

■ Chapter 1 ■

Elective Affinities

- *The attractions of science* ■ *The glory days of particle physics*
- *Driven by ambitious dreams to Columbia* ■ *Legendary physicists and budding wunderkinder* ■ *Talent versus character, plans versus luck* ■

I expected New York to glitter. Instead, when I arrived on that hot August afternoon in 1966, the city was grimy and littered, disappointingly unmodern. I was jet-lagged and weary, and the sweaty cab ride from Kennedy airport to upper Manhattan tilted me towards depression. The cramped Formica-filled rooms in International House, a graduate student dormitory established by the Rockefeller Foundation on the far reaches of New York's Upper West Side, bore little resemblance to the spacious-looking illustrations in the brochure they had sent to me in South Africa. The sickly green-and-white walls in the corridors and the guards at the back entrance added to the prison sensibility. It took several months before habit obscured all of this ugliness. "I. House," as we all called it, was actually a very good place for foreigners.

A few hours after stepping off the airplane, I descended into a state of acute loneliness. It must have had something to do with the sudden perception of distance and time; I had been away from home many times before, but never this far, and never for so undetermined a period. For weeks, verging on months, I walked around with a lump in my throat that threatened to overwhelm me. This welling-up sensation took a long time to pass, and when it finally did, I missed the

painful intensity that the sadness and longing had brought to my existence. A few years later I read *Young Törless* by Robert Musil, and recognized the adolescent protagonist's piercing and yet delectable unhappiness. The echoes of that first loneliness never totally faded away. Ever since, whenever I've had to start out in a new city alone, I feel again the resonances of those desolate days, at least for a short time.

I spoke to almost no one during those first few weeks in I. House, which was virtually empty in the quiet lull before classes began. Ever cautious, I had arrived three weeks early, compulsively planning to settle down and get acclimated before starting my PhD program in physics. Instead, I felt isolated from everyone I had ever known. It is almost impossible today to be as cut off from any place in the world as I was from Cape Town during that first year in New York. There were almost no telephones in I. House—one extension in a badly soundproofed booth in the corridor served a floor of fifty people. Phone calls to South Africa were expensive and had to be booked in advance through an operator. I never called home; instead, I wrote letters to family and friends several times a week. Finally, mercifully, my first semester at graduate school started.

A blind but avid desire for success in physics spurred me to leave Cape Town; simple chance brought me to Columbia. I had entered the University of Cape Town four years earlier at the age of 16. We were educated in the British style: You had to choose your major area—science, arts, medicine, or commerce—before you began your studies. I chose the natural sciences. In my freshman year I took four separate year-long courses in Physics, Pure Maths, Applied Maths, and Chemistry. There was not much choice of subtopics; you studied everything they chose to teach and then received a grade based on the grand final exam at the end of each year. By my final year I had decided on a joint major in Applied Maths and Theoretical Physics. Foolishly, the school had permitted me to study only theoretical physics from my second undergraduate year onwards, and so I emerged with no experimental skills. It was a premature specialization that no good American university would have tolerated.

In late 1965 I suddenly noticed that the more ambitious students in my class were planning to apply to graduate schools abroad. Serendip-

itously, I stumbled on a path to the United States through a bad case of acne. By coincidence, my sister, a clinical psychologist, had helped my dermatologist's young nephew successfully overcome "poor concentration" ten years earlier. The dermatologist took a benevolent interest in me, and encouraged me to apply to study physics abroad. I took his advice without really understanding what I was embarking on, and began to apply for scholarships to programs in the United Kingdom and the United States. The Cape Town physics department was insularly lukewarm about the benefits of study abroad, but I did not let them dissuade me.

If not for the acne, I might have remained in South Africa. Ever since, I've liked to believe that the course of my life, the old friends I parted from and the new friends I made, my marriage and my children, were the consequence of a random case of acne.¹

Particle physics is the study of the smallest and most fundamental constituents of matter. Even in Cape Town, 5,000 miles from Europe and civilization, we knew that we were in the glory days of the field. As the 1960s passed, each year brought yet another triumph. At accelerators around the world, experimentalists clashed ultrahigh-speed protons against each other like cymbals and discovered a multiplicity of new particles emerging from the collision. Richard P. Feynman once said that doing elementary particle physics is a lot like banging two fine Swiss watches against each other and trying to figure out their workings by examining the debris. That was the challenge.

The proliferation of new particles made it difficult to know which were elementary and which were compound. The mystery was a recapitulation of the great puzzle of nineteenth-century chemistry, when the similar proliferation of new substances provoked the quest to understand chemical structure. That pursuit had culminated in Mendeleev's construction of the periodic table, which arranged all the elements in an understandable order based on their chemical qualities. Empty spots in the table corresponded to as-yet-undiscovered elements whose qualities,

¹The dermatologist's poorly concentrating nephew was Jonathan Dorfán. A few years later he, too, came to graduate school in the United States, where he is now head of the Stanford Linear Accelerator Center, one of the few great global laboratories for experimental particle physics.

associated with their place in the table, suggested how to find them. Now, in the twentieth century, the race was on to find an analogous table for the qualities of so-called elementary particles. So many new ones were being discovered in cosmic rays or man-made colliders that some serious physicists (from California, of course) began to propound holistic sorts of models in which no particle was more elementary than any other and any particle could be considered a composite of all the rest.

In Cape Town in the summer of 1964, we heard popular lectures about the work of physicists Murray Gell-Mann and Yuval Ne'eman, both modern-day Mendeleeyvs, who each invented their own periodic table of particles. Some of the subtables in their system contained eight distinct particles. Gell-Mann dubbed his model the Eightfold Way, a sophisticated and hip allusion to the eight Buddhist principles of living. By looking at the properties of the unpopulated gaps in their table, Gell-Mann and Ne'eman had predicted the observable properties of a very strange new particle called the Omega Minus. Shortly thereafter, exactly as forecast, the particle was created in a collision in the particle accelerator at Brookhaven National Laboratories on Long Island. It was recognized by the characteristic trail it left in a giant bubble chamber, a signature whose properties matched the exact predictions of the Eightfold Way. It seemed you could apprehend the universe with thought.

I became deeply attracted to particle physics and general relativity, subjects that dealt with the ultimate nature of matter, space and time; a life spent studying these topics would be a life devoted to the transcendental. Like many of my physics friends, I began to develop an almost religious passion for fundamental physics. But beneath my passion was an even greater desire for fame and immortality. I dreamed of being another Einstein. I wanted to spend my life focusing on the discovery of truths that would live forever. Sometimes, I felt arrogantly superior to people who were headed for more mundane professions.

My mother encouraged me to devote myself to academic pursuits. My father, though he was more naturally scholarly than my mother, might nevertheless have been happier if I had gone into business with him. I myself would have laughed quite disbelievingly, at age 16, 21, or 34, if someone had told me that I would be working at an investment bank at age 40.

At registration on the first day of my first semester at Columbia, my assigned course advisor was Professor Henry Foley, himself a near-famous physicist who had been part of a classic 1940s experiment that verified Feynman's Nobel Prize-winning theory of electrons. Foley, a charmingly cynical man, quizzed me briefly about my knowledge of atomic physics and discovered how little I had learned in Cape Town about the details of the spin-orbit interactions of electrons. Then he commanded me to register for G4015, the introductory Columbia graduate course in atomic physics and quantum mechanics.² Most physics majors at American schools had taken the equivalent subject in their junior or senior year of college, so here I was starting off a year or more behind the rest of the pack. It was a disheartening setback, the beginning of three long, tedious, and unexpected years of coursework and examinations at a time when I had expected to soon embark on original research.

Foley was right, though—I didn't know enough. In Cape Town in the early 1960s we had learned a shallow rudimentary version of modern physics and quantum mechanics. The physics professors there, for the most part, seemed uncomfortable with everything that had developed after 1930. Their attitude—that you were lucky if you ever got to *really* understand quantum mechanics—stayed with me a long time. Physics in the United States was much more professional, hard-nosed, and businesslike. Columbia's physics department, I saw over and over again, didn't think of modern physics as something esoterically advanced and difficult, to be revealed to you only when you crossed some threshold and finally became an initiate. They expected you simply to plunge right in.

The one subject I had learned really well as an undergraduate was Applied Mathematics, a slower-moving subject easier to keep abreast of in distant, isolated South Africa. In Cape Town, the closed-book, year-end exams were fashioned after the famous Cambridge Tripos examination on which many of the British-educated faculty had been reared.

²You may think me pedantic to list the actual course number. But even now, more than thirty years later, each prosaic course number still conjures up a vivid subworld of a certain year, a particular classroom, a specific professor, a sliding chalkboard, and a noisily clanking steam radiator, together with the exciting sensation of being on the threshold of mastering some new and arcane alchemical subject.

Rapid, practical problem solving as well as memorization were heavily emphasized. Everything was done thoroughly. In each successive year we were taught progressively more advanced versions of classical mechanics and electromagnetic theory. I can still recite some of the indefinite integrals and Fourier transforms we had to learn by heart in order to take the final exams.³

The physics department I entered at Columbia in 1966 was legendary. The first thing that struck me was their direct connection to so many groundbreaking episodes of twentieth-century physics. The recipient of the first PhD degree ever awarded by the department, at the start of the century, had been R. A. Millikan. Later he received the Nobel Prize for his precise measurements of the invisible electron's charge by ingeniously measuring the deflection of tiny oil drops carrying an unseen electron or two's worth of static electricity.

When I arrived, I. I. Rabi, the grand old man of physics in the United States after Oppenheimer's death, was nearing the end of his reign over the Columbia department. He had received the Nobel Prize in 1944 for finding a method of measuring the magnetic properties of nuclei. Rabi was the intellectual father of a whole generation of American physicists, a respected government advisor, and one of the creators of the Brookhaven National Laboratory, where Gell-Mann and Ne'eman's Omega Minus particle was finally discovered. Now near retirement and seemingly garrulous, he struck me as more comic than genius. But I was young and a little arrogant then, and I had no conception of his wisdom and influence. Recently, I saw his old quote that "If you decide you don't have to get A's, you can learn an enormous amount in college."

The late Enrico Fermi, a 1938 Nobel Prize winner, was regarded as the spiritual father of the Columbia physics department. His black-and-

³During my last few years at Goldman, Sachs, I interviewed undergraduates applying for jobs in investment banking, and I was often surprised at how little of their coursework some of them recalled, how little a sense they had of the essence of their field. I met juniors majoring in statistics who couldn't define standard deviation and students who had taken several courses in electromagnetic theory but couldn't remember Maxwell's equations. What I had learned I had learned well. Theirs sometimes seemed a wasted education.

white three-quarter profile photograph graced the seminar room on the eighth floor of the Pupin Physics Building; he had been on the faculty there during World War II and the Manhattan Project. Fermi was the experimentalist who had created the first self-sustaining nuclear reaction at the University of Chicago, a step on the way to the bombs that leveled Hiroshima and Nagasaki. Amazingly, he was also the theorist who, in the 1930s, had predicted the existence of the neutrino, a massless and chargeless particle that interacted so weakly with ordinary matter that it wasn't detected until some twenty years later. He was one of the last physicists to make major contributions to both theory and experiment, an eclectic Goethe of the field.

Columbia had also been the wartime home of the beautiful Maria Goeppert-Mayer, who later received the 1963 Nobel Prize for her theoretical proposal that the nucleus of an atom, like the atom itself, consisted of shells of orbiting particles. Because of Columbia's antinepotism laws—her husband Joseph Mayer was a professor of chemistry—she had been only a member of the research staff of the university, and not a full faculty member.

Closer to the present, Columbia had been at the center of the post-war development of relativistic quantum electrodynamics (QED), the fabulously accurate theory of how electrons emit and absorb light that I would soon struggle to learn. Atoms and the electrons inside them are so small that physicists can only indirectly examine their structure. You cannot actually “see” the inside of an atom; instead, in much the same way that doctors used to tap a patient's chest and listen to the quality of the sound emitted in order to figure out the state of the patient's insides, so physicists must poke at an atom and then diagnose the character of its internal electrons from the light they emit. Until the late 1940s, QED was riven by such deep mathematical and conceptual inconsistencies that, in many cases, calculations of the frequencies of emitted light led to literally infinite results.

In the late 1940s, Feynman and Julian Schwinger in the United States (and, unknown to them, Shin-Ichiro Tomonaga in Japan), in a *tour de force* of insight and mathematical prowess, showed how to mend the theory of QED. They were then able to predict correctly minuscule and previously unsuspected corrections to the wavelengths of the light emitted by the internal electrons as they jumped from one orbit in the atom to another.

Willis Lamb and Polykarp Kusch, both at Columbia in the late 1940s, had carefully and accurately measured a variety of these almost infinitesimal corrections, and they found perfect agreement with Feynman and Schwinger. Lamb and Kusch each received a Nobel Prize, as did Feynman, Schwinger, and Tomonaga a while later.

It didn't take me long to learn that not all Nobel Prizes are equal. In 1968, when I was the teaching assistant for Kusch's junior-level course on electromagnetic theory, I met with him regularly. Soon I began to notice that people in Pupin treated him as though his Prize was somehow worthy of less respect than those of the other faculty members. A few years later he left for the University of Texas.

Also at Columbia, but not yet Nobelists at that time, were Leon Lederman, Jack Steinberger, and Mel Schwartz, all of them renowned even then for a host of elegant experiments and discoveries. In 1988 they received their Nobel Prize for having shown, almost thirty years earlier, that there were not one, but two different types of the neutrino that Fermi had proposed. (The discovery of a third type of neutrino in 2000 was less astonishing and definitely not Nobel-worthy.)

Finally, fiercely brightest among all the stars in the Columbia firmament was Tsung-Dao Lee, the embodiment and perhaps even the cause of all the good and bad qualities of the department. He had won his Nobel Prize in 1957, at the age of 31, for theoretical investigations that led to the startling discovery of the so-called nonconservation of parity. Lee and his fellow Prize-winner C. N. Yang had intrepidly suggested that nature's laws were not symmetric with respect to the seemingly arbitrary human definitions of "left" and "right." It was an almost unbelievable hypothesis, but they proposed experiments to test it. In less than a year they were proved correct. When I arrived at Columbia only eight years later, the consequences of this discovery were still working their way through the framework of physics.

"T. D.," as everyone called him, was Pupin's version of the Pope and the Last Emperor of China rolled into one. He was a holy terror, self-centered and intense. About ten years ago I saw a photograph of him in the literary magazine *Grand Street*, taken as part of a series of photographs of scientists writing on blackboards. One was of Feynman, lively and jovial, lecturing on QED. Another featured Mitchell Feigenbaum of the Rockefeller University, examining his doubling equations that

revealed the hidden order behind apparently chaotic phenomena. Most of the physicists looked prosaic, even Gell-Mann himself. But T. D.'s photo was different. Taken in the 1950s, it showed his fervent young face glowing with light as he spoke, for all the world looking like Moses descending from Sinai. T. D. set the tone at Columbia. His presence could inspire, but it could consume, too.

The faculty were not the only extraordinary beings at Columbia. Many of the students seemed to be *wunderkinder*, too. My graduate classes, even the advanced ones, always contained a smattering of precocious smart-aleck American undergraduates. I was envious and wary of them. Some, sporting crewcuts and narrow-shouldered dark suits with ties, were relics of the Fifties; others had lank, long, hair and dressed in faded jeans and sweatshirts. But whatever they wore, they all raised their hands in class to ask questions whose answers they already knew.

I was awed by these people who knew more than they had been taught. In South Africa I had mastered only a limited number of skills really well, and that knowledge lasted a lifetime. There, I had waited obediently year after year to get to the level at which “they” would begin to teach “me” the things I was able to handle. It had never occurred to me that I could learn what I wanted when I chose. In America, I was alarmed to see students who set about learning things on their own. I’m still embarrassed to admit to myself that I almost never studied anything I wasn’t officially taught. I recall one major exception. In my fourth year of college I spent many months studying unified field theories of gravitation and electromagnetism for my honors’ thesis. My independent investigation of the extension of Einstein’s theory of gravitation exhilarated me, but this autonomy was an exception.

In 1966 and during subsequent years, I dreamed ambitious dreams about success on the scale of T. D. Lee. By this unrealistic measure, few of the *wunderkinder* fulfilled the full magnitude of their intimidating promise. One became a think-tank military analyst whom I was pleased to recognize on television during the Gulf War following the Iraqi invasion of Kuwait. Another completed a PhD in physics, moved to medical school, began a residency in psychiatry, and finally became a

well-known neural-net theorist. A third, after winning the prize for the best physics undergraduate at Columbia, struggled with manic depression. Determined to keep studying, he would keep a running daily log on his canary-yellow legal notepad of the exact number of minutes he had spent actually working at full concentration. Each time he paused or took a break, he stopped the clock and he wrote down the number of minutes he had worked since the last interruption. At the end of the day he computed the total. Compulsive myself, I was sympathetic to his counting; I knew how few were the hours in the day one actually works seriously and undistractedly, and was momentarily tempted to start my own time sheet.

I learned one lesson from the fates of both the professors and students I met at Columbia: In the end, character and chance counted at least as much as talent. Luck, combined with what my mother called *sitzfleisch*, the capacity to persevere, played an overwhelming role.

First in Cape Town and then in New York, I had been steadily learning what kind of physics suited me.

Like most physicists, I was a reductionist: I believed that you can explain complex things by reducing them to their constituents. Biology depends on chemistry; chemistry is merely the physics of molecules and atoms; atoms are made out of electrons and nuclei; nuclei contain protons and neutrons, and protons and neutrons seem to be made of quarks. What are the ultimate subnuclear particles at the putative root of this hierarchy, and what are the laws that determine their behavior? These questions are the domain of particle physics.

Particle physicists are snobs who think that their field is the source of the most fundamental knowledge, and take some mischievous pleasure in denigrating other messier or more complex areas of physics. Gell-Mann, the codiscoverer of the Eightfold Way and the discoverer of quarks, succinctly summarized the latent prejudice of most particle physicists about the superiority of their enterprise when he famously referred to solid state physics, the apparently more mundane study of bulk matter and its variety of forms, as “squalid state physics.”

Nowadays, not everyone agrees with Gell-Mann’s clever *bon mot*. Over the last twenty years physicists have discovered a deep commonality between large-scale bulk matter and small-scale particle physics.

Much of what is new and interesting in both fields seems to emerge from what is called their “many-body” nature: Both bulk matter and tiny particles can each be viewed as resembling a medium, each made out of a very large number of similar constituents. When many similar constituents are clumped together, their collective behavior can display completely new and unexpected characteristics. A drop of water can suddenly freeze and turn solid in a way that no single water molecule can. A ripple of excitement or a hush of expectation can sweep over a crowd but not over a single individual. In the words of another Nobel Prize-Winner, P.W. Anderson, “More is Different!” He, and many other “squalid-state” physicists believe that there is no single grand reductionist Theory of Everything.

It is unlikely one will ever know who is right, but, like most aspiring physicists of the postwar period, I was immensely attracted by the reductionist point of view. I wanted to be the ultimate reductionist, a particle physicist.

Technically, I still had to choose between being a theorist or an experimentalist, but for me, this wasn’t much of a choice. The essence of theoretical physics is the attempt to look at the universe, and then mentally apprehend its structure. If you are right, you emulate Newton and Einstein: You find one of the Ten Commandments. You write down a simple set of laws that, plucked from nowhere, miraculously describes and predicts how God’s world works. This was the struggle to which I aspired. Anything else would have been a compromise that I was not prepared to make.

Even within theoretical particle physics there are further refinements. Pure theory is the search for abstract laws, for a formulation of the divine commandments that rule the world. But, for every Moses descending from the mountain with a valid new law, there are countless well-intentioned prophets whose proposed laws turn out to be wrong. So how does one tell when a theory is right?

Beauty, even mathematical beauty, is not enough. Physicists must test a new theory by elaborating the ways in which it manifests itself in the world. Physicists who do so-called phenomenology work out the detailed and observable consequences of a theory, providing the practical link between principles and experiment, between mind and matter. Phenomenologists elaborate the theory; they create heuristic approximations to engineer the theory into a pragmatic tool; they propose

experiments to validate or refute a theory, using the theory itself to compute the expected results. Phenomenologists deal a little more with the ripples on the surface and a little less with the laws beneath it.

Though I wanted to do pure theory, I ultimately ended up spending much of my life in physics as a phenomenologist. Over the long run, this stood me in very good stead. When I moved to Wall Street, I found quantitative finance to resemble phenomenology much more than it resembled pure theory. Quantitative finance is concerned with the techniques that people use to value financial contracts and, given the fluctuations of the human psyche, it is a pragmatic study of surfaces rather than a principled study of depths. Physics, in contrast, is concerned with God's canons, which seem to be more easily captured in the simple broad statements that characterize profound physical laws.

I had a passion for the content of physics, but I was also possessed by a hungry ambition for its earthly rewards. Both passion and hunger persisted over the years, despite the inevitable disappointments. Ten years later, as a postdoctoral researcher at Oxford in 1976, I experienced a minor epiphany about ambition's degradation. At age 16 or 17, I had wanted to be another Einstein; at 21, I would have been happy to be another Feynman; at 24, a future T. D. Lee would have sufficed. By 1976, sharing an office with other postdoctoral researchers at Oxford, I realized that I had reached the point where I merely envied the postdoc in the office next door because he had been invited to give a seminar in France. In much the same way, by a process options theorists call time decay, financial stock options lose their potential as they approach their own expiration.