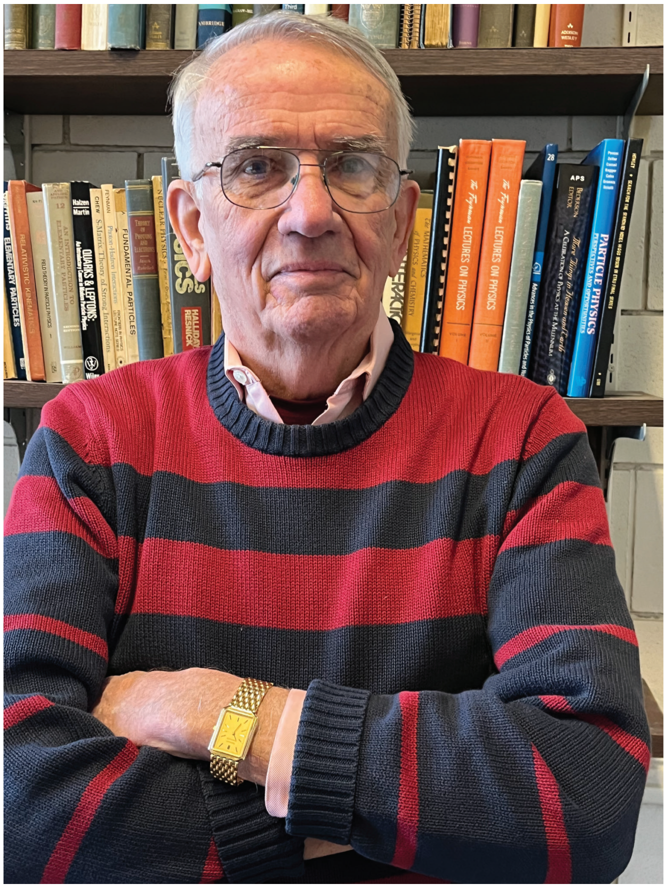

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Annual Review of Nuclear and Particle Science

An Experimental Life

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Abstract

Over the past 60 years, particle physics has seen the maturation of its Standard Model and an enormous change in the character of the experiments that have defined it. I have had the good fortune to participate in and help shape this evolution.

Contents

1. FORMATIVE YEARS

I grew up just outside Dayton, Ohio. My childhood interests were mostly in any kind of sporting activity—I aspired variously to be a tight end, a high jumper, or a shortstop, not understanding yet that my talents and stature were insufficient for any of these. I spent days exploring the woods and streams in the countryside not far from our home, allowed to roam freely so long as I returned by dinnertime. My father was an architect, turned into a civil engineer by the Depression. In keeping with the times, my mother left her early career as a teacher for the sake of family, but she had wide interests. They set very high academic standards for my older sister and me. I did enjoy primary school—particularly when it had some sort of analytic or competitive character. Most math seemed irredeemably lame, but I liked the one-on-one contests to give the answer to a flash-card calculation, in which the winner stayed alive to challenge the next student. The goal was to make it all the way around the classroom successfully. And one of my more pleasurable pursuits was diagramming the structures of sentences. I don't remember anything interesting in any of the courses labeled as science; they mostly seemed like make-work or dull facts.

The hook that initially brought me to science was astronomy. A visit to the Hayden Planetarium in New York when I was 11 and buying their pamphlets opened a wonderful world to ponder. Those on the moon and the solar system were interesting, but the description of our galaxy and what was then known to lie beyond it were even more so. While walking the dog on clear winter nights, I looked at the stars and moon and marveled at being able to see the Andromeda Galaxy by glancing at it from the corner of my eye. I even developed the conviction that I was of the right age, the right temperament, and, given time, the right training to become the first person to walk on the moon.

In high school, I began to appreciate the rigor and challenges of mathematics, and of all the science courses, I most enjoyed physics. From this, and my teachers' encouragement, I began to sense that I would pursue some sort of mathematics-based career. In 1956, in a rare display of overconfidence, I submitted only one college application: to Cornell University, where my father and sister had preceded me. Rather than aiming at astronomy or physics, I chose engineering physics, partly because my family's expectations were for the more practical professions—and partly because I had heard that this was the most challenging curriculum available.

2. CORNELL

The engineering physics program had many applied physics and engineering courses, but as time passed it became increasingly clear that more fundamental science was my thing. I greatly enjoyed courses in complex analysis and statistical physics (that the statement "all states are a priori equally likely" was able to explain such a vast array of phenomena was mind-boggling). At the time, Cornell had only a two-person astronomy department with barely any students and few courses, all of which I took. In my last year I became a teaching assistant (TA) due to the lack of grad students. So my astronomy interests were not wholly eclipsed.

Although the collection of math, physics, and engineering courses demanded a good bit of time, I was determined to make the most of other opportunities at Cornell. Courses such as music appreciation, twentieth-century art, and British poetry laid a basis for much that I have enjoyed ever since. I also decided to try my luck as a diver on the swim team despite never having had any formal coaching. The first year was basically spent undoing the bad habits acquired from self-taught summertime pool experiences. In time, I became passably expert and competed creditably at the Ivy League level.

I had summer jobs at the Battelle Memorial Institute seeking new materials for tunnel diodes and at DuPont Research Labs programming a primitive computer (with 65 jumper wires on a patch panel) to describe the relaxation of macromolecules in a fluid after alignment in a pulsed electric field. These were interesting, but they did not excite me. Through these experiences, the notion I had when beginning college that industrial research would be my career was abandoned. I came to see that basic physics research was my path.

I learned a great deal from my senior thesis with Tommy Gold, explaining how the moon's craters were filled with dust by the hopping of small grains, electrically charged by cosmic rays. Gold was an interesting character. He had little mathematical acumen but was very long on innovative thinking. The steady-state model of the universe that he proposed with Hermann Bondi and Fred Hoyle postulated that as the universe expands, its density keeps constant through the creation of new protons and electrons, thus maintaining an unchanging cosmos. The created mass energy was offset by the gravitational potential energy due to the preexisting matter. Although the big bang theory ultimately carried the day, I felt that the steady-state universe idea had elegant simplicity. The thesis project led to my first publication, "Electrostatic Erosion Mechanisms on the Moon" (1).

But my biggest influence at Cornell was Ken Greisen's course on nuclear and particle physics, taught from his own lecture notes. He wove together the story of how relativity and the role of symmetries, taken in conjunction with seminal experiments at increasingly powerful accelerators, guided the development of the new field of particle physics. Though this course was popularly regarded as the most difficult in the syllabus, for me it seemed like a delightful feast.

3. BERKELEY

I limited my applications to graduate schools to those with good departments of both physics and astronomy, imagining that my career would be at the interface. In 1961 I went to the University of California at Berkeley because of its fine reputation and also because up to then I had lived in the East and thus welcomed a chance to explore the left coast. A Danforth scholarship freed me from needing a TA stipend, so I was able to add the astrophysics courses, in which I did well due to having a better physics background than the astronomy majors. But the available research topics seemed to center on elaborating stellar evolution models that emphasized detailed calculation more than exploring new ideas, so I gravitated toward particle physics—at the time, a thriving enterprise on "the Hill" above campus at the Lawrence Radiation Laboratory (LRL, now LBNL).

Like so many students whose experience had largely been lectures and textbooks, I fancied becoming a theorist. Steve Weinberg taught the field theory course (he gave us a textbook from which to learn standard field theory on our own and lectured on general relativity and Feynman rules for particles with spins greater than one). At the end of the course I asked him if he would take me as a thesis student; the answer was a terse, if not unfriendly, "No!"

At the same time I started a summer job on the Hill with Owen Chamberlain's group and became enamored with the life of an experimentalist. The late nights on the beam line arguing physics with my informal faculty mentor Herb Steiner and fellow students, and seeing the different roles involved in the experimenter's life, were most appealing. And so the die was cast—I relished the experimentalist's life, and in any case would probably not have been so happy or successful as a theorist. It was probably just as well since my talents were not to excel in a particular aspect of physics so much as to be moderately good at several.

At the time the Chamberlain group was doing a series of measurements using a polarized target made of crystals with protons in its waters of hydration. With the crystals at 1 K in a strong magnetic field, the electrons were naturally polarized. Optical pumping could transfer that polarization to the protons with either spin orientation. We measured the polarization and spin correlation parameters in a variety of two-body scattering reactions at both the Bevatron and the 184-inch cyclotron, as well as a determination of the relative $K\Sigma$ parity. My thesis was the measurement of the polarization parameter in pp scattering between 1.7 and 6.15 GeV (2).

The prevailing Berkeley religion for high-energy scattering at the time was Regge poles, so I interpreted my results in that framework. The Regge phenomenology is based upon singularities (Regge poles) moving in the complex angular momentum plane. We assumed that we were at high-enough energy that only two poles were important—the pomeron that governs very-high-energy scattering and another that is odd under a charge conjugation (C) transformation. Nonzero polarization arises from the interference of the two. As the center-of-mass energy ($\sqrt{s}$) increases, the polarization at fixed momentum transfer (t) would decrease linearly as the C-odd pole contribution goes to zero. My results agreed with this and indicated that the second pole was roughly consistent with the ω meson trajectory, as predicted. In my thesis defense, when I was asked to explain how a quadrupole magnet worked and I muffed it, Owen quipped that I seemed more comfortable with Regge poles than with quadrupoles. I blush now at the hubris of thinking that $\sqrt{s} = 3.6$ GeV was high energy. For much of the rest of my career, Regge poles did not figure, but in its waning phase, they reappeared like a bookend, albeit then at the TeV scale.

In my last couple of years at Berkeley, I set out to obtain fits for the set of partial-wave amplitudes for pion–nucleon scattering using the available cross section and polarization data in πN elastic and charge exchange processes. The goal was to identify the baryon resonances below about 2 GeV using fits in a parameter space of tens of amplitude and phase variables. This led to my building a precursor of the now-ubiquitous MINUIT program. It also led to a graphic lesson on the complexity of higher-dimensional spaces and the difficulty of finding the one true minimum among the forest of false minima.

4. STONY BROOK, BROOKHAVEN, AND AN ENGLISH INTERLUDE

In early 1966, after a 1-year postdoc at LRL, I felt the call of changing seasons and began looking for a position at some Eastern institution. Several well-established universities offered me postdocs, but a few, just starting up in the general climate of expansion following Sputnik, were more aggressive and ready to make faculty appointments. Among these, the State University of New York at Stony Brook had several attractions. The campus had only been completed in 1962 but was heralded by Governor Nelson Rockefeller as the "Berkeley of the East." At the time, planning was ongoing for the new accelerator to follow after the Brookhaven National Laboratory (BNL) Cosmotron and the Berkeley Bevatron. Berkeley and BNL seemed to be the frontrunners, and in my optimistic view the new machine would be sited at BNL, where there was sufficient flat land to accommodate it. This was my first failure to recognize the importance of political considerations in big science, as the next machine was awarded to a green field site near Chicago through a deal between President Lyndon Johnson and Senator Everett Dirksen.

The Stony Brook area was a delightful collection of old villages amid a complex of harbors on Long Island Sound, but the campus was an aesthetic wasteland. Nevertheless, the decision of prominent theorists like Chen Ning (Frank) Yang, Ben Lee, and Gerry Brown as well as the proximity to BNL convinced me to accept an offer to help build a physics department from scratch. And so, my first wife Dorothy and our daughter Jennifer (soon to be joined by sister Eliza) trekked back across the continent.

A major factor in my decision was that Myron (Bud) Good had agreed to leave Wisconsin for Stony Brook. Bud was an imaginative person who had many wonderful, if sometimes impractical, ideas. He had helped introduce the ideas of diffractive dissociation, regeneration of K_s mesons in matter, and electrostatic separators. At Stony Brook he developed the phenomenology of pulsars as neutron stars and invented a scheme for accelerators using fixed-field superconducting dipoles that counter-rotated to give an increase in net field as the beam energy increased.

Juliet Lee-Franzini had come to Stony Brook a few years before and, with her husband Paolo Franzini at Columbia, had built a strong research program at BNL and Nevis Labs. Paolo helped convince me that accepting the Stony Brook offer was a better choice than a Columbia postdoc, and Juliet was an important mentor to me over the years. Although their research then used bubble chambers, she advocated that I join the new high-energy group using electronic techniques under Bud's leadership. Bubble chamber experiments had enjoyed prominence in the 1960s due to their ability to see the full set of particles in collisions and their decays, but electronic experiments—with their vastly larger data sets, ability to trigger on events of interest, and many detector innovations—provided increasingly detailed representations of events.

I actually arrived in Stony Brook before Bud and began thinking of possible experiments for us to do. Over the next couple of years, the formation of the core High Energy Counter Group was completed with the arrival of Janos Kirz from Berkeley and Guido Finocchiaro from CERN. Our first experiment was a measurement of the associated production reactions $\pi^{\pm}p \rightarrow K^{+}\Sigma^{\pm}$ (3) at the BNL AGS, in which we sought to explore symmetries between strange and nonstrange particle production. The technical infrastructure at Stony Brook was primitive, so we bought our spark chambers. In dealing with the vendor, I said, "I need. . . ." Bud pulled me aside to instruct me that I should have said, "We need. . .," and thereby gave me an important lesson in how collaborative research ought to be done. Over the succeeding decade, I was greatly influenced by Bud's integrity and imagination, Janos's mentorship and deep understanding of physics, and Guido's quiet but passionate approach to experimentation.

In 1969 I was awarded a Sloan Fellowship and originally thought I would spend a year at CERN. However, friends argued that working in England was a delightful experience, and anyway I could always hop across the Channel for visits to Europe. And so I went to the Rutherford Lab near Oxford where, although the Nimrod accelerator was nearing the end of its productive life, good opportunities still existed for studies of neutral K and other light mesons. After the observation of CP violation in K^0 decays, T.D. Lee and others argued (4, 5) that it might be due to C violation in electromagnetic interactions, which had not been well studied experimentally. The favored channel involved differences in π^+ and π^- kinematics in $\eta \rightarrow \pi^+\pi^-\pi^0$ decays, and my timing was right for joining Norman Lipman's experiment on C violation in η decays. This study had an interesting connection to our Stony Brook group: In 1966, the Franzinis had found evidence for an asymmetry of 7.2 ± 2.8% in a BNL bubble chamber experiment (6), whereas in that same year Finocchiaro and others working on an electronic experiment at CERN had reported a null result, $A = 0.3 \pm 1.0\%$ (7). Our result put the question to rest with a measured asymmetry of 0.28 ± 0.26% (8). So our quest for C violation failed, but while at the Rutherford Lab I did complete a most successful search by finding my future wife Barbara at the holiday party of a student in the group.

Over the succeeding decade, our Stony Brook group worked on AGS experiments, which typically involved six to eight people studying various two-body scattering processes, and a measurement of anomalous electron production in 24 GeV/c pp collisions (9) with Gene Beier and Howard Weisberg at the University of Pennsylvania. Higher-energy experiments at Fermilab and the CERN Intersecting Storage Rings (ISR) had seen high-p_{T} anomalies that were attributed to a cocktail of the decays of known particles and the charmed mesons expected after the 1974 J/ψ discoveries. The Penn–Stony Brook experiment concluded that the rate of single electrons produced at $0.5 < p_{\mathrm{T}}(e) < 1.0$ GeV/c could not be fully explained by decays of known particles. I then joined with Howard Gordon and his group at BNL in an experiment searching for associated production of charmed mesons and baryons at the Multiparticle Spectrometer facility at the AGS. I expanded the goals to search for anomalous e^+e^- pair production and built a set of lead scintillator electromagnetic calorimeters. We found a pair rate that exceeded the expectations from known particle decays and was consistent with being the source of our previous measurements of anomalous single electrons (10). The source of the pair anomaly was not pinned down definitively but was consistent with models in which a precursor of the quark–gluon plasma was created, giving an enhanced amplitude for Drell–Yan production (11). However, charmed particle discovery eluded us.

5. THE CERN INTERSECTING STORAGE RINGS

The CERN ISR accelerator was proposed in 1964 to study proton–proton collisions in the range $\sqrt{s} = 23.5$–52.8 GeV (later extended to 62.7 GeV by accelerating the beams after injection). The ISR was the first hadron collider, and it introduced a number of innovations that were adopted by succeeding machines, including radio frequency (RF) stacking to obtain higher beam currents and stochastic cooling to increase the luminosity. Although the deployment of detectors was quite different from that at fixed-target experiments, the nature of the initial experiments themselves was rather similar to that of their predecessors in focusing upon a relatively limited set of questions. This was quite unlike the general-purpose 4π solid-angle detectors resembling electronic bubble chambers that were then being pioneered by Burt Richter for studying e^+e^- collisions at SLAC (12). This transition to complete event reconstruction with digital readout was a paradigm shift in experimental physics: No longer were electronic experiments forced to study relatively narrowly drawn questions; instead, they became capable of broadband studies of many phenomena simultaneously.

When CERN called for proposals for experiments, Giorgio Bellettini and his Pisa group proposed a measurement of the pp total cross section. Giorgio had great respect for Finocchiaro and invited him to bring a group from Stony Brook to join them. The method was straightforward: Detect the rate at which collisions of any sort occur and measure the luminosity, the proportionality constant between cross section and event rate that depends on the number of colliding protons, the geometrical sizes of the beams, and the frequency of their crossings. The Pisa group built the scintillation counter hodoscopes that surrounded the interaction point and enabled the measurement of the total rate. The Stony Brook contribution was a large-angle spectrometer, later expanded to include a lead glass array for π^0 detection. The total collision rate was deduced from time-of-flight spectra between hodoscopes on opposite sides of the interaction point. The luminosity measurement using the van der Meer method (13), based on the collision rate as a function of the vertical separation of the two beams, was supplemented with measurements of the beam sizes using the Stony Brook spectrometer.

The expectation based on measurements at lower energies in fixed-target experiments was that $\sigma_{\rm tot}$ would approach a constant as $\sqrt{s} \to \infty$, though there were hints from cosmic ray measurements of a rise at very high energies. By early 1973, the Pisa–Stony Brook measurements showed an increase with energy with a significance for departure from energy independence of about 3σ (14). A result obtained by the CERN Rome group of Ugo Amaldi and Giuseppe Cocconi using a measurement of the elastic pp cross section and the optical theorem (15) showed a similar increase in $\sigma_{\rm tot}$. Although the rising total cross section came as a surprise to most of us at the time, it had in fact been envisioned by Heisenberg (16) in 1952 based on a picture of the expanding size of the overlap region for inelastic reactions of the two highly Lorentz-boosted colliding disks. The CERN Rome publication was actually ready for submission a couple of weeks before the Pisa–Stony Brook result was finalized. It is to Amaldi's great credit that CERN Rome waited and the two papers were submitted and published together. Although the impact of the rising $\sigma_{\rm tot}$ was somewhat diluted with the discoveries of quarks, leptons, and gauge bosons beginning in 1974, it remains a profound mystery that the "size" of colliding particles seems to grow with energy, apparently without limit.

The Pisa–Stony Brook total cross section measurements continued with more precise $\sigma_{\rm tot}$ determinations, the incorporation of a new method employed by the joint CERN–Pisa–Rome–Stony Brook collaboration that combined total and forward elastic rates to yield luminosity-independent $\sigma_{\rm tot}$ (17), and later on, measurements of the rising $\sigma_{\rm tot}$ for $p\bar{p}$ collisions (18). In the Regge pole framework, the difference $\sigma_{\rm tot}(pp) - \sigma_{\rm tot}(p\bar{p})$ can be attributed to the presence of the subdominant ρ and ω poles and a possible $C = -1$ odderon companion to the $C = +1$ pomeron. The measured difference as a function of energy was consistent with the diminishing contribution from the ρ and ω terms at high energy but agnostic with respect to the existence of the odderon.

Many other features of high-energy scattering were studied, including two-particle correlations in rapidity (19), the angular dependence of high-$p_{\rm T}$ π^0 production and demonstration that the hypothesis of radial scaling is not valid (20), and particle multiplicity correlations with high-momentum-transfer photons (21).

The two sides of the Pisa–Stony Brook collaboration, now comprising about 20 people, complemented each other, and I greatly enjoyed working with Giorgio, Nino del Prete, Lorenzo Foà, Paolo Giromini, Aldo Menzione, and all the others. Although there were some factions within the Pisa group, the presence of the Stony Brook folks as intermediaries seemed to moderate some of the frictions.

Life within the Stony Brook family was very congenial. Guido Finocchiaro and Anne-Marie Cnops introduced us all to the pleasures of the Geneva environs. Barbara and I lived for 6 months with Stony Brook postdoc Dan Green and his wife Andrea. Since the exchange rate between the dollar and Swiss franc dropped by about a factor of two during our first stay at CERN, the Stony Brook contingent, also including postdoc Rudi Thun and my students Bob Kephart and (later) David Lloyd-Owen, opted for cost-free activities like mushroom picking, hikes in the Alps, and games of charades.

My own family was growing. My daughter Nellie was now in her first winter, and Barbara and I strapped her on my back for cross-country skiing tours in the Jura. When I fell headfirst into snowdrifts, she, not knowing better, thought it normal and took it in stride. Jennifer and Eliza had been born in Berkeley and Stony Brook, respectively, and Nellie was born in London, so we imagined that a subsequent baby would arrive a further 3,000 miles eastward, perhaps in Moscow.

But in the event, our son David was born back on Long Island 2 years later. I have been very happy that the four children have been as close as full siblings. Over the years we have been together for holidays, physics workshops in the mountains, family gatherings, and now for their own children's events.

6. FROM ISABELLE TO THE TEVATRON

In the early 1970s while the ISR program was underway, BNL was developing plans for a $\sqrt{s} = 400$ GeV (later 800 GeV) pp collider based on superconducting magnets dubbed ISABELLE (the *ISA* stood for Intersecting Storage Accelerator). In a 1972 summer workshop at BNL, I worked with Green and others to outline an experiment based on ISR-inspired questions such as multiplicity and rapidity distributions, rapidity correlations, total cross sections, and an attempt to find the recently postulated hadron jets. We sketched out a nonmagnetic detector with proportional wire chambers and scintillators covering nearly the full solid angle.

The subsequent discoveries of the J/ψ meson at BNL and SLAC and the programs of the UA1 and UA2 experiments at CERN stimulated a wholly new mindset for a detector design. In 1978 I gave a talk in Stony Brook, "What to Propose at ISABELLE," that envisioned a detector with a central solenoid magnet, drift chambers, and a lead glass shower detector covering pseudorapidity $|\eta| \leq 1$. The concept, developed largely with my Stony Brook colleague Mike Marx, would focus on a range of topics, such as W and Z boson studies, high-p_{T} pion and single electron production, a search for toponium (for expected top quark masses of a few tens of GeV), searches for new physics in the bottom and charm sectors, heavy leptons, and even a nod at discovering a Higgs boson in the 10- to 20-GeV range. As a reaction to the somewhat fanciful, often pretentious, names being proposed for detectors, I suggested the name LAPDOG for our experiment with the postfitted acronym "Large Angle Particle Detector—Or Gammas." I was able to get a suitable doggy cartoon (**Figure 1**) with permission rights from my neighbor George Booth, the *New Yorker* artist. The LAPDOG detector and physics goals clearly foreshadowed much of the later D0 program. The dog was hated by some and loved by others, but it achieved a lasting presence as an 8-foot-high plywood replica made by Hans Jöstlein for a D0 office trailer.

By 1980, BNL had encountered problems in large-scale production of superconducting magnets, and the delays and costs were mounting. Early in 1981 the Fermilab Director Leon Lederman issued a call for a "small, simple and clever experiment" to be mounted in the D0 location in the

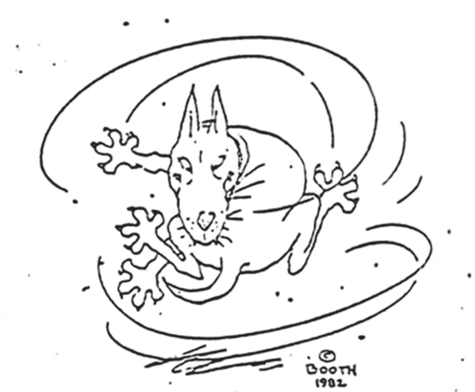

Figure 1

The George Booth LAPDOG. Cartoon reproduced with permission from the estate of George Booth.

Tevatron $p\bar{p}$ collider then under construction. Nineteen letters of intent were received proposing experiments that ranged from a stack of Lexan sheets for magnetic monopole detection to large 4π magnetic detectors, as well as the construction of an electron accelerator to allow study of *ep* collisions. The LAPDOG Collaboration—now including Sam Aronson, Bruce Gibbard, and Hywel White at BNL; Dave Cutts and Bob Lanou at Brown; Franzini and Mike Tuts at Columbia; Chuck Brown, Roger Dixon, and Jöstlein at Fermilab; Bernard Pope at Princeton; and the Stony Brook group of Rod Engelmann, Finocchiaro, Kirz, Lee-Franzini, Marx, Bob McCarthy, Dean Schamberger, and me—decided to throw the LAPDOG proposal into the hopper.

A portion of the LAPDOG group undertook a Fermilab test beam study of a 24-radiation-length-deep lead glass array with 12 layers, each with five unpolished extruded lead glass bars. We determined the electron energy resolution to be consistent with previous results using the more expensive polished lead glass. I investigated a technique to discriminate pions and electrons that used the covariance matrix of energy depositions of all 60 elements of the calorimeter. This "H-matrix" method (22) gave improved hadron rejection and also provided an estimate of the fraction of energy leaking out of the back of the array, based on the pattern of deposits in the instrumented detector. It found extensive use in the subsequent D0 program.

In summer 1983, the High Energy Physics Advisory Panel (HEPAP) recommended that the ISABELLE project be canceled in favor of the Tevatron $p\bar{p}$ collider in the near term and a more ambitious *pp* collider later on. With our focus now shifted to the Tevatron, the LAPDOG proposal evolved to have a central tracking region with no magnetic field, lead glass coverage out to $|\eta| = 3$ with interleaved proportional wire chambers, and toroidal magnets in the region between 10° and 30°. A part of our justification for no field was that at sufficiently high energies, the energy resolution of electrons using calorimeters is better than that attained by bending in a magnetic field. Another part was the more pragmatic need to contain the detector within the confines of the anticipated experimental hall. We were joined by Green and Ernie Malamud of Fermilab to add an iron scintillator hadron calorimeter and external muon detector. By this time, UA2 had found high-p_{T} jets (23) and UA1 had discovered the *W* and *Z* bosons (24) at the CERN S$p\bar{p}$S collider, thus giving more pointed focus to Tevatron proposals as well as a resounding justification for the 4π detector designs.

After the final presentations of the proposals by 12 surviving competitors, the Physics Advisory Committee (PAC) led by Vera Lüth retreated for its annual Aspen meeting to make a choice. Somehow the rarified mountain air emboldened the PAC, particularly the theorists Stan Brodsky and Tini Veltman, to do its own experiment. They declared that none of the proposals was sufficiently ambitious and recommended that all be rejected in favor of a new, sight-unseen experiment, already awarded Stage I approval and charged "to be no worse than LAPDOG." When Lederman asked me to be the sole member and leader of that new experiment, bringing together members of rival proposals to the extent that I saw fit, it took me several days of soul-searching to come to a decision that I would leap into the unknown and try to build such a collaboration. I had not pictured myself as the Carlo Rubbia of the prairie. The problem was compounded by the fact that the CDF Collaboration already had a 5-year head start and thus was likely to cream off all the good discoveries before a new competitor could get online.

7. THE D0 ADVENTURE

The longest-running (still ongoing) feature of my career began with Lederman's July 1, 1983, call that initiated the D0 experiment. A first meeting in Stony Brook later that month began the process of assembling a collaboration, initially bringing in the former LAPDOG and Fermilab muon proponents and the Michigan State component of a separate proposal that included Maris

Abolins, Chip Brock, and Harry Weerts. Our charge from the Lab was to provide an experiment that would start to run in 1986 and would carry on for a couple of years. The luminosity was expected to be about 1×10^{30} cm^{-2} s^{-1} (in the end, the Tevatron delivered over 400 times that!).

Up until Lederman's call I had planned to spend a year at SLAC working with Dave Burke and Bob Hollebeek on an early supersymmetry search experiment. Now with this new adventure commencing, Lederman urged me to come instead to Fermilab. It had been so hard to find accommodations near SLAC that accepted dogs that when Barbara and I went to Fermilab to discuss relocating, we agreed not to mention our pooch, until the housing staff asked, "Well, what pets are you planning to bring?" In August, with two grade-school children, Marco Polo the dog, and a promise to obtain a cat for Nellie, we moved to an on-site Fermilab house. The spirit of adventure and friendship in "the Village" among the visitors from around the globe was memorable. David happily joined in baseball games on the Village green with boys from Japan and Brazil in which the lack of any common language seemed to cause no problems. Jennifer and Eliza came for holidays.

Because of our focus on the physics that had been so successfully pursued by UA1 and UA2, we avoided inviting those who aimed at low-p_T studies, but before long we brought in the groups of Walter Selove at Penn, Brad Cox at Fermilab, Bruno Gobbi and Dave Buchholz at Northwestern, and Howard Gordon and Serban Protopopescu at BNL. With a set of people that had not yet jelled into a real collaboration, one of the earliest struggles was finding a name for the new venture. Various suggestions were found wanting, particularly any that incorporated any hint of a doggy theme. At least some of the names considered, such as GEM and BELLA, resurfaced to see new life in other contexts. But it was one of my signal failures to have settled on the mundane "DØ" taken from our address in the machine lattice. The slash in the Ø was a Fermilab convention, but over time we reverted to simply "D0" or "DZero."

The initial concept for the design was a small, field-free central tracking detector surrounded by electromagnetic and hadron calorimeters and a muon detector. The LAPDOG lead glass EM calorimeter was replaced with scintillating glass to improve the radiation hardness. But after trying for weeks to make the glass bar design viable, many arrived at a biweekly collaboration meeting in September convinced that it was cumbersome, too coarsely segmented, and too difficult to calibrate. Stimulated by Gordon and Franzini, we arrived at the decision that the calorimetry should be based on liquid argon readout with uranium absorber plates so as to allow fine segmentation, good hermeticity, stable unit gain, and radiation hardness. The choice of liquid argon followed the approach of the Mark II Collaboration (25) at SPEAR, and the uranium was motivated by the Axial Field Spectrometer (26) at the ISR and the belief that the energy deposits from fission products would compensate the energy lost to breaking nuclei apart and give a pion-to-electron response ratio close to one (27). Alas, no one in the collaboration had any experience with liquid argon calorimetry, and we were aware that some previous attempts to use it in a collider environment had failed. Working with uranium, available in quantity only from Oak Ridge National Laboratory's military armaments projects, turned out to have its own special problems. I recall going home from that meeting convinced that we had just added years to the gestation time for the experiment, and feared that we would never pull it off.

However, by December the nascent collaboration had produced a conceptual design report and presented it to the PAC. And the PAC saw what it had wrought and said "*Behold it is Good*." Fermilab took notice and set the new experiment in motion by adding Peter Koehler as my project co-manager and providing modest funding for design activities. However, the prototype modules and test beam campaign needed to develop experience and expertise in liquid argon calorimetry would be costly, and supplementary funds from the US Department of Energy (DOE) were needed. At the same time, the SLD experiment at SLAC was getting underway and also required a funding infusion. In early 1984, DOE sought the opinion from HEPAP on which experiment to prioritize.

The argument that SLD would be in a race with LEP to do the Z boson physics carried the day, and SLD was put on a faster track. Nonetheless, D0 was seen as an important initiative, and having two complementary Tevatron detectors was desirable. Despite the HEPAP recommendation, DOE found some funds for the D0 R&D and scheduled a late 1984 "Temple" review (a precursor to Lehman reviews and the current Critical Decision reviews conducted by DOE's Office of Project Assessment).

From our modest beginnings we slowly managed to find additional collaborators. The most important politically was the first non-US group, led by Yves Ducros and Armand Zylberstejn of Saclay. In summer 1984, the collaboration gathered in East Lansing, Michigan, for the first of three decades of annual off-site, weeklong workshops to assemble the technical design report (TDR) and cost estimate. The collaboration had grown to almost 100 collaborators from 14 institutions with many now working nearly full-time on D0. The TDR design was simplified from the 1983 version and was quite similar to what was ultimately built (28). The physics goals included the measurement of the W and Z boson production and decay properties, QCD studies, and a variety of searches for new phenomena, including heavy quarks and leptons, supersymmetry, toponium, and the quark–gluon plasma. We were blinded by the expectation that the top quark would be less massive than the W boson and thus observable at the S$p\bar{p}$S, so we said nothing about discovering it. We also failed to identify the program of bottom quark studies that ultimately became a major theme, and we did not mention the search for the Higgs boson at all. The 300-page TDR seemed a mammoth undertaking, but compared with the documentation of LHC proposals, it was laughably brief. In hindsight, it is sobering to see just how quickly the physics landscape changed, but also gratifying that even so, the detector was able to address the modified goals very well.

The November 1984 DOE review went well, and we received provisional approval and some funding for the R&D program. By October 1985, the new D0 collision hall and assembly building construction had begun. In the same month, CDF recorded the first $p\bar{p}$ collisions in the Tevatron, and in 1987 they embarked on their first physics run (strangely called Run 0). The race was on, but the D0 horse was rather late out of the gate, and progress was slow. The Saclay group announced that unless data-taking began by 1988, they would be forced to leave. Although we missed that date substantially, the Saclay group is still an active D0 partner!

For me, the next 8 years were something of a blur as we completed the design, fought for scarce technical resources, performed numerous beam and lab tests, constructed the subdetectors, built the readout and event selection electronics, wrote the data acquisition, control, and event reconstruction software, and finally assembled the detector on rollers in the assembly hall adjacent to the collision hall. In most weeks I commuted from Long Island to Chicago for three intensive days of activities. There were divergences within the collaboration on how to resolve the perennial conundrum that the better can be the enemy of the good (and thus the time required to complete the project), and as a result some groups packed up and left the collaboration. But as we came closer to completion, new groups from the United States joined as well as groups from Russia, Brazil, India, Colombia, Mexico, and South Korea, bringing the collaboration size to about 400. There were many problems to overcome: The uranium liquid argon prototypes showed an annoying tendency to emit noise bursts, getting the uranium oxide off the absorber plates before sealing the calorimeter modules required heroic efforts, shipments of vital components from overseas were stalled, and converging on the then-novel multiple-level trigger systems with software processors was an adventure.

Along the way we did have a lot of fun as the collaboration matured. In the era before video meetings, much of the collaboration came to Fermilab frequently, so people knew each other well. Our location on the far side of the Tevatron from the high-rise gave us a sense of independence. However, as the building phase dragged on, I did worry that we might have forgotten how do physics. So partly to exercise our new software and partly to have some physics exercise, I generated a Monte Carlo data set of dominantly run-of-the-mill QCD events intermixed with a sprinkling of more interesting processes. I invented a scheme for the event numbers in which the pattern of the digits encoded the physics source so that I could monitor how the analyzers were doing. Mostly the correct physics was found, but one sample of a few leptoquarks flummoxed everyone. I had to make their mass rather low to allow their production with the available integrated Monte Carlo luminosity. Due to recent HERA limits on leptoquarks that excluded that mass, people failed to search in this disfavored region. But the McPhysics exercise was a success and did reassure us that when data came, we might be able to digest it.

In February 1992, the completed detector was rolled into the collision hall for final checkout with cosmic rays. On May 12, nearly 9 years after inception, D0's first collisions occurred. Celebrations were clearly called for, but at that moment the DOE tiger teams charged with ferreting out any hint of unsafe operations were on-site, so we had to toast our success in a clandestine postmidnight party in the D0 hall.

In 1989, Fermilab called for proposals for operation with the upgraded Tevatron–Main Injector complex with a significant luminosity increase, and D0 needed to consider its future despite being 3 years away from its first collisions. It became clear to me that trying to bring up the Run I program while undertaking a major upgrade made it necessary to expand our organization. I had become friends with Hugh Montgomery (Mont) during our days in the Village and mounted a campaign to bring him to D0 to help run the upgrade. The Lab had various plans for Mont in leadership capacities, but in the end I prevailed, and he and Tuts agreed to lead the upgrade. We proposed a major overhaul of the original detector, adding a superconducting solenoid surrounding new silicon-strip and scintillating fiber tracking detectors, new calorimeter electronics to accommodate the shorter interbunch interval, and wholly new forward muon detectors (29). The PAC, once so proactive in the D0 adventure, was now reticent about supporting a proposal for an upgrade to an untried detector. They were especially skeptical of a tracking system consisting of only eight measurement stations over a small lever arm. From the 1990 Run II proposal, it took 6 years to get final approval despite the fact that by that time, all of the major subsystems were well into production.

Run I of the Tevatron lasted only from 1992 to 1996 but shone brightly in my view as the culmination of a decade of hard labor. In 1993, an election for spokesperson was initiated, and I felt that with the active physics program and the ongoing upgrade work, the job had grown too large for a single person. Mont and I were elected as co-spokesmen for a 3-year term, and Weerts replaced Mont as upgrade co-manager. Working closely with Mont to lead the collaboration through the intensive period of top quark discovery and initiation of the upgrade was a great pleasure. It was not obvious that two people could mesh to give effective leadership, but it worked, and D0 has retained that model for the remainder of its existence.

I felt the need for some fresh air after over a decade in the trenches building D0, so in 1994 I used a part of my sabbatical leave to work with the OPAL Collaboration at CERN. The e^+e^- environment was novel for me—low background, relatively simple events, no triggers, and full knowledge of the initial state. OPAL, under the leadership of Rolf Heuer, was welcoming, and I did two studies that were important, at least for reassuring myself that I could still be a hands-on physicist. One used a selection of three-jet events in which the gluon jet could be identified to devise an algorithm for distinguishing quarks and gluons. The other used the H-matrix to estimate the momentum of B^0 mesons decaying semileptonically and improve the precision of B-mixing measurements. I also learned valuable lessons on how to organize a mature collider experiment that were subsequently transported to D0. And because there were four LEP collaborations, I could observe the rather pronounced differences in their sociology and scientific style. It struck me that these differences were hardwired at inception and reflected the personalities of the experiments' founders—an observation that also seems valid for other large collaborations.

Barbara had been most patient with my years of commuting to Fermilab, but the sorties each weekend from our CERN base were also a welcome respite.

The first D0 physics publication in 1994, a search for first-generation leptoquarks (30), was quickly followed by the last-ever lower limit on the top quark mass (31) of 131 GeV. That paper, however, highlighted a tantalizing $t\bar{t}$ candidate event containing an electron, muon, and missing E_{T} all above 100 GeV and two jets, with a probability to be due to a background process that was infinitesimal. In spring 1994, CDF unveiled its analysis showing evidence for top quark pair production (32). This stimulated the D0 response (33) at the International Conference on High Energy Physics in Glasgow that showed, based on 15 pb^{-1} of data, a small excess consistent with top quark production but with an expected sensitivity equal to that of CDF. These hints spurred us to initiate a revision of the selection criteria optimized for $m_t \approx 180$ GeV.

By January 1995, additional data suggested that top quark discovery would be possible with the 50 pb^{-1} then in the can, and a comment signaling this expectation was inserted into a D0 talk at the Aspen winter workshop. At this point D0 and CDF went into high gear with frenzied efforts to analyze the full data sets. I had earlier suggested to Fermilab Director John Peoples that he establish a codicil stipulating that in the event of a major discovery paper by one collaboration, the other be given a week to finish its companion analysis before joint submission for publication so as to retain some sanity in the discovery process. This agreement to synchronize the submissions echoed the earlier joint publication of the σ_{tot} results at the ISR. On February 17, CDF delivered its paper to John. The D0 paper draft was already in hand, but we decided to wait a few days for final cross-checks.

During this critical week I was in Rio de Janeiro for a workshop on heavy quark physics where I had organized the top quark session. John Hobbs (later to succeed me as the Stony Brook collider group leader) said that his talk, in which he claimed "nothing new" while he knew of the impending discovery announcement, was the most difficult of his career. I spent the week on the phone to Fermilab finalizing the paper. The joint submissions were duly made on February 24, and the seminars announcing the results were set for March 2. Acceptance by *Physical Review Letters* came in just 12 days (34, 35).

The story of the discovery was captured in an article in the *SLAC Beam Line* (36). I have always relished James Bjorken's guest editorial in that issue commenting on the vigorous competition between CDF and D0: "This piece of competition has been a class act.... In this increasingly fractious world of ours, it should be read and taken to heart by all those who despair of progress being made through reasoning and consensus" (37).

I was privileged to give the public presentation of the D0 discovery, representing over 400 colleagues whose contributions made it possible. I had sometimes wondered about the relationship between the total achievement in large collaborations and the number of collaborators ($A = N^{\alpha}$); in the case of the run-up to the top quark observation, α was very close to 1. The top quark discovery was the high point of the Run I program, but the measurements of the W boson mass (38), high-p_{T} jets (39), b hadron production (40), and limits on supersymmetry (41) and anomalous gauge boson couplings (42) broke new ground as well. It was also notable that the first use at the Tevatron of multivariate analysis techniques involving neural networks, etc., occurred in Run I (43). There was considerable resistance in the community and within the collaboration to the use of such black-box techniques whose internal workings were rather opaque. But I felt that such measures were just as valid for selecting events as the more traditional particle kinematics variables, so long as they were well defined.

Even though it had been foreseen as theoretically necessary, and an approximate mass window had been given by precision electroweak measurements, the discovery of the top quark was a major milestone. Contributions were made by essentially all members of the D0 Collaboration, and I found it impossible to single out a few individuals to whom a prize recognizing the discovery could be given. Indeed, in 2005, several of us in D0 made a nomination for the American Physical Society (APS) Panofsky Prize to be given jointly to the CDF and D0 Collaborations, explicitly recognizing the group achievements. Such awards to collaborations had previously occurred elsewhere. Alas, the APS was unable to approve a prize given to more than three individuals, and the attempt failed. It was therefore of immense gratification to me that in 2019, the European Physical Society gave its prize to the CDF and D0 Collaborations "for the discovery of the top quark and the detailed measurement of its properties." This award implicitly honored all D0 collaborators, not only at discovery time but also afterward, without distinction for contributions to the detector, software, or analyses. Accepting that prize on behalf of hundreds of colleagues was an honor.

When my term as co-spokesman came to an end in 1997, I decided that it was time to move on. Perhaps I had stayed on longer than I should have, but I had wanted to see the physics payoff from the long years of assembling a collaboration and building the experiment. With Run I completed and the Run II upgrade fully in progress, it was a good time to step down. Mont continued, now with Weerts as co-spokesman. In retirement I led the effort on the hardware track and preshower detector trigger for Run II. In 2000 and 2012 I served as chair of the institutional board that handled issues such as governance and computing policy as well as new additions to the collaboration. I chaired the internal "editorial board" reviews for publication and conference presentations of Higgs boson searches, top quark properties, and dijet resonance searches.

I was happy to once again have the opportunity to do physics. My students, postdocs, and I were able to perform the final Run I W boson mass measurement (44), a Run II search for a third-generation leptoquark decaying to τb (45), and measurement of the $Z + b$ jets cross sections (46) that are backgrounds for Higgs searches. We also made a search for Higgs bosons in tau final states in a 2013 analysis (47) that was the most complex I had ever undertaken. We considered 27 distinct production and τ decay channels and τ parent (Higgs or W/Z boson). The search used separate multivariate classifiers for the H and W/Z parentage and combined them in one final analysis to reach a 95% confidence limit of 11.3 times the Standard Model expectation at $M_H =$ 125 GeV—the best at the Tevatron, but of course quickly superseded at the LHC.

I was honored by my selection as the 2000 Panofsky Prize winner, with the following citation: "For his distinguished leadership and vision in the conception, design, construction, and execution of the D0 experiment at the Fermilab Tevatron proton-antiproton collider. His many contributions have been decisive in all aspects of the experiment." Although the discovery of the top quark was chief among the D0 accomplishments in Run I, the prize committee carefully avoided its mention, again in recognition that no one person had played a singular role and that the CDF and D0 Collaborations shared the credit equally. In my acceptance talk at the 2001 APS meeting, I chose instead to focus on the nature of large scientific collaborations and whether they were effective scientifically, financially, and educationally. In related articles, I attempted to articulate the case for why such big science projects were worthy of societal support (48) and to establish ground rules for promoting individuals who worked in large collaborations (49). I returned to this theme later with somewhat more careful thought when I gave the 2012 W.V. Houston Memorial Lecture at Rice University (50).

Looking back on my time as D0 spokesperson, I find it odd to realize that I was relatively autocratic about it despite my personality as a rather uncontentious individual, usually undogmatic about the rightness of my opinions. Although I tried to understand the basis of everyone's argument in making difficult decisions, I made those decisions myself rather than through a vote by an advisory board. And perhaps my softspoken way served me well, since on the occasions that I raised my voice and shouted at someone whose point of view seemed a bit outrageous, it came as enough of a shock that they backed off.

8. EXCURSION INTO MANAGEMENT

Another reason to step down as D0 co-spokesman was that in 1995 I had started my term on the Executive Committee of the APS Division of Particles and Fields (DPF), and in 1997 I served as chair. It was stimulating to broaden my view of particle physics after years of being buried in a single experiment. I was able to revitalize the spring APS meetings by adding general interest talks each morning covering recent advances across all branches of physics. To commemorate the 1999 APS centennial, I joined with Mary K. Gaillard and my DPF predecessor Frank Sciulli in writing a review article on the Standard Model of particle physics (51), and I was happy to follow up with several colloquia describing the state of particle physics. Following the debacle of the cancellation of funding for the Superconducting Supercollider, high-energy physics was in poor odor in Congress, so the attempt to bring the United States into the LHC program was a hard sell. As DPF chair, helping to reach an agreement to fund US participation in the ATLAS and CMS experiments was my most difficult, but ultimately most rewarding, contribution.

In my DPF role I also felt the need to understand the growing interest in new e^+e^- colliders: the Next Linear Collider being proposed by SLAC and the TESLA project at DESY. As a long-time hadron collider person, I had misgivings about an e^+e^- machine that operated below 1 TeV being sufficient to carry the field after the expected discoveries at the LHC. I worried that an e^+e^- collider, with its low cross sections, the need to operate at multiple energies, and the complexity of the new physics panorama expected at the TeV scale, would be unequal to the task. Nevertheless, it seemed clear that the case needed careful thought, and in 1997 I agreed to co-lead a US effort with Charles Baltay to evaluate linear colliders, their detectors, and their physics potential, and set out to make my own evaluation of hadron and lepton collider capabilities. In 1999, Fermilab Director Mike Witherell asked Chris Quigg and me to organize a series of discussions that I dubbed "Circle Line Tours" (52) to evaluate the case for a variety of hadron and lepton colliders. Through these discussions I became convinced that the old pattern of discovery at hadron machines followed by detailed investigations at lepton colliders was the best way forward, and I slowly evolved from being a skeptic to a supporter of an e^+e^- machine as the next step. My advocacy, however, elicited some rather pointed accusations of betrayal from some senior hadron collider physicists at Fermilab.

In 2000, the Department of Physics and Astronomy at Stony Brook started its search for a new chair. For some time I had evaded this job due to my D0 responsibilities, but now the writing was on the wall. I had secured a Guggenheim Fellowship for the calendar year 2001, so my predecessor Janos Kirz graciously agreed to stay on until I returned from Imperial College London at the beginning of 2002. After leading D0 for years I was not particularly daunted by the prospect of chairing a department of about 60 faculty, but I quickly learned that there are differences. Although the people in a particle physics collaboration do not have a line management structure with a CEO who controls salaries, hiring, and promotions, there is a commonality of purpose that tends to make collaborations operate smoothly. By contrast, a university department (and even more so the university itself) has a range of individuals with quite diverse interests and working relationships, so steering the department requires a great deal more effort to move the rudder than in an experimental collaboration. And the battles for support, the bureaucracy in the State University of New York system, and dealing with the few squeaky wheels in the department could be time-consuming. However, I was gratified by the opportunity to shape the future of the department through the hires of several supremely talented young physicists who later blossomed into leaders in their fields. Another success was to convince the university administration that the department should expand to include my old love, cosmology. Although Frank Yang had maintained a valuable Stony Brook link to accelerator physics through adjunct appointments in the Institute for Theoretical Physics for people such as Ernest Courant and Claudio Pellegrini, I was proud to get the university to further capitalize on our proximity to a major national laboratory and support regular faculty appointments in accelerator science. One somewhat questionable aspect of my chairmanship was the exhortation to the faculty to retire at age 68 so as to make way for vigorous new blood in the department. It was no doubt inappropriate to suggest this, but several faculty did move to focus solely on research, and I also chose to become a research professor in late 2006, shortly after my 68th birthday. I was able, however, to retain my National Science Foundation grant and continue supervising students, but now without the introductory physics lectures that had until recently been my lot.

9. A LINEAR COLLIDER

My conversion to the case for an e^+e^- collider was accelerated by an invitation in 2002 to join the International Linear Collider Steering Committee (ILCSC) from Maury Tigner, who had first proposed a linear collider over four decades earlier. The ILCSC was a subcommittee of the International Committee for Future Accelerators (ICFA). At the time there were several flavors of a linear collider in contention—versions using room-temperature copper RF accelerating structures driven by klystrons or a low-energy, high-current drive beam, and an alternative with niobium superconducting RF cavities. The ILCSC was trying to rationalize this complex picture and move toward a more unified international plan.

In 2003, I joined an ILCSC subpanel chaired by Heuer to specify the parameters (the energy range, luminosity, beam polarization, number of interaction regions, and operational benchmarks) for the baseline machine as well as a desired energy upgrade and options beyond the baseline. This panel reconvened in 2006 to validate the use of just one interaction region with a push–pull operation of two detectors. These specifications have remained the defining standards for linear colliders.

In 2004, ICFA sought to define a specific path for a linear collider project by creating the International Technology Recommendation Panel (ITRP) to recommend the technology choice. The ITRP was composed of four physicists each from the Americas, Asia, and Europe with Barry Barish as chair. For me, the panel was an opportunity to learn about accelerator physics at a deeper level, and I formulated the 34 detailed questions that became the basis of the ITRP deliberations. The main contenders were the X-band room-temperature machine and the superconducting RF version. In September 2004, the ITRP voted for the superconducting version, which was duly endorsed by ICFA (53). Among the arguments in its favor was the stimulating effect of superconducting RF upon a broad range of science and technology, although it was clear that much work remained to achieve the accelerating gradients needed.

ICFA moved rapidly to establish the Global Design Effort (GDE) charged with developing the plan for the International Linear Collider (ILC) and asked me to lead the search for the director. Barish was chosen to lead the GDE with the charge to make a detailed and coherent design of all aspects of the ILC machine, its civil infrastructure requirements, and a cost estimate, and to conduct the necessary R&D to support that design. The ability of the international community to reach a consensus on a globally supported future project was hailed as a major success—as Jonathan Dorfan, then ICFA chair, noted, "Never before has a field of science attempted to globalize itself as extensively as HEP is doing"—but in time, this unanimity unraveled. Unlike the ITER fusion energy project, which was initiated by heads of state, the ILC was a bottoms-up effort driven by the community of scientists, making an endorsement by governments more difficult.

In 2005, as my term as department chair was coming to an end, Robin Staffin, associate director for high-energy physics at DOE, phoned to ask me to oversee the planning process for the ILC, which was then seen as an attractive prospect for a future facility in the United States. The director of the Office of Science, Ray Orbach, felt that he had indications of congressional support for the project at a certain cost level. During the time I spent at DOE, we were able to provide substantial support for the GDE's detailed design effort as well as R&D on superconducting accelerating cavities, high-power RF systems, and the damping rings needed to reduce the phase space of the beams before their acceleration. The approximately $30 million per year that DOE provided was a critical contribution to the completion of the reference design report (RDR) delivered by the GDE in 2007 (54). The RDR provided a coherent design for the ILC, laid out the R&D program needed to achieve it, and described an organizational blueprint for a new global laboratory. It also provided a cost estimate in international currency units and an estimate of the manpower needed. Alas, the cost was more than twice the amount that Orbach had in mind, and the prospects for an ILC in the United States dimmed accordingly.

My job at DOE faced in two directions: advocating and educating within the bureaucracy and interfacing with the people involved in the ILC project, particularly the leaders of the effort in the Americas—first Gerry Dugan and then Mike Harrison. The mode of working at DOE was vastly different from what I was used to in my research; there were often requests like "Specify the annual costs of the ILC for the next 20 years," delivered in the morning with a response deadline of the close of business that day. One had to learn to make one's best estimate without the detailed analysis typical of a scientific project. Over the course of my 2 years at DOE, I gained great respect for the permanent staff, who have to function efficiently in the hierarchical world of the federal government and frequently have to deliver unwelcome news on available funds or bureaucratic hurdles to the physics community. Despite this, their desire to enable the best science and their deep satisfaction in the field's achievements were evident.

Following my return to Stony Brook in 2007, now as a research professor, I continued my ILC involvement with the International Detector Advisory Group charged to evaluate proposals for ILC detectors. In 2009, two concepts, ILD and SiD, were chosen for further development and to serve as templates for the design of the collider intersection region. I also became the chair of the committee advocating for the ILC within the Americas. With the US retreat from aspirations to host the ILC, the physics community in Japan mounted a campaign to bring it there. The United States still maintained some interest, but the level of support dropped significantly. Nevertheless, the GDE worked to evolve the 2007 RDR and deliver the 2013 TDR (55). By then, the effort on superconducting RF cavities had shown that the desired gradients were achievable, and the development of superconducting technology spurred by the ILC had opened new possibilities across many fields of science. In Japan, members of the National Diet had formed an organization devoted to establishing the ILC in Japan, and many prominent Japanese industrial companies joined in support. However, Japan's Ministry of Education, Culture, Sports, Science and Technology (MEXT), the government agency responsible for scientific research, realized that the ILC would be more costly than its own budget could provide and subsequently introduced a series of steps to review and evaluate the project, thus delaying a decision.

To make the ILC more fiscally palatable, the successor to the GDE—the Linear Collider Committee chaired by Lyn Evans (who had led the LHC project at CERN)—reduced the scope so that the initial phase of the ILC would be at half the energy of the originally proposed 500 GeV. This helped somewhat, but as I write this in 2023, MEXT has still not committed to proceed with the project. MEXT sees the ILC as a global project in which international partners would agree ab initio to jointly design and fund the facility before Japan would announce its proposal to host, thus creating a chicken-and-egg problem. Meanwhile, the European community has proposed the

Future Circular Collider (FCC) as the next major initiative at CERN; in its first phase, it would collide electrons and positrons at energies up to 350 GeV to be followed by pp collisions at the 100-TeV scale. The FCC is now the subject of intensive study in Europe and has strong support from the CERN administration. The Chinese community has proposed a similar circular facility, and the CERN effort on a linear collider powered by drive beams continues. A new proposal for a klystron-driven linear collider based on copper cavities operated at liquid nitrogen temperatures has emerged from SLAC. Thus, despite broad agreement that the next step for the field is an e^+e^- Higgs factory, the path forward has become murky.

Despite the stronger expectations of political support for the circular e^+e^- machines, I see scientific advantages for the linear colliders. Chief among them is the possibility for upgrade to higher energy by replacing the accelerating elements with higher-gradient cavities or by extending the linac lengths. This higher energy would have benefits for understanding the couplings of the Higgs boson and would offer opportunities for more incisive searches for physics beyond the Standard Model. The linear collider costs are also less than those of the 100-km-circumference circular collider tunnels. It is clear, however, that the saga of the e^+e^- collider has not yet played out. I hope for a resolution of this puzzle in my lifetime!

10. D0 REDUX

The Tevatron ceased operations in 2011 when the LHC took over at the energy frontier. Despite the movement of many collaborators to the ATLAS and CMS experiments, a strong D0 effort to conclude ongoing analyses persisted. Among these was the search for the Higgs boson through its production in association with a W or Z boson and subsequent decay to $b\bar{b}$. Neither CDF nor D0 could reach the level of evidence for the Higgs boson by themselves, but a combination of the two analyses barely reached 3σ significance (56). This combination was completed in summer 2012 just after the ATLAS and CMS discovery (57) of the Higgs boson through its subdominant decays to $ZZ^* \rightarrow 4\ell$ and $\gamma\gamma$, but it was important to see that the dominant $b\bar{b}$ decay also existed. At the time, D0 was at its annual workshop, this time in Lancaster, England. I was chair of the internal review and we worked feverishly with the D0 analyzers and spokespersons to finalize the result. Since our CDF counterparts were 6 hours later than us, we sacrificed not only the workshop excursions in the lovely Lake District but also much of the nighttime.

In 2014, Gregorio Bernardi stepped down as co-spokesman of D0, and the search for a replacement was not turning up a sensible candidate. So I was again drafted and have served in that capacity with Dmitri Denisov ever since. Leading D0 through its endgame as Fermilab withdrew its support has been tricky, and Dmitri's steady hand has been essential. The number of publications diminished from 22 in 2014 to 1 or 2 in the last couple of years. But there have been some notable results during this time—legacy measurements of top quark properties and Higgs boson searches, final W boson mass measurements, cross sections for production of W and Z bosons with b or c quarks, and a series of studies of heavy quark mesons, including exotic four-quark structures. As time went on and the LHC results on the Higgs boson and searches for new phenomena multiplied, the Tevatron focus shifted toward measurements that were hard at the LHC due to pileup or the difficulty of triggering on low-p_T particles, and toward combinations of results from different experiments.

Two late results stand out for me. The first was a combination of CDF and D0 measurements of the forward–backward asymmetry in dilepton production in the vicinity of the Z pole, from which the electroweak mixing parameter $\sin^2\theta_{\text{eff}}^{\text{lept}}$ could be extracted (58). In these measurements, the unique ability of D0 to reverse the polarity of its solenoid and muon toroid magnets enabled the control of false asymmetries and thus improved precision, as it had done previously in the measurements of CP violation in the neutral B mesons (59). The combined result shed light on the puzzle of the discrepant results of the SLD experiment at SLAC and the CERN LEP experiments (60). The Tevatron combination, dominated by the D0 measurement, neatly falls midway between the LEP and SLD values with comparable uncertainty.

The second example was the 2021 combination of the D0 $p\bar{p}$ differential elastic cross section measurement (61) at 1.96 TeV and a series of measurements of the pp differential cross sections by the LHC TOTEM Collaboration between 2.76 and 13 TeV (62). The shapes of the $p\bar{p}$ cross section and the pp values extrapolated to the D0 energy differ significantly in the region of the first diffractive minimum. At the multi-TeV energy scale, such a difference implies the existence of the odderon Regge pole, the C-parity-odd counterpart to the well-known C-even pomeron. The D0-TOTEM combination (63), taken together with TOTEM measurements of the pp total cross section and the real-to-imaginary forward amplitude ratio (64), permits the claim of discovery of the odderon. This makes a pleasing bookend to my career, as the C-odd Regge poles had been postulated as the explanation for the behavior of the polarization parameter in pp scattering in the few-GeV region in my PhD thesis 57 years earlier. And this was the first-ever combination of Tevatron and LHC results.

Compared with my first stint as D0 spokesperson, the second has not had the thrill of a major discovery or the deep satisfaction in building a major new experiment. Nevertheless, being able to guide the final publications and to help close out the very successful program has had its own rewards. The D0 legacy stands today at over 500 publications (see **https://d0.fnal.gov/d0publications/**). However, the lights have not yet been totally extinguished, and I still look forward to the final chapter.

11. CODA

My career has spanned the time from small experiments with a half-dozen people and quite specific goals to ever-larger collider experiments involving hundreds or thousands of physicists pursuing multiple topics. When asked how this has played out, I tend to respond with a variation of *plus ça change, plus c'est la même chose*. On the one hand, the day-to-day experience has not changed so much: Groups of a few people work on specific analyses or instrumentation projects much as they did in the past, so the feel of research life at the working level is not so much changed. However, with the advent of the large collaborations, there is a new aspect of "organization" that compels ever-larger fractions of one's attention. There are numerous internal and external reviews, a hierarchical structure of groups and subgroups, and specializations in physics analyses, operations, computing, and management. Few individuals have a full overview of the activities of a collaboration. I mentioned earlier the relation between achievement and number of collaborators, $A = N^{\alpha}$. There is a price to be paid for keeping a large, complex experiment working smoothly, and the exponent α is certainly less than one.

The large collider experiments cast much wider nets and harvest measurements from a broader range of physics topics than their more focused predecessors did. Their ability to adapt to new physics ideas is quite remarkable. And unlike the earlier era of experiments involving just a few physicists and a couple of students, the amount of potential science left on the table is quite low. Early in my career, most experiments achieved the main goal announced at the beginning, but as the students and postdocs left the scene, the ancillary analyses that might have been done were lost.

Modern collider experiments are very expensive. But the cost per person per year is probably less than it was 50 years ago, and not so different from that in other fields of physics. And the complexity of modern experiments has enabled a far more incisive understanding of the physical world than would have been possible with the smaller, narrow-focus experiments of the past.

As I noted above, the large collaborations acquire their own distinctive personalities. They also become clans with strong ties among their members despite the fact that there is no line authority

Figure 2

The D0 Collaboration in the assembly hall. Photograph by Reidar Hahn, Fermilab; used with permission of Fermilab Creative Services.

as in corporations or universities. But often these ties are stronger within collaborations than they are in the institutes employing individuals. Old collaborations never die; they just fade away. As the generations of experiments march on, one can still discern the tendrils of previous collaborations flowing within the larger fabric of the newer projects.

My life in this evolving milieu (see **https://sbhep.physics.sunysb.edu/~grannis/home.html**) has provided me with many challenges, gratifying scientific successes, and wonderful interactions with hundreds of individuals (**Figure 2**). It has certainly not been monotonous. I have been able to play at being an electronics tech, plumber, and computer programmer as well as psychological counselor, organizational manager, shaper of future facilities, and government advisor. I may not have filled all of these roles well, but chasing those various fleas (see **Figure 1**) has kept things lively.

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