highlight of that occasion was meeting fellow recipient Murray Perahia. He expressed his thanks with an impromptu performance of the *Moonlight Sonata*.

More recently, I became a corecipient of the Wilson Prize in accelerator theory, together with Anton Piwinski and Sekazi Mtingwa (67). To my surprise and satisfaction, our intrabeam scattering work turned out to be rather broadly applicable. I am absolutely delighted that Sekazi got this recognition. Sekazi's public service roles include promotion of accelerator physics in Africa and promotion of African American physicists everywhere. Being a Wilson Prize recipient obviously does make a substantive difference for him.

Honorifics that have come my way would not have happened without nominators. The task of nomination is nontrivial. I would like to take this opportunity to recognize those individuals, some of whom are known to me and others who are not. Thank you all.

17. EPILOGUE

This rich journey through the world of physics has been, and continues to be, enormously satisfying for me. It was a golden age for particle physics. And the environments in which I worked were enriching and pleasurable. I am profoundly thankful that this occurred, and I am especially thankful for all those friends and colleagues who made this experience possible.

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Contents

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