Technology Review
PUBLISHED BY MIT

March/April 2009

Physics: A Family Business

Modern physics through the generations.

By Gino Segrè

In September 1955, just off the boat from Italy and not yet 17, I enrolled as a freshman at Harvard College. Luckily, I already knew English. Although I was born in Florence just before the outbreak of World War II, my family had taken refuge in New York during the conflict, not returning to my birthplace until 1947. Eight years later, when it was time for me to go to college, my parents decided I should do so in the United States. My trip to Cambridge began with their delivering me to Florence's Santa Maria Novella train station and waving good-bye. The Ferrovie dello Stato, Italy's train system, conveyed me to a boat at Le Havre, which in turn transported me to New York. Another train landed me in Boston. With a bulging suitcase in hand, I took the subway from South Station to Harvard Square.

I still remember my dismay when, expecting to be greeted by the inviting setting I had seen in pictures, I exited from underground to see nothing but traffic and busy stores. I timidly asked an elderly, professorial-looking passerby the whereabouts of Harvard. He answered, "You must be a freshman. Walk a few steps forward, turn to your right, and you will see a gate. Go through it!" The sight of the promised Harvard Yard reassured me. A room on the quad also portended well. But soon I received another shock, albeit a minor one: wearing a tie and jacket was obligatory at all meals. I came equipped with the latter, but I had no tie. Since this meant no meals, I immediately went out and bought a bow tie--the clip-on variety, so that I wouldn't have to learn how to tie the knot.

During my first year of college I dutifully wrote home once a week, reporting on the progress I was making but glossing over some of my adjustment difficulties. In time I made close friends, many of whom were struggling with the same problems. My immediate academic goal was to learn physics, the discipline I had chosen. This didn't turn out to be as easy as buying a bow tie: I was not the genius I had hoped to be. Nonetheless, after four years of serious endeavor, I was accepted by good graduate schools and chose to go to MIT. That choice was due largely to Francis Low, a physics professor from down the river who came to deliver an endowed set of lectures at Harvard during my senior year. Even though I didn't understand much of what he was espousing, Harvard's graduate students and faculty paid close attention to the equations he was writing on the blackboard and the intriguing Chew-Low scattering model he was presenting. It all sounded exciting; I checked with my faculty advisor, who agreed that going to MIT seemed like a good idea. Having little mechanical aptitude, I also decided that I would try to become a theoretical physicist.

In November 1959, my first year in graduate school, I heard that Owen Chamberlain and my uncle Emilio, an experimental physicist, had won that year's Nobel Prize in physics for their discovery of the antiproton. The existence of antimatter particles--identical to the electron and the proton in mass but opposite in electric charge--had been proposed almost 30 years earlier by Paul Dirac as a result of an attempt to combine quantum mechanics with the special theory of relativity in a single beautiful equation. Most physicists initially regarded this idea as wildly speculative, but the 1932 discovery of the antielectron (a.k.a. the positron) proved that Dirac was right. Finding antiprotons took another quarter-century because their production in the laboratory required powerful particle accelerators, which were not available until the 1950s. Unlike the discovery of the antielectron, which had come as a shock and a surprise, proof of the antiproton was expected. But it was a crucial confirmation of the equations that theoretical physicists were now using as common tools.

Back in 1959, I wondered if my uncle's Nobel Prize was an augur that I was making the right choice. I thought that if he could succeed in the profession, perhaps I would as well. On the other hand, my uncle was also setting the bar incredibly high. My father tried to cheer me up by writing that, prize or no prize, it was better to be a theoretical physicist than an experimental one like my uncle. His reasons for reaching this conclusion were murky at best; he was a professor of ancient history and knew next to nothing about the nitty-gritty of physics. I couldn't help thinking that his judgment might have more to do with the unfortunately strained relations between the two brothers than with anything else.

In any case, my chosen field--high-energy physics, sometimes called elementary-particle physics--seemed particularly promising, and I was happy with the choice. The interplay between theory and experiment was especially exhilarating. Experimentalists were finding surprising results, with theorists providing explanations shortly afterward; in other cases, theorists made predictions that were quickly proved or disproved by ingenious experiments. The most striking example at the time was Tsung-Dao Lee and Chen Ning Yang's analysis of how a reaction and its mirror image might be distinguished from one another, a violation of so-called parity symmetry. Their 1956 conjecture was rapidly confirmed, and the Nobel Prize was awarded to them just a year later, in 1957.

In addition, ever-larger accelerators were being put into operation, producing new and often unexpected particles at a prodigious rate. Murray Gell-Mann was pioneering attempts to group these new entities into families, with members related to one another by symmetry considerations. And he was only 30, the same age Lee had been when he received the Nobel Prize. This was a new field with new leaders. I was beginning to think that the situation might be like the earlier development of quantum mechanics, when Wolfgang Pauli, Werner Heisenberg, and Dirac had created a revolution while still in their mid-20s. Since I was only 21, there was hope that I could be a player within a few years if I had the necessary ability.

Fifty years later, I view that moment differently. I see myself not stepping into a rapidly emerging field but entering at the midpoint of a great century-long arc that stretches from Ernest Rutherford's first scattering experiments to CERN's Large Hadron Collider--from a seemingly unimportant research exercise carried out by two students to an international endeavor engaging thousands in a decade-long quest to build a multibillion-dollar machine. Though the beginning was simple, the end point is probably the most technologically sophisticated experiment ever attempted.

I called this century-long search an arc, but an ascent might be a more appropriate metaphor, for we have moved steadily over the course of these hundred years toward bigger and bigger experiments. On the other hand, we have also been descending, probing matter at ever smaller scales--from the atom to the nucleus to the protons and neutrons to the quarks and, finally, to whatever comes next. I place my entry into the field not only at a chronological midpoint but also at an organizational one--a time when a single university group could still mount a successful experiment, when computers were in their infancy and analyses could be carried out in days.

Perhaps one should actually start the story 113 years ago, when Henri Becquerel discovered radiation coming from uranium ores; this indicated the presence of a novel energy source, more powerful than anything then known. Two years later, Marie Curie and her husband, Pierre, published their discovery that radioactivity was an atomic property of uranium and other materials. It was not long before Rutherford, a young New Zealander working in Cambridge, England, found that this radiation had two components; he called them alpha rays and beta rays. But I place the beginning of the arc in 1909, when two young physicists working for Rutherford, by then an established professor in Manchester, began at his urging a new kind of experiment. They bombarded a thin gold foil with alpha particles, constituents of alpha rays like the ones Rutherford had discovered a decade earlier. Since that revolutionary experiment, physicists have been smashing ever-more-energetic particles against ever-more-sophisticated targets. The means have changed over the course of the century, but the goal of probing the constituents of matter at smaller scales has not.

It was in 1911 that Rutherford realized what the Manchester experiments implied: the atom, contrary to prevailing beliefs, was composed of electrons moving about a minuscule, massive core. The following year he used the term nucleus to describe that core. Measuring typically little more than a hundred-thousandth of the atom's radius, the nucleus nevertheless contained essentially all of its mass.

Twenty years later, probing deeper, physicists discovered that the nucleus is composed of neutrons and protons, presumably held together by a previously unimagined force. Forty years after that, they found that neutrons and protons are in turn each made up of three quarks. This year, when the Large Hadron Collider begins operation in full, we will take the next step on the journey--one whose progression Abraham Pais, a physicist and historian, has described in a book aptly called Inward Bound.

Physics' First Families

Over the span of this century-long arc, the energy of the projectiles employed has increased by a factor of a million, the cost of the necessary apparatus from a few hundred to billions of dollars, and the size of the teams at work on a typical experiment from at most two or three to hundreds. But this story is much more than simply one of bigger machines and larger expenditures. It is also a tale of the people who achieved this extraordinary growth--people who were linked to one another, sometimes by blood or marriage (as I can attest, having a slew of relatives in what I sometimes jokingly refer to as "the family business"), but in all cases by common aims.

I had a romanticized image of the field when I entered it 50 years ago, but it's been moderated by finding out about some of the bitter disputes that have arisen along the way. (Two of my early heroes, Lee and Yang, once as close as brothers, have not spoken to each other for decades.) Yet while I now see the warts, I also have a greater appreciation for the support and even affection so common in the physics community. Great labs such as the Cavendish at Cambridge, Curie's Radium Institute, and Niels Bohr's Institute for Theoretical Physics often fostered quasi-familial feelings, occasionally heightened by the sight of parent and child working side by side.

A family-like atmosphere certainly existed wherever Rutherford presided. At the end of World War I, he left Manchester. Joseph John Thomson, who had received a Nobel Prize in 1906 for discovering the electron, decided to step down from Cambridge's Cavendish Professorship, a post he had held for 35 years. Rutherford, then at the peak of his powers, was his natural replacement. He accepted, and for the next 15 years the Cavendish Lab, under his leadership, was the world's premier research facility in nuclear physics.

At the Cavendish, Rutherford was jovial but stern when he needed to be, always encouraging the group he called "his boys." Bohr once wrote of him, "However modest the result might be, an approving word from him was the greatest encouragement for which any of us could wish." There was no doubt who was the "father" and who had the last word, but Rutherford's intuition was formidable and his judgment excellent, and he was never threatened by suggestions from others. He began his day by going over the physics news with the assistant head of the lab, James Chadwick, who had worked by Rutherford's side since his own undergraduate years at Manchester before World War I. Rutherford would then walk around the lab, offering suggestions. When preliminary results were available, he would sit on a stool near the experimenter's lab bench, pull a pencil out of his waistcoat, and check to see if the data seemed right. The lab's restrictions now seem archaic: doors that shut punctually at 6:00 p.m., mandatory vacations, and a prevailing ethos that you built your own equipment--not too expensively, either. Viewed through today's lenses, Rutherford's behavior was paternalistic. But there was no resentment.

No experiment at the Cavendish would be more influential than Chadwick's 1932 discovery of the neutron. Ushering in the modern era of nuclear physics, it was a triumph for Rutherford's boys, and it marked the start of a period in which experimentalists regained the lead from theorists like Bohr, Heisenberg, and Schrödinger, who had been dominant since Rutherford discovered the nucleus 20 years earlier.

The nucleus's makeup had been a puzzle ever since that surprising discovery. It was known that an oxygen atom, for example, had eight electrons surrounding a nucleus containing eight protons, but the atom's mass seemed to indicate the presence of 16 protons--twice the expected number. It was commonly believed that nuclei contained additional protons tightly bound to the very much lighter electrons, thus neutralizing their charges. But this didn't seem to make much sense: how was it possible that electrons were sometimes inside the nucleus, if ordinarily they resided well outside it? An alternative explanation, long suspected by Chadwick and Rutherford, was the existence of a particle with a mass very close to the proton's but with no electric charge. As expected on the basis of mass estimates, the oxygen nucleus contained eight of these newly named neutrons, alongside the eight protons.

Chadwick's discovery beat out the cross-Channel competition of Madame Curie's daughter Irène, who had formed a formidable research duo with her husband, Frédéric Joliot. Irène and Frédéric had the Nobel Prize in physics within their grasp twice, having achieved first sightings of both the positron (the electron's antiparticle) and the neutron. Each time, they misidentified their observation and saw the prize go to others. Forging ahead despite these disappointments, in January 1934 the Joliot-Curies announced the first instance of artificially induced radioactivity, a result that would have immense repercussions for medicine as well as pure science. All were satisfied when, in 1935, Chadwick received the Nobel in physics and the chemistry prize went to the young French couple.

The literal family ties hardly end with the Curies. William Lawrence Bragg, Rutherford's successor as Cavendish Professor, had shared the 1915 Nobel Prize in physics with his father, William Henry Bragg, for their study of crystal structure by means of x-rays. Rutherford's predecessor, too, saw his son receive a Nobel, albeit 31 years after his own: curiously, Joseph John Thomson was cited for discovering that the electron is a particle, while George Paget Thomson received the award for proving that the electron is a wave. Cognoscenti recognize this apparent contradiction as a confirmation of one of the central tenets of quantum mechanics: that an electron (as well as a photon) is both a particle and a wave, though the two manifestations cannot be detected simultaneously. The particle nature of radiation explains the photoelectric effect; the wave nature of electrons has enabled the development of the short-wavelength microscopes that bear their name.

The man principally responsible for developing the theory of wave-particle duality is Niels Bohr, a theoretical physicist whose career was critically shaped by a 1912 stay with Rutherford in Manchester. A deep bond of affection was forged between the established scientist and the young Dane, who later referred to Rutherford as a second father and even named one of his sons Ernest. Although Rutherford tried more than once to have Bohr join his professional family, first in Manchester and later in Cambridge, Bohr's commitment to his native Denmark could not be broken. Yet the two maintained a tie grounded in their common physics interests and their complementary areas of expertise. In approaching problems of first the atom and later the nucleus, Rutherford looked to Bohr for guidance in theoretical matters and Bohr to Rutherford for the significance of experiments (though as their frequent correspondence attests, neither shied away from criticizing the other's conclusions).

In Copenhagen, Bohr modeled his style of work on Rutherford's, tailoring it to the pursuit of theoretical problems. As in Cambridge, the ideal was to surround yourself with young people and follow their work at an almost daily level while pursuing your own. To that end, Bohr founded the Institute for Theoretical Physics in 1921. Carrying the notion of family even further than Rutherford's lab, it was housed in a single three-story building comprising a lecture hall, a library, work space for the young physicists, a cafeteria, and an apartment on the top floor for Niels and Margrethe Bohr and their children. One of the children who grew up there, Aage Bohr, succeeded his father as director of the Institute for Theoretical Physics and, in 1975, won a physics Nobel of his own.

Out of this institution came the Copenhagen interpretation of quantum mechanics, the set of rules for what is probably the 20th century's greatest physical-science revolution.

Uncle Emilio Heads to Berkeley

While all this was going on, my father, Angelo, was establishing himself as a professor of ancient history. He had heard about the great advances in physics, chiefly while spending three years away from Italy in the early 1920s, one in Vienna and two in Munich. Though he didn't fully comprehend the physicists' achievements, he sensed the excitement surrounding the discoveries and felt, with some regret, that the future belonged with them. Despite his quite respectable career as a historian, I believe there was nothing he admired more than science, and physics in particular.

By primogeniture my father was destined to take over running the paper mill his father owned in Tivoli, a beautiful, ancient city close to Rome. But from an early age he showed no aptitude or inclination for the task. Fortunately, his slightly younger brother, Marco, had both, so my father was free to do something else. Since Italian Jews saw academics as a clear path for advancement, it is not surprising that my father's other brother--my uncle Emilio--became a professor, too. I sometimes think that these two brothers, one born in 1891 and the other in 1905, belonged to two different generations: one never learned to drive a car, and the other was the first in his group of friends to have one. However, I also see how much alike they were, and I realize that despite their differences, each maintained a lively interest in the other's work.

I suspect that my father felt at some level that he had failed twice, first by not running his father's paper mill and second by not becoming a scientist. But perhaps he thought that he could recoup some of his losses by having his children become physicists. And they both did. I don't know what message he gave my brother, but when I was a teenager he pointed me toward that future in no uncertain terms. According to him, theoretical physics was the best possible profession, because "you will be able to tell right from and wrong, and you will not have to talk to anybody you don't want to speak to." I am not so sure he was correct on either count, but I did follow his directive and do not regret it. I am, however, getting ahead of myself.

Though my father was already a physics fan, news from his brother in the late 1920s had clinched his admiration for the subject. Emilio had entered the University of Rome as an engineering student and probably would have continued on this path had his life not changed in early 1927, as he was turning 22. A fellow student, the son of a mathematician, told Emilio that a supposed genius named Enrico Fermi had just been selected, at only 26 (an unheard-of age for a Rome appointment), for a new chair of theoretical physics. Furthermore, since there apparently were no Rome students interested in physics, he was looking for recruits. My uncle and his good friend Edoardo Amaldi, later the leader of post-World War II Italian physics, were the first to respond to the call.

The ensuing exploits of the growing Rome group were remarkable in absolute terms but even more important for Italy, which felt, correctly, that it was lagging behind its northern neighbors in scientific research. Italians are still proud of the group's achievements, and even today, Fermi is regarded as the only true physics genius the country produced in the 20th century. He is also arguably the only 20th-century physicist from any country to have achieved true greatness as both a theorist and an experimentalist.

Fermi's early fame rested on his achievements as a theorist, and perhaps most famously on his explanation of a long-standing mystery: nuclear decays involving the emission of an electron. This phenomenon seemed to violate the bedrock physics principle of conservation of energy. Furthermore, how was it possible that an electron could be emitted from within the nucleus when there presumably were none there to begin with? In late 1933, Fermi was on a skiing vacation with a few members of his group. He convened them in his hotel room and, as my uncle remembers, told them he had solved the problem. This was probably the most important piece of work he had yet done, he said, and might well be the most significant he would ever do. Drawing on an idea of Wolfgang Pauli's, he proceeded to explain his insight, showing them how a new kind of interaction would allow a neutron to decay into a proton, an electron, and a very light particle--not yet observed--that had no electric charge. The last two would escape from the nucleus simultaneously, with the neutral particle carrying away the seemingly missing energy. To distinguish the new particle from the massive neutron (neutronein

Italian), he gave it the name neutrino.

Fermi's most notable work as an experimentalist also began around late 1933, when he realized that the recently discovered neutron provided the means for a new kind of projectile in Rutherford-type experiments. Protons or alpha particles, used heretofore, had to have relatively high energies to penetrate a nucleus, since the electrically charged target and projectile repelled each other. A neutron, on the other hand--even a slow one--could freely make its way into a nucleus, because it was without electric charge. Fermi, his old friend Franco Rasetti, and a few assistants--including, of course, Amaldi and my uncle--quickly began a multiyear study of such reactions, yielding many new and important discoveries about how nuclei behave.

The enterprise came to an end when Italy adopted racial laws in 1938. Fermi, realizing that his family was endangered (his wife, Laura, was Jewish), left for the United States in December of 1938--departing from Stockholm, where he had just received a Nobel Prize in recognition of his pioneering work with neutron scattering. His group at the University of Rome dissolved.

As Fermi was leaving Italy, two German chemists, Otto Hahn and Fritz Strassmann, found a curious result when they bombarded uranium with neutrons. Lise Meitner, a longtime collaborator of Hahn's who had been forced to flee Germany a few months earlier because she was Jewish, helped explain what would prove to be a crucial discovery. During a walk on Christmas Eve of that year, she and her physicist nephew Otto Frisch (family again) realized that uranium nuclei had probably been split into two pieces as they absorbed a neutron, a process that would necessarily lead to a large release of energy. Two weeks later, Frisch coined the term nuclear fission to describe what had happened in the Hahn-Strassmann experiment. It also became clear that if additional neutrons were released during fission, a chain reaction could occur. The first controlled such event, guided by Fermi, took place in a squash court at the University of Chicago in December 1942. Soon after that, Fermi and many others, including my uncle, moved to Los Alamos to work on developing a much bigger chain reaction: the atom bomb.

My family left Italy soon after the Fermis did--in our case, supposedly to visit the 1939 World's Fair in New York. I was then only seven months old. My seven-year-old brother might benefit from the experience, the U.S. consul in Florence politely suggested when we applied for visas, but wasn't I a little young? My father replied that Jewish children were now becoming interested in such events at a very early age. Fortunately, the consul--knowing full well our intention of staying in the United States if at all possible--had a sense of humor and an abundant dose of charity.

Emilio left Italy in the summer of 1938. His exodus took him to Berkeley, CA, a place he had visited in the summer two years earlier. He was gratefully discovering that the physics family was growing rapidly across the Atlantic and welcoming refugees from Europe. Ernest Lawrence's Radiation Laboratory at Berkeley was in the process of replacing Cambridge's Cavendish as the world's great nuclear-physics lab. In some ways Lawrence was just as much a pioneer as Rutherford. One grew up in New Zealand as the child of immigrants and attended local Canterbury College. The other, a grandchild of immigrants, was raised in South Dakota and studied at his state university. Both were forceful and effective leaders in later life, but their aims and styles were different. Rutherford liked inexpensive experiments that could fit on a bench. Lawrence, an enthusiastic fund-raiser and entrepreneur, was interested in building bigger and better cyclotrons, machines capable of accelerating particles to much higher energies than anything that could be achieved at Rutherford's laboratory. In realizing his dream, Lawrence made ample use of America's ingenuity and its new economic power.

Rutherford believed in building the apparatus that the physics required. But Lawrence's philosophy was different: build machines, he thought, and the physics would follow. This was a decisive point in the arc of 20th-century physics. It would no longer be possible for a few individuals to simply set out and collect the necessary tools for an experiment they planned. The era of big physics had begun.

Chadwick had discovered the neutron in February 1932. Two months later John Cockroft and Ernest Walton, encouraged by Rutherford and by George Gamow, a protégé of Bohr's, managed to induce nuclear disintegration by bombarding lithium nuclei with accelerated protons. This would be the old Cavendish Lab's last Nobel Prize-winning physics experiment. Within months, Lawrence had replicated their result with his cyclotron and then quickly moved on. By 1939, the year he was awarded the Nobel for his achievements in developing that apparatus, Lawrence was planning the fourth and largest version--one with a 184-inch chamber and a magnet weighing thousands of tons. Its eventual successor, the Bevatron, began running in 1954. The name came from its ability to accelerate particles at energies up to billions of electron volts, a thousand times greater than those achieved by the first cyclotron. The discovery of the antiproton a year later, thanks to the high-energy collisions the Bevatron made possible, was its first dramatic success (and the reason for Uncle Emilio's Nobel).

Yet Berkeley's physics heyday would soon be over, just as the Cavendish's had passed. Probing the structure of neutrons and protons would require beams of higher energy, and this meant even bigger and more expensive particle accelerators. Since it was becoming increasingly clear that no single institution could afford to construct or staff the new machines, consortiums had already begun forming to plan for building them. A group of universities in the eastern United States joined forces in 1947 to construct an accelerator on Long Island. The result was Brookhaven National Laboratory's Cosmotron, which started running in 1952. Europe's major nations made their own plans, loath to be left behind although World War II had left them impoverished. They banded together in 1954 to found CERN, the European Organization for Nuclear Research (the acronym comes from the French Conseil Européen pour la Recherche Nucléaire), in Geneva, Switzerland. Its first particle accelerator began operation in

1957.

Packing My Bags for Switzerland

These advances were very much on my mind as I decided where to use the two-year postdoctoral fellowship I had been awarded by the National Science Foundation after completing my PhD thesis in 1963. CERN seemed to be the natural choice. A stay there would allow me to see more of my parents and, in some way, to reconnect with the Europe I had left behind. I made the decision without hesitation, even though CERN, having yet to make any major discoveries, seemed to suffer from an inferiority complex vis-à-vis its rivals in the United States. Spirits were nevertheless high there, as I found on arrival. In addition, the laboratory's director general, Victor Weisskopf, was an inspiring presence, familiar to me because he had been a professor at MIT. Weisskopf, who came to CERN in 1961, appeared to represent a melding of the great old European tradition and the new American can-do attitude.

Though only in his mid-50s, Weisskopf had worked with the likes of Bohr and Pauli during the heyday of quantum mechanics and the beginnings of nuclear physics. After arriving as an immigrant to the United States from Europe in his late 20s, he had been an active participant on the atom bomb project and had later helped develop MIT as a center for physics teaching and research. He now seemed to be the right person to guide CERN in its transition to world eminence. Weisskopf also tried to re-create, in a completely different and much larger setting, some of the atmosphere he had benefited from in Copenhagen 25 years earlier. Writing of Bohr in his autobiography, he says, "From the beginning he made the most profound impression on me. He was my intellectual father." And Weisskopf tried to convey some of this same sense of family to the young CERN scientists--speaking informally about physics on Monday afternoons, inviting us to his Geneva house, and always emphasizing what a wonderful enterprise we were engaged in, how lucky we were to be able to work on the great and beautiful problems of physics.

CERN's climb to success was not easy, nor was the United States standing still. With a 1968 ground-breaking for the National Accelerator Laboratory (now renamed Fermilab), U.S. physicists were planning a machine capable of accelerating protons to almost 10 times the energy reached at Brookhaven, a level competitive with anything Europe would achieve. But CERN persevered, and within a decade of my arrival, it announced its first truly major discovery. There is an old adage in physics that "yesterday's discoveries are today's tools and tomorrow's background events." In 1933 physicists were quite sure they would never be able to detect a neutrino being scattered by another particle. By 1973 CERN had a neutrino beam that made it possible to study the details of a newly identified force that acted on these particles and on electrons and protons. This was a breakthrough. It would be another decade before CERN would announce its triumphal sighting of the particle that mediated this force, commonly called the Z boson. Its discovery was another example of the back-and-forth between theory and experiment that has characterized the whole century-long arc. Theorists had predicted that the Z boson would be 90 times as massive as a proton; consequently, it would not be directly observed until machines were capable of reaching the energies necessary for its production. When the Z turned up in 1983, with the predicted mass, the discovery became one of the cornerstones in the establishment of what has come to be called the standard model of particle physics. The long journey begun in 1909 had now reached a summit. The atom's constituents and the nature of all the forces between them finally seemed to have been identified.

By then I had long since left CERN. In the summer of 1965 I began a two-year postdoctoral appointment at Berkeley, still a power in the world of high-energy physics, even if its impact was not quite what it had once been. A side benefit of this stay in California was getting to know my uncle Emilio, now a senior professor at the university. I had seen very little of him while growing up because my father and he always seemed to be at odds. Emilio, never known as an easy person to get along with, described their relationship this way in his autobiography: "My patience and tolerance derived in part from a certain regard I felt for Angelo's keen intellect, and in part because in several respects I felt that I to some extent resembled him." My own view was that, despite Emilio's claims of patience and tolerance with my father, neither of them was a paragon of such virtues. However, spending time with my uncle, now near retirement, was a great window into the evolution of 20th-century physics and, with no sibling rivalry in play, an altogether pleasurable family experience.

My father was a historian who wanted to be a scientist. I now saw Emilio turning to history, in part to examine the scientific events he had witnessed and in part to describe the extraordinary people he had met and sometimes worked with. Within several years he had written From X-Rays to Quarks, an engrossing history of a hundred years of physics as observed by a participant. I read the book (in its original Italian version) when it appeared in 1976, but I was too involved with the day-to-day events of establishing my own career to give it much thought. By then a professor at the University of Pennsylvania and deeply involved in the mysteries of the standard model, I was back at CERN for a year on a Guggenheim fellowship. Europeans were now beginning to talk about building a new kind of particle accelerator: one that would produce very high-energy electron-positron collisions, envisioned as ideal for studying Z meson decay. The Z had of course not yet been observed experimentally, but its discovery was anticipated on theoretical grounds, and planning had to start right away, since it took many years to construct a large accelerator. This was now the way high-energy physics operated: build for the expected and the unexpected. The LEP (Large Electron-Positron Collider) was formally approved in 1981. Construction began in 1983 and finished in a little over five years; by the end of the 1980s, the LEP, also known as the Z Factory, was working marvelously.

The United States now needed to act if it wished to remain competitive. In 1993, Congress canceled the U.S. physics community's response, the Superconducting Super Collider, after a $2 billion initial expenditure. With that move, it was clear that the balance of power was shifting to Europe. Thirty years earlier, I had returned to a Europe envious of America's success in building particle accelerators. It was now

America's turn to be envious. The completion of the Large Hadron Collider has simply underlined this shift, though I do wish to emphasize that the collider's European location does not mean the end of U.S. participation in operating such machines. In an era of global scientific coöperation, one finds people from all countries involved in ensuring the success of experiments at the great accelerators. In fact, the field has rightly claimed to be a model of international collaboration.

The completion of the Large Hadron Collider and the hundredth anniversary of Rutherford's first scattering experiments make this seem like a good time to reflect on the century of incredible changes in this field--where the arc has taken us, where we want to go, and what new challenges loom ahead.

Newton's aphorism "If I have seen a little further, it is by standing on the shoulders of giants" is as true for today's physicists as it was for him. The generation of Einstein, Planck, Curie, Rutherford, and Bohr, our intellectual ancestors, laid the foundations for understanding the atom. Then came the early 20th century's young geniuses, who discovered quantum mechanics, explored the nucleus, and built the new machines. Though the divisions blur, a new generation--my own--emerged in the wake of World War II. We identified the elementary particles and the forces between them, uniting them in the standard model.

This has been a remarkable journey, but great challenges remain. How do the elementary particles acquire mass? Is there some deeper theory explaining the identities of particles and the relations between forces that also encompasses Einstein's grand vision of gravity? Many think that string theory is a giant step in this direction, but conclusive evidence is not yet available. These are all questions for the present generation, now reaching its peak of creativity.

Nor can one omit from the story the realization that conditions fleetingly created by collisions in the highest-energy accelerators mimic those that took place, for a fraction of a microsecond, immediately after the Big Bang that marked the universe's beginning. Because of this development, which has caused a great stir, elementary-particle physics and cosmology now frequently see their aims as parallel. Today's young physicist might go to a conference called "Inner Space/Outer Space" or study a book called From Quarks to the Cosmos.

This convergence has also led to a revival of interest in neutrinos, for they seem to play an important role in our cosmos. They are a subject that has become near and dear to me. We now speak routinely of situations that Fermi, my uncle, and their friends could not have imagined in the winter of 1933. Experiments have shown neutrinos being emitted from the sun's core. We even know that in the explosion following a large star's collapse, neutrinos carry off 99 percent of the emitted energy in a single 10-second burst. This amount is comparable to all the energy radiated by the sun in its 10-billion-year lifetime. Yet three-quarters of a century after the neutrino's existence was proposed, we still don't know its mass. It is much, much less than that of the electron, but how large is it?

I conclude by bringing you up to date on my end of the family business. None of Emilio's children became physicists, but one of his grandsons and a nephew of mine did. My three daughters didn't go into the business, but the oldest married the son of a well-known theoretical physicist. So perhaps a grandchild will carry on the tradition, though I am not sure how much influence the two grandfathers have. The days when parents told their children what careers to pursue are over.

Given all this history, it will probably not surprise you that I married the daughter of a physicist, Herman Hoerlin. I know that when my wife, Bettina, told him about me, he checked me out in American Men and Women of Science before giving his approval. Fortunately, I passed. Finally, I acquired a physicist brother-in-law a little over 15 years ago, when Bettina's sister Duscha met, fell in love with, and soon married an elderly Austrian widower. He was none other than Viki Weisskopf, the idol of my youth at MIT and CERN. At first I was a bit tongue-tied in his presence, but then I realized that it was going to be okay. He was family.

Gino Segrè is a Professor Emeritus of Physics at the University of Pennsylvania and the author of Faust in Copenhagen: A Struggle for the Soul of Physics.