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ANNUAL REVIEWS

Annual Review of Nuclear and Particle Science

Adventures with Particles

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Annu. Rev. Nucl. Part. Sci. 2021. 71:1–21

The *Annual Review of Nuclear and Particle Science* is online at nucl.annualreviews.org

https://doi.org/10.1146/annurev-nucl-111119-053716

Keywords

Standard Model, particle physics, autobiography

Abstract

Despite some gender-related bumps in the road, the author had the good fortune that her career spanned the evolution of the Standard Model from its inception in the late 1960s and early 1970s to its final confirmation with the discovery of the Higgs boson in 2012. Her major contributions to these developments and other facets of her career are described.

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1. STARTING OUT

My first encounter with physics was the course I took as a senior in high school. I immediately decided to major in physics in college—probably because it was the most mathematical of the sciences, while providing, as I saw it, more relevance for the real world than pure mathematics. Beyond that, I didn't think much about what I would do after college. This was 1956, a time when women weren't expected to do anything, although my mother, who had been a teacher of English, speech, and drama, and a Planned Parenthood and high school counselor, was probably more of a role model than I appreciated at the time.

My father was a history professor at Lake Erie College, a small liberal arts school, and we didn't have much money, so I went to the college that offered me the biggest scholarship. That was a women's college (now Hollins University) in Virginia, with about one physics major every other year, and two physics professors. However, the department chair, Dorothy Montgomery, had been active in research on soft money at Yale. She was let go when her husband died, and she moved to Virginia to bring up her two small children. She turned out to have an enormous influence on my subsequent career path. When I spent a year in Paris, she got me into the Louis Leprince-Ringuet laboratory, a group that had done important cosmic-ray physics in the 1950s. More importantly, she got me to apply to the summer student program at Brookhaven National Laboratory. There I worked with a group of physicists from Columbia University. It was this experience that got me hooked on particle physics. I remember the buzz of excitement when a scanner came out with an unusual event. It was the first observation of the decay $\Lambda \to p\mu\bar{\nu}$. Noticing my puzzlement, Leon Lederman said, "It's because you're not educated to appreciate it." That was not a put-down, but an incentive to learn more about the field.

My official supervisor at Brookhaven was Bob Adair from Yale, who gave lectures to another student and myself on the rudiments of particle physics, as understood at the time, which are summarized in **Table 1**.

After my senior year and my second stint at Brookhaven, I started my graduate studies at Columbia. A (then prestigious but soon extinct) Woodrow Wilson Scholarship financed my first

Table 1 Elementary particle physics in 1959

Force	Range	Strength	Particles
Strong	10^{-13} cm	1	$p, n, \Lambda, \pi, K \ldots$
Electromagnetic	Infinite	10^{-3}	Above + e, μ
Weak	10^{-16} cm	10^{-10}	All above + ν
Gravitational	Infinite	10^{-38}	All

year at Columbia. I got the paperwork to apply for a National Science Foundation (NSF) fellowship. This was in 1961. Joseph McCarthy had met his downfall, but the House Un-American Activities Committee was still influential, and the application required a loyalty oath. I was in a quandary about what to do, when the issue became moot because that summer I married Jean-Marc Gaillard, who had been a postdoc with the Columbia group, and I followed him back to France.

In Paris I was advised to join an experimental laboratory (as most graduate students there did) because virtually no one was admitted to the theory group where the graduate courses were taught. After being turned down by all the labs on the grounds, at times tinged with misogynist implications, that they accepted only students from two elite Parisian schools (École Normale and École Polytechnique, both all male at the time), I continued the coursework, did well on the exams, and was accepted into the theory group after all. But then I had to follow Jean-Marc, newborn in tow, to Geneva, where he had been offered a 6-year junior staff position at CERN (originally *Conseil Européen pour la Recherche Nucléaire*). There I was assigned to a basement office that I shared periodically with as many as three other students, as well as two distinguished Swiss physicists, J.M. Jauch and E.C.G. Stückelberg, and Stückelberg's dog. At CERN I completed my doctoral thesis and was given a research position in the French National Centre for Scientific Research (CNRS). As it turned out, we spent almost 20 years at CERN as visiting scientists. Over the years I rose up the ranks of the CNRS to the highest position of Director of Research. Concurrently, I moved up the floors of CERN, first to offices with just one other theorist, and finally to an office by myself on the fourth floor along with the theory group senior staff. However, I was never considered for a position of any level at CERN, except for an unsuccessful effort by two of my CERN collaborators after I had received offers from Fermilab (Fermi National Accelerator Laboratory) and University of California, Berkeley.

2. WEAK DECAYS OF STRANGE PARTICLES

As it happened, Bernard d'Espagnat, a professor from the Paris theory group, was visiting CERN the year I arrived, and he agreed to take me on as his doctoral student. We coauthored a few papers, but then his primary interest turned to the foundations of quantum mechanics, so most of my early papers were single-authored. One of these dealt with the decay $K \to \pi\ell\bar{\nu}_\ell$ (1). The experimental measurements, in two independent experiments, were in strong disagreement with the prevailing theory. By this time, Murray Gell-Mann (2) and George Zweig (3) had independently postulated that protons and neutrons, as well as other strongly interacting particles, were composed of more elementary constituents dubbed quarks by Gell-Mann. It was believed that up and down quarks, the constituents of the nucleons, were very light, and that the strong interactions were approximately invariant under chiral transformations on the quarks—that is, isospin rotations with a helicity flip. This postulate enabled the prediction of amplitudes for the emission of soft pions. However, there was one measurement in serious disagreement with the theory. The final-state configurations in $K \to \pi\ell\bar{\nu}_\ell$ were determined by two independent functions, one of which was predicted to be very small, in stark contradiction with experiment. I proposed (1), as did Richard Brandt and Giuliano Preparata (4), an alternative theory that could account for the observation. However, I subsequently realized that the two independent functions, somewhat arbitrarily chosen, that were used to analyze the data were correlated. The experimentalist Louis-Michel Chounet and I wrote a paper (5) promoting the use of uncorrelated functions associated with different angular momentum states of the final-state lepton pair.

Jean-Marc and I were asked to chair parallel sessions at the 1972 International Conference on High-Energy Physics (ICHEP) at the recently established (and still under construction) national laboratory (now Fermilab) in Batavia, Illinois. I presented our results in my session and Stan Wojcicki presented the results (6) of his group, which, analyzed in terms of these functions, gave beautiful agreement with the theory. The verification of this and other predictions of approximate chiral symmetry confirmed that, up to quark mass effects, interactions among quarks left their helicity unchanged, which in turn implied that these interactions were mediated by spin 1 particle exchange, like quantum electrodynamics (QED). However, in contrast to QED, the strong coupling among quarks is asymptotically free, which means that the coupling strength decreases with increasing energy and decreasing distance, a feature demonstrated first in electron–proton scattering experiments (7) at SLAC (originally Stanford Linear Accelerator Center), and then in neutrino–nucleon scattering (8) in the Gargamelle bubble chamber at CERN.

The following year, Leon Lederman invited us to spend the 1973–1974 academic year at Fermilab, where I worked with Ben Lee, the group leader and sole permanent member of the theory group. By this time, elementary particle physics was rapidly evolving. Gauge theories—akin to QED but much more complicated—had been proposed for both the strong and weak interactions. In particular, David Gross and Frank Wilczek (9) and, independently, David Politzer (10) had shown that non-Abelian gauge theories—those with mediators that interact with one another as well as with matter fermions—are indeed asymptotically free. Compatibility of the baryon spectrum with Fermi statistics had led to the conjecture (11) that in addition to the "flavor" quantum number, which distinguishes an up quark from a down quark, for example, there is also a (conserved) "color" quantum number: An up quark can take any of three different colors. A year before the results on asymptotic freedom, Harald Fritzsch, Murray Gell-Mann, and Heinrich Leutwyler (12) had suggested that the color quantum number is the charge of a new non-Abelian gauge theory, now known as quantum chromodynamics (QCD), with the mediators called gluons (the glue that binds the quarks inside nucleons).

Ben and I decided to apply this theory to address a long-standing puzzle. According to Cabibbo theory (13), nonleptonic weak decay should change isospin I by 1/2 or 3/2 of a unit, with roughly equal probability. Instead the $\Delta I = \frac{1}{2}$ transition was found to dominate by a factor of about 20 or more in amplitude. We used the renormalization group equations (RGEs) to study the effects of gluon exchange among quarks and indeed found (14) an enhancement of $\Delta I = \frac{1}{2}$ amplitudes and a slight suppression of $\Delta I = \frac{3}{2}$ amplitudes, a result found independently by Guido Altarelli and Luciano Maiani (15). The effect was too small to fully account for the observed ratio, which involves several additional effects. However, the method we developed—writing the decay amplitudes in terms of operators with coefficients computed using the RGE—provided the foundation for the modern theory of heavy quark interactions at high energy scales.

3. CHARM

The term charm was coined in a 1964 paper by James (Bj) Bjorken and Shelly Glashow (16), who, among others, were looking for alternatives to fractionally charged constituents of hadrons (strongly interacting particles). They introduced a fourth constituent, which they called charm, in addition to the three familiar constituents, now known as up, down, and strange. Meanwhile, theorists were puzzling over the observed strong suppression of "strangeness changing neutral current" processes, such as the decay $K^0 \to \mu^+\mu^-$. Specifically, Mohapatra et al. (17) estimated that the second-order weak contribution to the (quadratically divergent) amplitude for the decay $K \to \mu^+\mu^-$ in Fermi theory was proportional to $G_F^2\Lambda^2$, where G_F was the Fermi coupling and Λ an ultraviolet cutoff. Agreement with observation required a cutoff of a few GeV (gigaelectronvolts). Then, in 1970, Shelly Glashow, Jean Iliopoulos, and Luciano Maiani (GIM) pointed out that the up quark loop contribution to the amplitude could be canceled up to mass effects by the contribution of a new quark with the same electroweak couplings as the up quark: the charm quark (18). In this case, the charm quark mass provided the effective cutoff and consequently was predicted to be no more than a few GeV. GIM also included the charged intermediate vector bosons $W^{\pm}$, which softened the divergences of the Fermi theory. When they extended this set to include a neutral boson W^0 in such a way that the weak currents formed a Yang–Mills algebra, the neutral current conserved flavor to very high accuracy, in total disagreement with the case without charm. This picture gained credibility in 1971 when Gerhard 't Hooft (19) provided the first proof that theories with an exact or spontaneously broken non-Abelian gauge symmetry are renormalizable, meaning that the divergences of these theories can be absorbed into the definition of a limited number of measured parameters. This led to renewed interest in earlier proposals by Glashow (20), Steve Weinberg (21), and Abdus Salam (22) for a gauged version (GWS) of weak interactions, including neutral couplings. In 1972, Claude Bouchiat, Jean Iliopoulos, and Philippe Meyer (23) and, independently, David Gross and Roman Jackiw (24) (all five authors known collectively as BIM-GJ) pointed out that the renormalizability of the GWS theory would be spoiled by the presence of "triangle anomalies" (25) without adding a fourth quark charm.

When I arrived at Fermilab in the fall of 1973, the status of these new ideas was uncertain. Some experiments found evidence of the predicted neutral currents; others did not. When Ben and I were discussing the situation, I asked, "If the GIM mechanism is correct, why isn't the decay $K \to 2\gamma$ suppressed?" In contrast to $K \to 2\mu$, for example, the two-photon decay proceeds at the normal weak interaction rate, whereas, like other strangeness-changing neutral current processes, it would be forbidden by the GIM mechanism in the limit of equal up and charm quark masses. We worried about this overnight, until Ben remembered, based on work (26) he had done earlier with Joel Primack and Sam Treiman, that the suppression mechanism works differently in the two-photon case. We then decided to undertake a systematic analysis (27) of three neutral kaon processes in the GWS-GIM model: $K \to 2\mu, K \to 2\gamma$, and the K_L–K_S mass difference. The $K \to 2\mu$ decay rate gives only a weak upper limit: $\Delta m = m_c - m_u < 9$ GeV. The $K \to 2\gamma$ decay rate imposes the mass hierarchy $m_u \ll m_c$. Finally, the fit to the kaon mass difference has two solutions:

$$m_{u,c} > \Delta m \approx 1 \text{ GeV} \qquad \text{or} \qquad m_u \ll m_c \approx 1.5 \text{ GeV}.$$

Combining the last two conditions gave 1.5 GeV as the prediction for the charmed quark mass (and implied a very small up quark mass, consistent with the approximate chiral symmetry discussed above). After this work was completed, we learned that the previous year Vainshtein & Khriplovich (28) had analyzed $K \to 2\mu$ and the kaon mass difference and had concluded that $\Delta m \sim 1$ GeV, and about the same time that our paper appeared there was an analysis by Ernest Ma (29) of the decays $K \to 2\gamma$ and $K \to 2\mu$, with the conclusion that $m_c \approx 5$ GeV. The reason for this overestimate was that Ma apparently did not appreciate the fact that the two-photon mode provides only an upper limit on the charm–up mass difference; the observed rate is easily understood as a combination of first-order weak and electromagnetic effects.

When this analysis was complete, Ben said, "Now let's solve *CP* violation and the $\Delta I = \frac{1}{2}$ rule." Well, we never got around to *CP* (the combined operations of charge conjugation—turning a particle into its antiparticle—and parity, i.e., space inversion, or, in two dimensions, mirror reflection), but we did make a contribution to the latter issue, as discussed above.

We then joined forces with Jon Rosner, who had been studying the strong couplings of charmed particles, and wrote a long article (30) on charm production and decay. At that time our understanding of hadron interactions was largely empirical, as opposed to the very well-developed and tested theory of electromagnetism, and, to a lesser extent, weak couplings. For example, we used the parton model (31) to estimate charm production in neutrino–nucleon interactions, and the apparent violation of selection rules, like the "$\Delta S = -\Delta Q$ rule," relating the changes in strangeness S and electric charge Q of the hadrons in semileptonic strange particle decay, as a signature for charm production by neutrinos. This process, forbidden in standard current–current interactions, can be simulated by the two-step process of charm production and subsequent decay. We used mass sum rules based on approximate flavor symmetry to predict charmed particle masses, and the "Zweig rule" (32) to estimate a width of about 2 MeV (megaelectronvolts) for the lightest triplet $\bar{c}c$ bound state.

We also pointed out some hints for charm that already existed in the literature:

- In 1970, a group at Brookhaven led by Leon Lederman observed a bump near 3.5 GeV in the invariant mass distribution of $\mu^+\mu^-$ pairs (33). This could be interpreted as a $\bar{c}c$ triplet bound state. (However, the abstract reported "no resonant structure" (33, p. 1523); when Leon saw our paper, he called Ben and told him we were very gullible. After the discovery of the J/Ψ particle a few months later, Leon seemed rather overly inclined to believe everything I said.)
- An emulsion event that could be interpreted as the decay to $\pi^+\pi^0$ of the lightest charmed pseudoscalar meson, $D^+ = \bar{d}c$, was reported in 1971 (34).
- In 1974, Carlo Rubbia (35) reported the observation by his group at CERN of two dimuon events in neutrino–nucleon collisions; these could be interpreted as the production and subsequent decay of a charmed particle.
- The observed unexpected rise in the cross section at the Cambridge Electron Accelerator (CEA) in 1973 and at SLAC in 1974 could be attributed to the production of charmed particles and their antiparticles.

Our paper (30) was written when QCD was in its infancy, and for the most part we ignored it. As QCD began to be taken more seriously, some of our predictions were improved accordingly. For example, in March 1975, Álvaro de Rújula, Howard Georgi, and Shelly Glashow (36) replaced our sum rules for charmed baryon masses with an analysis of quarks in a Coulomb-like chromomagnetic (the QCD analog of electromagnetic) field. Soon thereafter, a group at Brookhaven (37) observed a neutrino event with $\Delta S = -\Delta Q$, in which the final-state hadrons had an invariant mass compatible with the masses of charmed baryons predicted by de Rújula et al. (36).

In 1974 Tom Appelquist and David Politzer (38) replaced our Zweig rule extrapolation of the $\bar{s}s$ triplet width to predict the $\bar{c}c$ triplet width using a more quantitative argument based on charmonium (in analogy to positronium), reducing our earlier prediction of 2 MeV to about 0.6 MeV. This was closer to the value of about 0.1 MeV of the J/Ψ discovered at Brookhaven (39) and SLAC (40) a month before, just 3 months after our preprint appeared. This event became known as the "November revolution."

However, this discovery was far from universally accepted as a $\bar{c}c$ bound state. The most popular competitor was "naked color." Suppose we call the three color degrees of freedom blue, green, and red. The observed particles are all colorless. The mesons are linear combinations (red, antired) + (green, antigreen) + (blue, antiblue) of $q\bar{q}$ pairs, and each baryon is "white," containing one red, one green, and one blue quark. It was speculated (41) that the new state might be colored—for example, a (red, antigreen) quark pair. John Ellis (at the time a junior staff member at CERN) and I were asked to defend the charm hypothesis in a debate with Paul Matthews, a color proponent, and Alexander Dolgov (42), who advocated for a new gauge boson. What I remember most about the debate is Matthews repeatedly referring to the paper I had written with Ben and Jon as "Lee, Rosner, and Gaillard," not in the normal alphabetical order used by particle theorists.

The case for charm was finally established with the discovery (43) in June 1976 of the D meson, a singlet $c\bar{u}$ or $c\bar{d}$ bound state. At the time, the wait for "naked charm" seemed to us like an eternity, but in fact it was a bit less than 2 years after the Gaillard–Lee–Rosner (30) preprint appeared. In contrast, it was about 40 years before our prediction (27) (in the meantime corrected by others after the discovery of the bottom b and top t quarks) for the decay rate for $K \to \pi\nu\bar{\nu}$ was confirmed by experiment (44).

After my return to CERN, I gave many talks on charm around Europe. These included a talk at the annual neutrino conference held in 1977 in the Russian Baksan Valley. Ben had also been planning to go, but had to cancel to attend a meeting of the Fermilab Program Advisory Committee (PAC) in Aspen, Colorado. Before the neutrino conference I spent a week at the Institute for Theoretical and Experimental Physics (ITEP) in Moscow, and gave a talk at the Institute for High Energy Physics (IHEP) in Serpukhov, about 60 miles south of Moscow. There I was hosted by Semen Gershtein, who earlier had come to CERN to ask me to speak at the neutrino conference. When I arrived at IHEP there was some commotion and I was told, to my bewilderment, that I had to go to the ladies' room to brush my hair. After lunch and my talk, Gershtein gave me an extensive tour of the laboratory and then took me to a car without—astonishingly—the standard "interpreter" to accompany me. He handed me a note and walked back across the street. The note was a telegram announcing the death of Ben Lee in an auto accident on his way to Aspen. I glanced across the street and saw Gershtein crying. Back in Moscow I got word from Ben's wife, Marianne, that the older two of our three children, Alain and Dominique, should not cancel their planned visit to the Lee family. They were the same age as the Lee children Jeff and Irene, with whom they had become close friends during our year at Fermilab. I arrived at the conference in a state of shock and sadness, but my Russian friends did their best to cheer me up.

4. THE CERN YEARS

While renting an apartment in the town of Gex in France, we had a house built in the nearby village of Échenevex, at the foot of the Jura Mountains. We had the good fortune that our next-door neighbor asked if we would like a full-time housekeeper, and she stayed with us until she retired, just before I moved to California. At that time CERN had only a half-day nursery school, so I had to drive back and forth at lunchtime, and later to music lessons and more. The housekeeper didn't babysit at night, so we eventually had a series of au pair girls as well, and bought a third car, until our youngest child, Bruno, was about 12 years old. However, upon our return from Fermilab, we encountered another problem. Échenevex had a one-room schoolhouse with an unskilled teacher. Alain and Dominique had attended the elementary school in nearby Gex, which, like the secondary school, no longer accepted students from Échenevex. Fortunately a new international high school had opened in Ferney-Voltaire, just across the border from Geneva, and Alain and Dominique were accepted on the basis of their dual nationality. We enrolled Bruno in an English school in Geneva. But the French taught there was very limited, so we next tried a French Catholic school, which seemed to place more emphasis on catechism than on academics. By the time Bruno was in third grade, a good international elementary school opened in the village of Maconnex, near Ferney, and the problem was solved.

At CERN I was approached by John Ellis, who suggested that we look into the hadronic decays of charmed hadrons (45) using tools similar to those that explained the $\Delta I = \frac{1}{2}$ rule. This was the start of a 6-year collaboration, including the first extensive analysis (46) of the properties of the Higgs particle (H) (47). This is a quantum excitation of the Higgs field (48), conjectured as a mechanism for breaking the electroweak symmetry. In particular, the W and Z acquire masses through interactions with this field, in contrast to the massless gluons of QCD.

We elucidated production processes and decay modes, and calculated production cross sections and decay rates. [We missed hadronic production of the Higgs via gluon fusion (49), $gg \to H$, and bremsstrahlung from a Z, W (50), or top quark (51) in hadron collisions.] These processes all depend strongly on the Higgs mass, which we varied from about 10 to 100 GeV, just short of the 124-GeV mass of the scalar particle whose discovery (52, 53) was announced on July 4, 2012, by Joe Incandela and Fabiola Gianotti, then spokespersons for the CMS and ATLAS experiments at CERN. In particular, we calculated the rate for $H \to \gamma\gamma$, the discovery mode. At present, the observed decay final states are $\gamma\gamma$, ZZ, W^+W^-, $\tau^+\tau^-$, and $b\bar{b}$; the $t\bar{t}H$ coupling has also been measured. The results so far are compatible with the predictions of the Standard Model, with by far the most precise results coming from the gluon fusion production mechanism.

The other two most important papers John and I wrote at the time were the predictions of gluon jets (54) and of the bottom quark mass (55). In 1975 an analysis led by Gail Hanson (56) had found that hadronic final states in e^+e^- annihilation appeared as two back-to-back jets, reflecting the underlying quark–antiquark production, and with the same angular distribution as a $\mu^+\mu^-$ final state, demonstrating that quarks are also spin $\frac{1}{2}$ particles. The following year, John suggested that gluon emission by bremsstrahlung from a quark should show up as a three-jet event. Together with Graham Ross, we calculated the cross section and the transverse momentum distribution for $e^+e^- \to 3$ jets. Our proposal at first met with skepticism from a number of theorists who thought that the third jet would be masked by a proliferation of soft gluons, but the TASSO group at DESY (Deutsches Elektronen-Synchrotron) was more receptive. This was followed by a paper (57) comparing QCD predictions with those of other strong interaction models (including, as had we, a comparison with the case of scalar gluons) and an important paper by Sterman & Weinberg (58) with a rigorous definition of QCD jets that is free of infrared singularities. There followed a flood of jet papers, some (59, 60) proposing new variables for analyzing jet event shapes. In particular, Eddie Farhi (60) proposed a variable he called *d*, for "maximum directed momentum." Then Álvaro de Rújula, John Ellis, Emmanuel Floratos, and I wrote a paper (61) on three-jet event patterns. We adopted Farhi's variable, which we called thrust, and that name seems to have stuck.

In November 1978, the PLUTO group at the DESY Proton-Electron Tandem Ring Accelerator (PETRA) analyzed (62) the decays into hadrons of the recently discovered (63) $b\bar{b}$ bound state Υ (Upsilon). These formed a three-jet configuration, interpreted as $\Upsilon \to 3$ gluons, as expected for a triplet $b\bar{b}$ state. A few months later, Sau Lan Wu and Georg Zobernig (64) proposed a method for the analysis of three jets from gluon bremsstrahlung, and the first evidence for jet broadening from the TASSO experiment was presented by Bjørn Wiik (65) at the 1979 neutrino conference. The discovery of gluon jets was cemented a month later at the Fermilab lepton-photon conference, where the PETRA groups PLUTO (62), TASSO (66), and JADE (67) presented evidence of jet broadening as well as clear examples of three-jet events, and the Mark-J group (68) presented an analysis based on our discussion (61) of event patterns.

The *b* quark had in fact been predicted. The search for charm at SLAC via the process $e^+e^- \to D^\pm + X$, where X stands for anything, had been muddied by the presence (69) of the (almost) unexpected third charged lepton tau (τ). The BIM-GJ anomaly cancellation condition then implied the existence of an associated neutrino ν_τ, and two new spin $\frac{1}{2}$ quarks (t, b), with the same gauge quantum numbers as (u, d) and (c, s). [A third set of quarks and leptons had in fact been anticipated in a paper by Kobayashi & Maskawa (KM) (70) showing that this provided an explanation for *CP* violation.] These were first called truth and beauty by some, but the less pretentious names top and bottom, introduced by Haim Harari (71) at the 1975 lepton-photon conference at Stanford University, eventually prevailed. So by 1976, the picture of elementary particles was as shown in **Table 2**, although some of the entries were yet to be experimentally confirmed: the *b* quark in 1977, the gluon in 1978, the W and Z in 1983, the top quark in 1995, and the Higgs not until 2012.

In 1977, John Ellis, Mike Chanowitz, and I had been studying "grand unified theories" (GUTs) that unified the strong, electromagnetic, and weak interactions into a single theory. These theories

Table 2 Elementary particle physics in 1976

Force	Matter	Mediator	
Strong	uuu, ddd, sss, ccc, bbb, ttt	$gggggggg$	
Electroweak	Above + e, μ, τ, ν_e, ν_μ, ν_τ	γ, $W^{\pm}$, Z	H
Gravitational	All	h	

predicted that the three associated coupling constants should be the same above the scale where the GUT was broken by the vacuum value of a scalar particle like the Higgs. An analysis (72) of the energy dependence of the coupling constants, due to quantum corrections, had indeed shown that they become equal at an energy scale of about 10^{15} GeV. We found that the Georgi–Glashow SU(5) model (73), the first—and simplest—of the proposed theories, predicted a bottom quark mass of about 4 to 10 GeV (74).

The KM paper (70), published in Japan, was essentially unknown in the West until 1975, when Luciano Maiani (75) used it to analyze *CP* violation in weak decays. A couple of months after the GUT paper, John and I, with Serge Rudaz and Dimitri Nanopoulos, wrote a paper (76) on *B* meson production and decay, and *CP* violation in the neutral *B* system.

About the same time that preprint appeared, I went to pick up Leon Lederman at the Geneva airport, and he handed me a histogram of the invariant mass of $\mu^+\mu^-$ pairs from proton–proton collisions. It showed a bump in the distribution at a mass of about 9.6 GeV/c^2, corresponding to a b quark mass of about 4.8 GeV/c^2. The following year, Andrzej Buras, John, Dimitri, and I wrote a paper (55) on the proton lifetime in GUTs, in which we also narrowed the predicted range for the b quark mass to 4.8–5.6 GeV. However, unlike the case of the charm quark mass, we don't know if agreement with the b mass prediction, derived from the assumption that the bottom and tau masses are the same above a scale of about 10^{15} GeV, is meaningful or just a happy coincidence. The specific SU(5) theory (73) on which it was based has been ruled out, as it predicted a proton decay rate faster than the current experimental upper bound.

5. LEAVING CERN

I was becoming increasingly unhappy with my status as a permanent visitor at CERN and began spending summers at Fermilab with various members of my family some or all of the time. During one of those visits, in 1978 I think, the then director, Leon Lederman, offered staff positions to Jean-Marc and myself, but Jean-Marc did not want to leave France. The same year, the prestigious biannual ICHEP meeting was held in Tokyo. Jacques Prentki, the CERN theory group leader at the time, told me that the organizers had wanted me to give the plenary talk on GUTs. Customarily, plenary speakers' expenses were covered by conference organizers, but the Japanese hosts could not afford to pay for travel from the West; they requested that CERN pay for my travel, but Jacques said that was not possible since I was not CERN staff. (I later learned that I was removed from the CERN list by a French experimentalist who was serving a term as the CERN Research Director.) The irony was that I later learned that there would be a conference in Seoul (from which most Western attendees would be going on to Tokyo) in honor of Ben Lee, where I was expected to be a major speaker. There was no way Jacques could refuse to pay for my travel to that one; he told me to find the cheapest travel possible. A bit later, another senior staff member, who probably knew nothing about what had transpired before, told me that one CERN delegate to Tokyo had withdrawn; would I like to take his place? I declined.

The CERN staff members did not take students, but visiting experimentalists from other institutions regularly brought their students with them. As the only long-term visiting theorist, I also had a few students. The first was initially a student of d'Espagnat's, and I saw him mainly on my trips to Paris. The second, also from Paris, didn't work out (he had some psychological issues), but during his time with me I learned that it would be difficult, if not impossible, for me to place a student with a Parisian group; at the time, each group tended to take the best of its own students, to the exclusion of outsiders. So I started my own group in a newly established experimental laboratory in Annecy-le-Vieux, over the French border on the other side of Geneva from CERN, and commuted two or three times a week. By then I had been sent another student from Paris, Pierre Binétruy, to whom I was supposed to assign a project. At the time, the French system required two theses: the first at a level somewhere between an American master's and a PhD, and the second somewhat beyond the level of a PhD. I misunderstood the nature of my assignment and gave Pierre a research project for the first thesis, which he successfully completed, unaware of what he was doing until it was finished. He subsequently became my student for the second doctorate, as well as the first student in, and a founding member of, the Annecy theory group. He went on to become a professor at the University of Paris and the founding director of a new cosmology center.

In 1979 Alain passed his high school baccalaureate, after being told that he could not return to the school if he did not pass it. He and his friends had become somewhat rebellious, and along with them, their younger siblings were barred from the Ferney-Voltaire high school. So we had to put Dominique in a private international school in Geneva. Along with exceptionally high salaries, CERN staff members had a number of perks, including tuition for private schools. So I went to Jacques and announced that I would spend no more time at CERN without some compensation, and for 2 years became a partially paid visitor at CERN.

Around that time there was a CERN staff meeting on women—maybe because of some fuss over a woman being denied a postdoc position. The women in the group, namely the theory secretaries and myself, were also invited. Toward the end of the meeting someone said that women face no more obstacles than men, to which I replied, "I could write an essay on the subject!" As we were leaving the meeting, John Bell said, "Why don't you?" So I did (77). I recently learned that my report was used extensively in setting up the CERN Diversity Office.

In December 1980, I spent 2 weeks at Harvard as a Loeb lecturer. Shortly after I received that invitation, I was invited to spend 4 weeks in Berkeley as a Chancellor's lecturer the following January, and I spent the winter break in between with my son Alain (who was a student at the University of Washington), including a week of skiing with friends at Sun Valley. Like most universities in the United States, Berkeley had never had a woman on the physics faculty, although there were some prominent women physicists at Lawrence Berkeley National Laboratory (LBL). But not long after I returned to CERN, I received an offer of a full professorship at Berkeley. Apparently this was largely the doing of Dave Jackson, under pressure from a group of women graduate students, who insisted that the department needed at least one woman. (At the time, I was the only woman among about 60 faculty; now there are 10 women among the active faculty, while I am the only woman retiree.) As a result, Dave was awarded a certificate granting him the title of "honorary woman."

Meanwhile, a CNRS position of Director of Research had opened up in January 1980. The Paris group was pushing for one of their own, but Raymond Stora, who had joined the Annecy theory group and was president of the relevant committee, insisted, contrary to custom, on outside letters. On the strength of the letters, I got the position. Then in 1981, after my return from Berkeley, a senior staff position at CERN was also opening up. Given that I lived close to CERN and still collaborated with CERN staff, this would have been much more convenient for me, not to mention more prestigious. [Long after I left CERN, I was told by a couple of colleagues that they had always assumed I was a CERN staff member until they read my book (78).] Again contrary to custom, two of my collaborators on the staff insisted on outside letters, but in this case the letters made no difference, nor did the fact that I had offers from Fermilab and Berkeley. Three men, at the time junior staff members, were offered senior staff positions as soon as they got outside offers—one from Berkeley and two from Stanford.

After the CERN decision, it was clear to me that I would leave for the United States. I spent the summer of 1981 running a 6-week summer school at Les Houches near the town of Chamonix in the French Alps and agonizing over the decision between Fermilab and Berkeley. Finally, I decided on Berkeley and arrived in the fall of 1981 with my daughter, Dominique, who had already planned to attend UC Santa Cruz, and my younger son, Bruno. My personal life had also changed, and we were followed a month later by Bruno Zumino, who also joined the Berkeley physics faculty, and our dog.

6. PHYSICS FROM THE TeV SCALE TO THE PLANCK SCALE

The Standard Model was complete, at least conceptually, by 1976, except for the mechanism responsible for breaking the symmetry of the electroweak theory. The Higgs mechanism was the simplest explanation, but in the early 1980s there was no evidence for a Higgs particle. In the early days of the Fermi theory one knew the theory was incomplete because the processes it predicted grew with energy in such a way that at sufficiently high energy, the scattering probability would exceed unity. This energy, a few hundred GeV, was known as the unitarity limit. The W and Z, with masses of about 80 and 90 GeV/c^2, provided the needed damping of the growth of the scattering amplitudes. But now there was a new unitarity limit. In 1977 Ben Lee, Chris Quigg, and Hank Thacker (79) showed that if the Higgs mass exceeded about 1 TeV (teraelectronvolt), the WW scattering probability would exceed unity at some sufficiently high energy. Mike Chanowitz and I inverted this argument to show (80) that for a sufficiently large Higgs mass the WW scattering rate would surpass the unitarity limit at 1.8 TeV center-of-mass energy. WW scattering could be observed by W bremsstrahlung from colliding proton or proton–antiproton pairs and subsequent rescattering. We calculated (80, 81) this process using what we called the equivalence theorem.

The Higgs mechanism is the simplest (and apparently correct within current measurement precision) model for electroweak symmetry breaking; it works as follows. The GWS theory is invariant under a gauge group $SU(2)_L \otimes U(1)_w$, where the subscript L implies that the group acts only on negative helicity (left-handed) fermions, and w stands for weak hypercharge. This symmetry is broken by the introduction of a complex scalar field that is an electroweak $SU(2)_L$ doublet. When one component acquires a nonvanishing value in the vacuum (state of lowest energy), in addition to the associated Higgs particle, the particles associated with the other three field components become the longitudinal spin components of the now massive $W^{\pm}$ and Z. It is these components that interact strongly at high energies, and we showed that, with increasing accuracy for increasing energy, their interactions were identical to those of the original scalars, considerably simplifying the calculation. In particular, this simplification allows the use of the chiral symmetry techniques of low-energy pion physics. We further showed that, although the longitudinal components couple to protons much more weakly than the transverse components, this is compensated for by kinematic effects that enhance the former relative to the latter when the W or Z is emitted in the direction of motion of the proton. Although the simple light Higgs model turned out to be the correct answer, the methods we developed are still used in studying models for physics beyond the Standard Model. For example, there could be a very high-energy regime where strong WW scattering does occur.

In any case, there is still an important puzzle surrounding the Higgs sector of the Standard Model—namely, the large energy gap between the scale v of electroweak symmetry breaking (about a quarter of a TeV) and the GUT scale where the gauge couplings unify, or the reduced Planck scale where gravitational interactions become strong (larger by 12 and 16 orders of magnitude, respectively). There is another, less precisely defined concept called naturalness, or the 't Hooft naturalness condition (82), that guides our intuition about how large the observed values are expected to be after quantum corrections are included. It states that a quantity is "naturally" small if the symmetry of the theory increases when the quantity vanishes. There is generally no such symmetry associated with a massless scalar, and one expects the quantum-corrected mass to be influenced by any larger mass scale in the theory. That is why a Higgs vacuum value $v \sim \mu$ ($\lambda < 4\pi$ if the coupling is not strong) is hard to understand in the presence of the much larger GUT and Planck scales. This dilemma is known as the gauge hierarchy problem.

For this reason the majority of the community believed that some new physics, responsible for the suppression of the Higgs mass, should show up in the TeV region for electroweak interactions. One candidate, known as technicolor, was first proposed by Steve Weinberg (83). The original idea was simple and elegant. In QCD, chiral symmetry is believed to be broken by a quark condensate—that is, a nonvanishing expectation value of a quark bilinear in the vacuum: $v_\chi^3 = \langle \bar{q}q \rangle \neq 0$. This is invariant under color SU(3) transformations, but not under electroweak transformations because left and right quarks transform differently: $\bar{q}q = \bar{q}_{\rm L}q_{\rm R} + \bar{q}_{\rm R}\bar{q}_{\rm L}$. If there were no other source of electroweak symmetry breaking, the W and Z would acquire masses of order $m \approx \frac{1}{2}g_{\rm w}v_\chi \approx 100$ MeV, where $g_{\rm w} \approx 1/\sqrt{2}$ is the electroweak coupling constant and $v_\chi \approx 300$ MeV is roughly the confinement scale. If there were a second, more strongly coupled gauge group called technicolor that confined at the scale v of electroweak symmetry breaking, this would produce W and Z masses $m \approx \frac{1}{2}g_{\rm w}v \approx 80$ GeV—that is, the observed masses. However, this gives masses only to the W and Z, and not to the Standard Model fermions. When the theory was extended to generate masses for fermions, it became considerably more complicated, and required the introduction of many arbitrary parameters. In addition, it predicted new heavy "technifermion" bound states, and no hints of these showed up in the experimental data.

Eventually a theory called supersymmetry (84–86) became the favored candidate for stabilizing the Higgs sector. This entails doubling the number of particles: For every fermion degree of freedom there is a boson degree of freedom, and vice versa. These fermion–boson pairs are degenerate in mass up to supersymmetry breaking effects. Because fermions and bosons contribute to quantum corrections with opposite signs, many of the infinities encountered in ordinary field theory are no longer present, and there is no impediment to stabilizing the Higgs mass. In order to have a Higgs mass less than about a TeV, this requires that the "superpartners" of Standard Model particles be lighter than a TeV or so. Unfortunately, none have shown up in the anticipated range. Specifically, squarks, the scalar superpartners of quarks, have been ruled out over a mass range up to more than a TeV, as have gluinos, the fermion superpartners of gluons, whereas lepton, electroweak gauge boson, and Higgs superpartners have been ruled out over somewhat smaller mass ranges.

Nevertheless, even if broken at a much higher energy than the electroweak scale, supersymmetry may play an important role in particle physics because of its cancellations of divergences. In extended supersymmetries there are N supersymmetry operations, and thus more cancellations. A supersymmetry-lowering operation reduces helicity by half a unit; thus, in a renormalizable theory, which has maximum spin 1, for $N = 1$ supersymmetry we get massless multiplets consisting of a complex scalar and a fermion or a fermion and a vector. More generally, since supersymmetry interchanges fermions and bosons, the supersymmetry generators $Q_i, i = 1, \ldots, N$, behave like fermions and can act on a state only in totally antisymmetric combinations. One cannot apply the same generator twice, so starting with helicity S and applying N Q_i, one gets helicities ranging from S to $S - N/2$ with multiplicities $m = N!/[n!(N - n)!], n = 0, \ldots, N$. Thus, in $N = 2$

supersymmetry there can be any number of triplets, with helicity components $(S, S-\frac{1}{2}, S-1)$; for a renormalizable theory, only $S=(1,\frac{1}{2},0)$ and their conjugates are allowed.

Since a renormalizable theory has at most $|S|=1$, the largest allowed value of N is four. In 1983, Stanley Mandelstam (87) proved that $N=4$ supersymmetry theories are in fact finite; no infinities are encountered in the evaluation of quantum corrections. However, these theories are not realistic for physics, since the matter particles all have the same gauge quantum numbers as the gauge bosons; all matter is in the adjoint representation of the gauge group. In fact, only $N=1$ supersymmetry is compatible with the Standard Model; $N=2$ has only Dirac fermions and no Weyl fermions such as neutrinos, and $N=3$ has the same spectrum as $N=4$, once hermicity is imposed.

When we include gravity, the graviton has a supersymmetric companion called the gravitino, and the theory is called supergravity, discovered in 1976 by Sergio Ferrara, Dan Freedman, and Peter van Nieuwenhuizen (88) and by Stanley Deser and Bruno Zumino (89). Its symmetry is a supersymmetric extension of the Poincaré group, and supersymmetric transformations are local—that is, their parameters depend on space-time coordinates, as for any gauge theory. The maximum allowed helicity is now $|S|=2$, and $N\leq 8$. It was conjectured that the $N=8$ theory might also be finite, a conjecture now supported both by explicit calculations in perturbation theory (90) and by general arguments (91). Murray Gell-Mann was the first to consider $N=8$ supergravity as a possible theory of particle physics (M. Gell-Mann, personal communication). Since there is no quantum theory with spin greater than two, and there is a single spin 2 elementary particle, the graviton, there is a single elementary supermultiplet. The theory is invariant under a global SO(8), which is promoted to a local SO(8) if the theory is gauged. The supermultiplet includes 28 spin 1 particles, which form an adjoint representation of SO(8) and become the gauge bosons in the gauged version. The SO(8) adjoint contains the generators of $SU(3)\times U(1)$, and the corresponding gauge bosons could be identified with the gluons and photon of $SU(3)_c\times U(1)_{QED}$. However, it does not contain the remaining generators of the electroweak gauge group, and the $W^{\pm}$ and Z would have to be considered as composites. Furthermore, this supermultiplet does not include the full spectrum of Standard Model fermions among its 56 elementary fermions, so some of these must also be composite in this picture.

John Ellis, Luciano Maiani, Bruno Zumino, and I (EGMZ) (92) considered instead the ungauged version of $N=8$ supergravity, which in fact has a much larger symmetry group, $SU(8)\times E_{7,7}$, as shown by Eugène Cremmer and Bernard Julia (93). The full global SU(8) symmetry can also be gauged, provided all observed elementary particles are bound states, with the elementary $N=8$ supermultiplet states presumably confined. In this way we obtained all the particles of the Standard Model, as well as many others. Including the full spectrum of massless states did not give an anomaly-free theory; we truncated this to an anomaly-free subset by invoking what we called Veltman's theorem. One day Bruno and I were having lunch with Martinus (Tini) Veltman and asked him whether one could argue that for a theory to make sense below some energy scale, it had to look renormalizable below that scale. He gave us a convincing argument as to why this was a reasonable assumption. Invoking this "theorem," we were able to show that we could obtain the full Standard Model as bound states, that there were precisely three families of fermions, as in **Table 2**, and that the unique GUT was the Georgi–Glashow SU(5) theory.

However, we were never able to derive the Standard Model interactions in this picture, and eventually we abandoned it in favor of string theory, which was emerging as the leading contender for a "theory of everything"—that is, a theory that unifies gravity with the other forces of particle physics. However, there were some amusing consequences of our endeavor.

In December 1980, my last Loeb lecture at Harvard was on $N = 8$ supergravity. At the time, my cousin was dating a student of Howard Georgi, John Hagelin, who was a disciple of the Transcendental Meditation guru Maharishi Mahesh Yogi. John attended all my lectures and copied all my slides. Not long after, a poster appeared featuring $N = 8$ supergravity as the Maharishi's view of the unified field, incorporating not only the forces familiar to particle physicists, but human consciousness as well.

Much later, in the mid-1990s, I was being vetted by the White House legal office for a position on the National Science Board (NSB); this is a presidential appointment, requiring Senate confirmation. I was asked about three issues: (*a*) Did I have a "nanny" problem? (This question was unsurprising, given Bill Clinton's recent difficulties trying to nominate a woman Attorney General.) I thought not because my children were grown, but it turns out that a house cleaner counts as a nanny, and I had to pay 5 years of back taxes in 20 separate payments, some as small as 5 cents, each carrying a 35-cent stamp. (*b*) In 1993 I had signed a letter to several newspaper editors in support of the then proposed, but later canceled, Superconducting Super Collider (SSC). I replied that I still thought it should have been built. (*c*) Most astonishing: Haim Harari had written a *Scientific American* article on composite models, including ours, of which he said, "ambitious," but "like other composite models. . .has serious flaws." I simply said that we no longer believed in the model ourselves, but I couldn't fathom why they cared.

A much more consequential spin-off of the EGMZ effort was a paper (94) I wrote with Bruno. There was some question as to whether composite spin 1 particles could be massless. Cremmer and Julia had speculated that this could happen, based on work by Alessandro d'Adda, Paolo Di Vecchia, and Martin Lüscher (95), who had studied a theory in two space-time dimensions with a symmetry structure similar to (albeit much simpler than) $N = 8$ supergravity. However, it had been argued (96, 97) that massless composite vector particles coupled to gauge-invariant conserved currents cannot exist. Bruno and I showed that the conserved currents associated with the symmetry of the $N = 8$ Lagrangian, which involved unitary transformations among the gauge field strengths along with the interchange of electric and magnetic field strengths, were not gauge invariant—although the associated conserved charges were. In this way we were able to evade the no-go theorems. It turns out that our paper has had many applications in string theory, and the current we found has been used by Renata Kallosh (91) in her attempts to prove that $N = 8$ supergravity is finite.

String theory, which replaces particles by tiny oscillating strings, was first introduced (98, 99) as a candidate theory for strong interactions, but consistency of the theory required more spatial dimensions than the three that we observe. The year 1984 marked the arrival of the "first string revolution," when Mike Green and John Schwarz (100) showed that supergravity in 10 dimensions, which is the limit of string theory for vanishing string size, is fully consistent for just two choices of gauge group. One of these, $E_8 \times E_8$, shows much more promise for describing particle physics when six of the extra dimensions are curled up into Planck length scales. This is what I have primarily been working on in recent years, studying its implications for both accelerator physics and cosmology (101–103; see also 104 and references therein).

7. SERVICE

Of the limited number of service tasks I took on shortly before leaving Europe, by far the most time-consuming—and most rewarding—was as scientific director of a Les Houches summer school in the summer of 1981 (105). I was familiar with the format of these schools, a French tradition since their establishment in 1951 by Cécile DeWitt-Morette. They typically last about 6 weeks, and I prepared a program accordingly. Then I was told by Maurice Jacob, who had asked me to take on this endeavor, that he had only a 2-week session in mind. When I showed him the program I had developed, which had an impressive list of lecturers, he gave the 6-week proposal the go-ahead. The school turned out to be an enormous success. Thirty-eight of the 51 young participants are still active in particle physics, including five of the seven women, an unusually large contingent of women at that time. The eminent mathematician Izzy Singer also came as a student, and the friendship my son Bruno developed with his daughter was a factor in our opting for Berkeley over Fermilab.

In 1979–1981, I served on the Visiting Committee of the Center for Particle Physics of Marseilles, where we had to contend with a fractured theory group, bitterly divided along political lines. This was not atypical at French laboratories at the time. There was a similar situation within my old theory group at Orsay. On one visit there I found myself, along with another "apolitical" theorist, negotiating the "release" of a visiting professor being held hostage in his office by a group of students because of his "elitist" views—too much reference to heroes of physics as opposed to espousing the correct Maoist view of physics research as a group endeavor.

The year before leaving, I also served the first year of a term on the CERN SPS (Super Proton Synchrotron) Committee, resulting in a paper (106) related to one of the proposed experiments that we were discussing.

Upon my move to California, as well as serving a term as LBL theory group leader, I was inundated with national committee assignments. In 1982 I was appointed to a Department of Energy (DOE) committee to review DOE-supported theory groups at universities. We also made some general recommendations for improving *Physical Review D* that were eventually implemented. Our other achievement was the establishment of the Theoretical Advanced Study Institute in Elementary Particle Physics (TASI), a summer school in the mold of Les Houches. It originally changed locale every year, eventually finding a permanent home at the University of Colorado Boulder. In 1983 I was also enlisted to serve on the APS (American Physical Society) Committee on Women in Physics, and as chair in 1985. Our major accomplishment was the foundation of the Maria Goeppert Mayer Award "to recognize and enhance outstanding achievement by a woman physicist in the early years of her career" (**https://www.aps.org/programs/honors/prizes/goeppert-mayer.cfm**). The idea was to increase women's visibility in the field, and the program was originally intended to continue for no more than 5 or 10 years, assuming that it would no longer be needed after that, but it continues to this day.

That year, I was also appointed to the most difficult—and painful—committee assignment of my career: the 1983 HEPAP (High Energy Physics Advisory Panel) subpanel, often referred to as the Woods Hole Panel because it traditionally held its final meeting at the National Academy of Sciences (NAS) Conference Center, a research complex in Falmouth, Massachusetts. There were two principal questions before us. The easy one was, "Should we recommend the construction of a proton–proton collider with an energy of 20 TeV per proton, which had been studied in detail by a working group at a 1982 meeting in Snowmass, Colorado?" This was supported by the vast majority of the high-energy community, and the answer was an emphatic yes—aside from one initial skeptic on our subpanel whose doubts that anything would be found prompted me to draft Mike Chanowitz to look into W and Z scattering in the multi-TeV energy regime (80, 81). The contentious issue was, "Should we recommend completion of the Colliding Beam Accelerator (CBA) at Brookhaven?"

Originally known as ISABELLE, the CBA had been proposed in the early 1970s as a proton–proton collider with 200 GeV per beam to address the first "unitarity limit." Its construction was recommended by HEPAP in 1974 and began in 1978. However, there were problems with the magnets, and a complete overhaul, with an upgrade in energy to 400 GeV per beam (along with the name change to CBA), was undertaken in 1982.

By the time our subpanel was formed in the spring of 1983, this first unitarity limit had already been addressed, in theory at least, by the proposed *W* and *Z* bosons. Now there was a new unitarity limit, imposed by the predicted rise in their scattering cross sections with increasing energy. An exploration of this region would require proton beams with many TeVs of energy in each beam because a parton that participates in the elementary scattering process carries only a fraction of the proton energy. This was the energy regime that the collider (the SSC) was proposed to explore, and many in the community, especially the younger contingent, felt that the CBA was no longer timely and that its construction would only delay the SSC. The Tevatron, a proton–antiproton collider with a much higher energy (1 TeV per beam) than the CBA, albeit a considerably lower intensity, was already being built at Fermilab. However, there was considerable pressure from the DOE, as well as senior members of the East Coast establishment, to proceed with the construction of the CBA, in part because Brookhaven had long been the center of US particle physics, and they could not envision losing this status. Even some of us who felt that the CBA had no potential for discovery (which proved correct in hindsight) had a sentimental attachment to Brookhaven, where our early careers had been nurtured. There were also two European members of the subpanel. John Adams, who had been a Director-General of CERN, clearly represented European interests: The CBA was needed to provide particle beams to European physicists while they awaited their own supercollider, the LHC (Large Hadron Collider), with higher intensity but less than half the energy of the SSC. Carlo Rubbia, a brilliant physicist and future Nobelist, freely acknowledged at dinners that dropping the CBA in favor of proceeding expeditiously with the SSC was the wisest course for US physics, but wore a distinctly European hat at meetings.

Our weeklong meeting at Woods Hole was tense and exhausting. One evening, following an after-dinner session, I went to relax with a beer and enjoy the view over the water, when I was joined by Jim Leiss, the DOE Associate Director for High Energy and Nuclear Physics, who immediately began pressuring me to support the CBA. At some point I lost my temper and asked him why they needed a subpanel if they already knew the answer they wanted. His deputy, Bill Wallenmeyer, got him to back off, and they left. Years later, Bill told me we had made the right decision. But it was a painful process. After several votes with the "no" on CBA prevailing by a slim margin, we were told we had to meet again at a later date. In the interim there was a theoretical particle physics conference in Shelter Island at the tip of Long Island. The three subpanel members who attended were hounded by other physicists as well as the *New York Times* science reporter Walter Sullivan, who were trying (unsuccessfully) to extract leaks about the status of our deliberations. Once, my husband Bruno and I were joined at dinner by T.D. Lee, who began pressuring me on behalf of the CBA. Bruno, an old friend of T.D.'s, deftly changed the subject and got me off the hook. The subpanel met again at Nevis Laboratories in Irvington, New York, an equally taxing ordeal. However, after many votes, with still a slim "no" verdict, we came to a conclusion and wrote a divided and contentious report (107).

Once our report was endorsed by HEPAP, I participated in a variety of activities promoting the SSC: articles (108, 109), conferences (110), lectures (111, 112). Two of my colleagues, who had earlier dragged me through the halls of Congress to lobby for the SSC, convinced me to take a one-day trip to Washington to make a 5-minute cameo appearance before a large gathering of high school students. Afterwards, one of them asked me to autograph his T-shirt; another thanked me for assuring his future. Alas, it was not to be. Shortly thereafter, the SSC died in Congress, and exploration of the TeV regime was to be put off for another decade. Perhaps that is why my research gravitated to the early Universe and an even more inaccessible energy scale—the Planck scale. And perhaps it is why I accepted an appointment to the NSB, as a way to do my bit in promoting scientific research.

I also served on various physics advisory (reporting to the funding agency) and visiting (reporting to the lab director or dean) committees at national labs and physics departments, including both of these committees in accelerator physics and in astrophysics at Fermilab. Except for the fact that we sometimes had to make hard decisions about what experiments should be funded, these were generally congenial and uneventful. An exception was one theory group with a situation reminiscent of Marseilles, except that the divisive issue was personalities rather than politics. When I was on the advisory board for the Institute for Theoretical Physics at Santa Barbara, I made the mistake of skipping my last meeting to go to my father's wedding, and came back to find myself chair of the search committee for a new director, a task that took up a major portion of my time for the following 18 months.

In 1992, while I was serving a 4-year term on HEPAP, I was appointed to another—slightly less painful—HEPAP subpanel. This time the issue was whether to proceed with the proposed B-factory at SLAC. The production of a large number of particles containing bottom quarks had a lot of scientific potential, in particular for the elucidation of the mechanism responsible for *CP* violation. However, there were similar facilities being proposed at Cornell and in Japan, and the Tevatron at Fermilab would produce a very large number of *B* particles, albeit in a less clean environment. Some felt that another B-factory was redundant and that SLAC's commitment to one would detract from the national effort in support of the SSC (a concern that I shared). Our subpanel had to visit every national laboratory, sometimes two in the same weekend. I remember one wintry evening waiting at the Ithaca airport for the weather to clear enough for us to fly to Islip on Long Island. We finally arrived very late at night only to be rudely awoken a few hours later because a prankster had set off the hotel fire alarm. Once, at a dinner, the young protégée of a close friend of mine began lobbying me to support the B-factory. My friend managed to change the conversation, much as Bruno had done at Shelter Island 10 years before. In selecting the members of the 1983 subpanel, the DOE had purposely avoided including lab directors or their lieutenants, a decision they perhaps regretted, given the outcome. This subpanel had a very different composition, and this time the establishment got the result they desired (113). As I recall, we wrote up an uncontentious, compromise report, approving the B-factory subject to certain conditions.

Finally, in 1996 I was promoted to my level of incompetence—dealing with issues I knew little about—with an appointment to the NSB. Aside from approving (or not) large science projects for support by the NSF, we studied issues relevant to the US scientific enterprise and published reports on our findings. Of those I was most involved in, the first was on setting priorities in scientific research (114). As chair of the relevant subcommittee, I was deeply involved in recommendations to improve K–12 STEM (science, technology, engineering, and math) education in the United States, which lagged behind that in many other industrialized nations (115). One of our major recommendations was for a standardized curriculum, which seemed to be gaining traction during the Obama administration. Our published reports (116, 117) emphasized a "mobile population" as the rationale for this. Our subcommittee felt strongly that the incentive for standardized texts with an in-depth treatment of limited material at each grade level, instead of a superficial treatment of a lot of material, was at least as important, but the chairs of the NSB and of the committee we reported to were afraid of offending textbook publishers, so this got short shrift in our reports. My reward for these efforts was a trip to Antarctica, where the NSF has a research station: five days "on the ice," with a day at the South Pole and, except for a light snowfall one day over the McMurdo Dry Valleys, beautiful, clear, sunny weather.

Last October, my colleagues organized a symposium in honor of my 80th birthday (see proceedings at **https://indico.physics.lbl.gov/event/978/**). One of the speakers was Shirley Jackson, the president of Rensselaer Polytechnic Institute and a former chair of the Nuclear Regulatory

Commission, with whom I had become good friends when she was a postdoc at Fermilab and then at CERN. She spoke of her career in service and then made the insightful comment that she does these things because she enjoys them, whereas "Mary K does them to support science."

8. WHAT'S NEXT?

The development and verification of the Standard Model of particle physics was an extraordinary achievement of twentieth-century physics. However, many unanswered questions remain, among them the origin of the gauge hierarchy discussed above. We don't understand the fermion particle spectrum with its large mass gaps, ranging from the nearly massless neutrinos and the extremely light electron to the very heavy top quark. Nor do we understand the pattern of Yukawa couplings, with the somewhat hierarchical pattern of quark couplings and rather chaotic pattern of neutrino couplings. Some of these issues will be addressed in planned facilities such as long-baseline neutrino beams and B-factories.

Questions about the nature of dark matter, and possibly about the even more mysterious origin of dark matter, are being addressed in underground and cosmological experiments. There has been a wealth of data in recent years informing us about the nature of our Universe, not least the unexpected discovery of a nonvanishing cosmological constant.

What remains more problematic, in my mind, is the possibility for exploring the multi-TeV energy regime. I believe this will require a drastic reduction in cost, which in turn will require dramatic advances in accelerator technology. The support of research in this area is therefore crucial for continued exploration of the high-energy frontier.

DISCLOSURE STATEMENT

The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

ACKNOWLEDGMENTS

I am grateful to Zoltan Ligeti for constructive remarks.

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Annual Review of Nuclear and Particle Science

Volume 71, 2021

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