QUANTUM PROFILES

SECOND EDITION

JEREMY BERNSTEIN

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Second Edition

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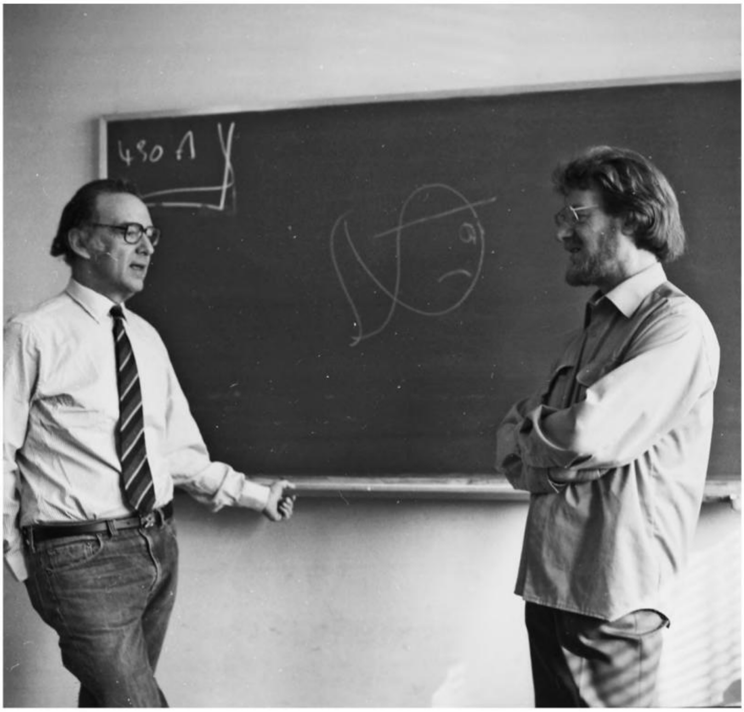
Foreword

Three of the profiles in this second edition appeared in modified form in the 1991 edition of this book. The John Wheeler profile was done at the request of the *Princeton Alumni Weekly* and appeared in a much shorter version in that publication on October 9, 1985. A profile of Besso appeared in a modified version in the *New Yorker* on February 27, 1989. I wrote the profile of John Stewart Bell with the *New Yorker* in mind, but it was not published there, and I took this opportunity to rewrite it. Most of the rest of this book has never been published.

Quantum Profiles

1
John Stewart Bell



John Stewart Bell and the author.
I would like to thank Tullio Basaglia of CERN for supplying the photo.

In 1902, the Olympia Academy was founded in Bern, Switzerland. It had three members: Maurice Solovine, Conrad Habicht, and Albert Einstein. Solovine, a young student, had answered a newspaper advertisement offering private tutoring in physics by Einstein for three Swiss francs an hour, while Habicht, who was studying mathematics with the idea of becoming a secondary-school teacher, already knew Einstein. The three young men had regular evening "Academy" meetings over the next three years, at which they studied philosophy and discussed physics. Solovine recalled having eaten caviar in his parents' home in Romania, and on one of Einstein's birthdays, he and Habicht treated Einstein to some expensive caviar, which he had never tasted. As luck would have it, that was the evening Einstein was scheduled to talk to the "Academy" about Galileo Galilei's principle of inertia. He became so absorbed that he ate all the caviar without realizing what he was eating.

In 1905, the group broke up when Habicht and Solovine left Bern. However, they began corresponding almost at once, and in one of his earliest letters to Habicht, written in the spring of 1905, Einstein described his program of research for the year in the sort of cheerful tone one might use to describe a little light reading. It was, however, this research program that laid the foundations of twentieth-century physics. He wrote:

> I promise you four papers . . . the first . . . deals with radiation and energy characteristics of light and is very revolutionary. . . . The second work is a determination of the true size of the atom from the diffusion and viscosity of dilute solutions of neutral substances. The third proves that assuming the molecular theory of heat, bodies whose dimensions are of the order of 1/1000 mm, and are suspended in fluids, should experience measurable disordered motion, which is produced by thermal motion. It is the motion of small inert particles that has been observed by physiologists, and called by them "Brown's molecular motion." The fourth paper exists in first draft and is an electrodynamics of moving bodies employing a modification of the doctrine of space and time; the purely kinematical part of this work will certainly interest you.

What is striking about this list—apart from the fact that it was compiled by a then totally unknown twenty-six-year-old physicist—is that the first paper announces, it turns out, the invention of the quantum, while the last paper announces the invention of the theory of relativity, and of the two, it is only the former that is in the young Einstein's view "revolutionary."

The theory of the light quantum, which Einstein initiated in 1905 and which is with us still, is certainly the most revolutionary development in the history of physics and arguably in the history of science. The creators of the theory, men such as Einstein, Niels Bohr, Werner Heisenberg, Erwin Schrödinger, Wolfgang Pauli, and Paul Dirac, were often struck by the apparent "absurdity"—the utterly noncommonsensical aspects—of the world depicted by quantum theory. Long after he had done his seminal work, Heisenberg recalled that

> an intensive study of all questions concerning the interpretation of quantum theory in Copenhagen finally led to a complete and, as many physicists believe, satisfactory clarification of the situation. But it was not a solution which one could easily accept. I remember discussions with Bohr which went through many hours till very late at night and ended almost in despair; and when at the end of the discussion I went alone for a walk in the neighboring park I repeated to myself again and again the question: Can nature possibly be as absurd as it seemed to us in these atomic experiments?

Unlike some of the other great intellectual revolutions of the twentieth century—in art, music, literature—until recently, at least, this one was not widely known, let alone understood, by the general public. Many physicists realized this, and a few of them tried to do something about it. For example, in 1953, J. Robert Oppenheimer delivered a very successful series of Reith Lectures over the BBC about subatomic physics. In one of them, he characterized the epoch of the discovery of quantum theory as follows: "It was a time of earnest correspondence and hurried conferences, of debate, criticism, and brilliant mathematical improvisation." And then he added, with a touch of the baroque eloquence of which he was a master, "For those who participated, it was a time of creation; there was terror as well as exaltation in their new insight. It will probably not be recorded very completely as history. As history, its recreation would call for an art as high as the story of Oedipus or the story of Cromwell, yet in a realm of action so remote from our common experience that it is unlikely to be known to any poet or any historian."

Oppenheimer died in 1967, and while there has not yet arisen "an art as high as the story of Oedipus," in the last few years, quantum theory has, much to the surprise of most physicists, entered into the popular culture. Fiction writers now cite, rightly or wrongly, the Heisenberg uncertainty principle as a basis for theories of literature. A *New York Times* book review said approvingly of a novelist that "she knows enough about Heisenberg to realize that the act of observation alters the object observed; or in literary terms, telling the story alters the story being told." Tom Stoppard's play *Hapgood* turns on the uncertainty principle; the printed text is preceded by a quote on the quantum theory of Richard Feynman. Even otherwise sober and non-science-oriented magazines such as the *Economist* have felt an obligation to alert their readers that something odd has happened in physics. In a feature article in its January 7, 1989, issue titled "The Queerness of Quanta," the

Economist notes that "many of this century's most familiar technologies come with an odd intellectual price on their heads. The equations of quantum mechanics explain the behaviour of sub-atomic particles in nuclear reactors and of electrons in computers and television tubes, the movement of laser light in fibre-optic cables and much else. Yet quantum mechanics itself appears absurd." Among the "absurdities," the following are offered: "There are no such things as 'things.' Objects are ghostly, with no definite properties (such as position or mass) until they are measured. The properties exist in a twilight state of 'super-position' until then."

"All particles are waves, and waves are particles, appearing as one sort or another depending on what sort of measurement is being performed."

Then, "A particle moving between two points travels all possible paths between them simultaneously."

And finally, "Particles that are millions of miles apart can affect each other instantaneously."

While most physicists would find these capsule descriptions of the quantum theory caricatural, there is enough truth in them to explain why people who seem to have an aversion to more conventional science are drawn to quantum theory. Quantum theory has become the basis of the New Age outlook, with its emphasis on Eastern religions and holistic medicine. This surely would have astonished Oppenheimer, who, incidentally, studied Sanskrit so that he could read the Upanishads in the original. Books such as Gary Zukav's *The Dancing Wu Li Masters*—quantum theory with a dash of Eastern mysticism—abound, and no ashram, at least no Western one, can afford to be without its resident expert. In a health-food store in Greenwich Village, I came across an announcement in the *I Am News* of the Ananda Ashram in Monroe, New York, which, under the heading "Quantum Dynamics," read, "Spiritual Purification Program: Includes meditation, fire ceremony, rebirthing, sweat lodge and Quantum Dynamics initiation for those who haven't had it; includes breathing techniques and mantra to dissolve upsets. This weekend we will work with Quantum Dynamics to dissolve past life karma all the way back to original cause."

Pace Robert Oppenheimer

Although it is always somewhat dangerous to look for the cause of a complex sociological phenomenon in a single event, nonetheless, I believe that a case can be made for the proposition that the present widespread interest in quantum theory can be traced to a single paper with the nontransparent title "On the Einstein-Podolsky-Rosen Paradox," which was written in 1964 by the then-thirty-four-year-old Irish physicist John Stewart Bell. It was published in the obscure journal *Physics,* which expired after a few issues. Bell's paper was, as it happens, published in its first issue. Bell, who began work in 1960 at CERN, the gigantic elementary-particle physics laboratory near Geneva, used to claim that his paper involved only the use of "high school mathematics"; however, its six pages are dense with an extremely abstract set of arguments, which even professionals in the field must work hard to understand. In fact, for several years after its publication, few, if any, professional physicists bothered to try.

This changed dramatically in 1969, when it was realized that "Bell's theorem" (or "Bell's inequality," as it is often called) could actually be tested in the laboratory. What was at stake in such a test was nothing less than the meaning and validity of quantum theory. If Bell's inequality was satisfied, it would mean that all of Einstein's intuitions about the essential incompleteness of quantum theory had been right all along. If the inequality was violated, it would mean—at least, many physicists believe—that Bohr and Heisenberg had been right all along and that no return to classical physics was possible. By the early 1970s, such experiments were actually being carried out. They still are. With a few exceptions—glitches, one thinks—all these experiments show that Einstein was wrong. It was these experimental results that caused a new generation of physicists to confront just how peculiar and counterintuitive quantum mechanics really is. It is this realization that is being reflected in the growing popular interest in the theory.

I knew John Bell for more than thirty years. When his tenure had just begun at CERN, in 1960, I had begun a series of visits to the laboratory. During that time, I talked to Bell about many things but very little either about his life or about his work in quantum mechanics. I also got to know his wife, Mary, who was also a physicist at CERN. The Bells, while charming company when one got to know them, tended to be very private people, keeping pretty much to themselves. "Mary and I are rather unsociable," Bell once remarked to me in his lilting Irish brogue. ("Mathematics" is pronounced something like "mah-thah-mahtics," and "now" comes out sounding something like "nae." Mary Bell is Scottish and has a fine burr. "Girl" sounds something like "gurrrle.") Bell had a dry wit, and one had to pay attention when he spoke to see that he was not teasing. The Bells had no children but gave the impression of taking great and constant pleasure in each other's company.

I am quite sure that Bell would have been most willing to discuss his ideas about quantum theory if I had asked, but somehow I never did. I had, in fact, never tried to read Bell's 1964 article. But with all the mounting interest in the subject, I decided that I had been missing out on something and that I would educate myself. This process was aided no end when Bell sent me a copy of his book—his collected papers on quantum theory, published in 1987 under the title *Speakable and Unspeakable in Quantum Mechanics.* It contains twenty-two essays, including his 1964 paper. Some of the essays are addressed to the educated layperson, including a celebrated one written in 1981 and with the unlikely title "Bertlmann's Socks and the Nature of Reality." (Reinhold Bertlmann is a real person.) I quote the opening paragraph because it is illustrative of Bell's style:

> The philosopher in the street, who has not suffered a course in quantum mechanics, is quite unimpressed by Einstein-Podolsky-Rosen correlations. He can point out many examples of similar correlations in everyday life. The case of Bertlmann's socks is often cited. Dr. Bertlmann likes to wear socks of different colours. Which sock he will have on a given foot on a given day is quite unpredictable. But when you see that the first sock is pink you can be already sure that the second sock will not be pink. Observation of the first, and experience of Bertlmann gives immediate information about the second. There is no accounting for tastes, but apart from that there is no mystery here. And is not the [Einstein-Podolsky-Rosen] business just the same?

That it is not—a fact that brings us deep into the mysteries of quantum theory—is the subject of the rest of this chapter and most of the other chapters in this book.

Bell always talked over his ideas with Mary, and at the end of the preface, he wrote: "In the individual papers I have thanked many colleagues for their help. But here I renew very especially my warm thanks to Mary Bell. When I look through these papers again I see her everywhere."

Having studied the collection and an equally rewarding one titled *Quantum Theory and Measurement,* edited by physicists John Wheeler and Wojciech Zurek, I felt that I finally understood enough about the subject to at least carry on a sensible dialogue with Bell. I thought I would also use the opportunity to find out something about his and Mary's lives. The odd twists of fate that determine the lives of young scientists have always fascinated me, and I could not imagine what circumstances had brought the Bells from Ireland and Scotland to Geneva.

Bell had a very busy schedule, but we finally agreed upon a week in January. For a skier, this is an especially happy time to visit CERN, which is less than a half hour from the ski runs in the Jura. I did not think there would be much chance to persuade the Bells to go skiing on their lunch hours, since I knew that both of them had given up downhill skiing in favor of cross-country some years earlier. They owned a modest apartment in Champéry, a ski resort not far from Geneva, where they could cross-country ski in peace. But I thought I would spend alternate lunch hours downhill skiing in the Jura and eating in one of the CERN cafeterias with the Bells. As the plane landed in Geneva, I saw that the Jura was brown—hardly a trace of snow. It later turned out that there was no skiing at all.

An informal tradition had developed at the laboratory, according to which "aristocrats" like theoreticians ate lunch late. one o'clock or so, while the more "plebeian" members of staff ate earlier. The Bells traditionally had lunch promptly at 11:45 and ate with the same small group consisting of, among others, a Dutch computer expert and a Norwegian in charge of laboratory safety. Both Bells were vegetarians, Mary since childhood and John since the age of sixteen. Since there was, at least in the past, no particular provision for vegetarian diets in the CERN cafeterias, Mary Bell would usually arrive at lunch with a satchel of fresh vegetables, which she and John would share. There is also the very pleasant tradition at CERN of after-lunch espresso in the large lounge next to the main cafeteria or outside on the patio, from which one has a view of Mont Blanc, if the weather is nice. Gossip after lunch is another tradition at CERN.

CERN is located on the French border, a few miles from Geneva. When I first went there, it was entirely in Switzerland. As the giant elementary-particle accelerators that are the main business of CERN got larger and larger, the terrain of the laboratory spilled over into France. The tunnels that contain the evacuated pipes in which the beams of elementary particles run cross the border in several places. At the time of my visit in 1989, it had reached a kind of apotheosis in an accelerator called the LEP (Large Electron-Positron Collider), whose construction was begun in 1981. By the time it was turned on in the summer of 1989, it had cost about a billion Swiss francs (nearly three-quarters of a billion US dollars), which was provided by the thirteen European member states that ran CERN at the time (that number is now twenty-three). The LEP tunnel had a radius of twenty-seven kilometers. The tunnel contained electrons and their antiparticles, positrons, streaming in opposite directions. From time to time, the beams collide, and the results were then the highest-energy electron and positron collisions ever observed.

Because of all this activity, the character of the laboratory had changed dramatically from what I remember of my first visit nearly thirty years earlier. This was not so long after World War II, and the European community had not yet rebuilt its scientific establishment; indeed, CERN was conceived to accelerate that process. Those days are long past. The Large Hadron Collider superseded the electron-positron collider. It was turned on in 2008 and among other things was responsible for the discovery of the Higgs boson.

While there was an enormous amount of construction at CERN by the time of my visit in 1989, the theoreticians still occupied part of the same compact-looking four-story building that they were in when I first went there. I had no trouble finding Bell's office, the same one I had been stopping in, on and off, for several decades. Like the rest of the laboratory, the theoretical division—TH, as it is called—had undergone an almost exponential expansion. When I had first been there, the entire division, including visitors like myself, consisted of something like thirty people. In 1989, there were about a hundred and forty.

The signs on Bell's door read "J. Bell" and "M. Bell." I knocked and was invited in by Bell. He looked about the same as he had the last time, a couple of years earlier. He had long, neatly combed red hair and a pointed beard, which made him a somewhat Shavian figure. On one wall of the office was a photograph of Bell with something that looked like a halo behind his head, and his expression in the photograph was mischievous. Theoretical physicists' offices run the gamut from chaotic clutter to obsessive neatness; the Bells' were somewhere in between. Bell invited me to sit down after warning me that the "visitor's chair" tilted backward at unexpected angles. When I had mastered it and had a chance to look around, the first thing that struck me was the absence of Mary.

"Mary," said Bell, with a note of some disbelief in his voice, "has *retired*." This, it turned out, had occurred not long before my visit. "She will not look at any mathematics now. I hope she comes back," he went on almost plaintively. "I need her. We are doing several problems together."

In recent years, the Bells had been studying new quantum-mechanical effects that would become relevant for the generation of particle accelerators that succeeded the LEP. John had begun his career as a professional physicist by designing accelerators, and Mary had spent her entire career in accelerator design. A few years earlier, John Bell, like the rest of the members of the CERN theory division, had been asked to list his physics specialty. Among the more "conventional" entries in the division, such as "superstrings," "weak interactions," "cosmology," and the like, Bell's read "quantum engineering."

Bell thought we would be more comfortable in our discussions if we found a larger office. It turned out that the one next to his was temporarily free. Mary, he told me, would be glad to talk to me over the phone if I had something I wanted to ask her. We settled into the new office, and I asked if he would mind telling me a bit about his early life and how he got into physics.

"I was born on July 28, 1928, in Belfast," he began. "My parents were poor but honest. Both of them came from the large families of eight or nine that were traditional of the working-class people of Ireland at that time. Both sides of the family have been in Northern Ireland for many generations. But we are from the Protestant tribe—the British side—so the real Irish people regard us as colonists."

I was curious, as I always am, about whether there had been any scientific or academic tradition in his family. He thought a moment and replied, "As far as I know, until the present generation, there was none. The kind of professions I had heard about in the family were carpenters, blacksmiths, laborers, farmworkers, and horse dealers. My father's first profession was horse dealer. He stopped going to school at the age of eight—his parents paid fines from time to time for that. He learned how to buy and sell horses instead. The nearest I heard of anyone in my family being educated was my mother's half brother. He was a village blacksmith, but he taught himself something about electricity at a time when not many people knew about electricity. I was the only one of my siblings who reached high school. I have an older sister and two younger brothers, and they left school at about the age of fourteen. The normal thing would have been for me to get a job when I reached fourteen."

Encouraged by his mother, Bell applied for financial help to go to secondary school. At the time, there was no universal system of free secondary education in Britain. That would come a bit later with the Labor government. "I sat many examinations," Bell recalled, "for the more prestigious secondary schools, hoping for scholarships, but I didn't win any." Some money did appear—Bell was not sure from where—so that he was able to attend the Belfast Technical High School, the least expensive. Bell remembered it with great affection. He did courses in bricklaying, carpentry, and bookkeeping, along with the more conventional curriculum.

Unlike many prominent theoretical physicists I have asked, Bell does not have any early memories of scientific or mathematical precocity. He did, however, recall that at the age of fourteen, he began a brief phase of reading Greek philosophers. "I was a bookish sort of child, much in the local public libraries. I was hostile to the idea of sports," he said, laughing. "I regret it now. I've grown up to be a seven-stone weakling. I was, you know, brought up in the Church of Ireland. I was even confirmed by a bishop. But in my adolescent years, I began to wonder if it was really true what they told you. Does God exist? Questions like that. So, like many children, I started looking for answers. One place to look, I thought, was philosophy. I was reading thick books on Greek philosophy. But very soon I became disillusioned with philosophy. I found that the business of the 'good' philosophers seemed mainly to refute the 'bad' philosophers. There didn't seem to be much else. The next-best thing seemed to be physics. Although physics does not address itself to the 'biggest' questions, still it does try to find out what the world is like. And it progresses. One generation builds on the work of another instead of simply overturning it. In my secondary school, I was already beginning to get some idea that nature respects laws, like Newton's laws of motion. I remember a big disappointment when we started our course in Newtonian mechanics. It was in a room with models of steam engines all around. The teacher said, 'Next time, we are going to play with the machines.' I thought he meant the steam engines. But it turned out that he meant things like levers. It was a great disappointment."

Because of the cost, the only university Bell could have considered going to after graduating from high school was Queen's University in Belfast. But Bell had graduated from high school at sixteen, and the university would not admit anyone before the age of seventeen. So Bell looked for work. "I applied to be office boy in a small factory," he recalled, "some starting job at the BBC—things like that. But I didn't get any of the jobs I applied for. One told me that I was overqualified; another didn't tell me anything. It may be that I was resisting presenting myself as if I really wanted a job. I really wanted to continue on to the university, and the job I finally did get was in the university." It turned out that a laboratory assistant was needed in the physics department, and Bell got that job. "It was a tremendous thing for me," he said, "because there I met, already, my future professors. They were very kind to me. They gave me books to read, and in fact, I did the first year of my college physics when I was cleaning out the lab and setting out the wires for the students."

Among the professors who were especially helpful to him that year, Bell remembered Karl Emeleus and Peter Paul Ewald. Emeleus gave him two books to read: the classic freshman physics text *Mechanics, Molecular Physics, Heat and Sound* by Millikan, Roller, and Watson and an odd Victorian text on electricity and magnetism titled *Elements of Electricity* by the British physicist J. J. Thomson. It was Thomson who in 1897 had identified the first subatomic particle, the electron. He measured its charge and mass, and for this work he was awarded the Nobel Prize in 1906. His work seemed to show that the electron was a particle, a motelike billiard ball. Thomson was still alive when his son, G. P. Thomson, shared the 1937 Nobel Prize with C. Davisson for their independent experimental discoveries in the late 1920s that the electron can also act like a wave, one of the mysteries of the quantum theory. In any event, Bell found the senior Thomson's book exceedingly difficult. While its donor, Professor Emeleus, was a rather formal character, Ewald, a very distinguished crystallographer who, in Bell's words, "had been washed up on the shores of Ireland after the Nazis forced him to emigrate from Germany," was just the opposite. "He'd discuss anything," Bell noted. "He even declared that one of his assistants was mad."

By the time Bell graduated from Queen's University in 1949, he had decided to make a career in theoretical physics. Ewald suggested that he continue his studies in graduate school with Rudolf—later Sir Rudolf—Peierls in Birmingham. Peierls was a physicist with an extraordinary breadth of interests. There is almost no branch of modern theoretical physics in which he had not made some very significant contribution. He was also a great teacher. While he was in Birmingham, several generations of young theoretical physicists came there to study with him. Some, like Freeman Dyson, even lived in the Peierlses' house. Bell would have liked nothing better than to have gone off to Peierls, except, as he put it, "By that time I had a very bad conscience about having lived off my parents for so long [Bell had lived at home while he was in college], and I thought I should get a job. So I did get a job, at the Atomic Energy Research Establishment at Harwell. There I went, and I found myself soon sent off to a substation at Malvern in Worcestershire." Bell was now twenty-one. While he returned to Ireland regularly to see his family (and in June 1988, in a single week, he was awarded honorary degrees from Queen's University in Belfast, his alma mater, and from Trinity College in Dublin), he never did return to live in Ireland.

In the meantime, by an equally serendipitous route, Mary Bell had also arrived in Malvern. Mary Ross was born in Glasgow into what Bell called a "slightly more bookish family than my own." Her father began as a clerk in a shipyard and finally became a commercial manager specializing in the commerce of wood. Her mother was an elementary-school teacher. Unlike John, Mary Bell recalled being especially fond of arithmetic problems as a child. Fortunately, as it turned out, the school she went to—a comprehensive school with grades from elementary school through high school—was coeducational, which meant it offered a physics course. "With girls alone," she told me, "there very likely would not have been physics—other sciences but not physics." She did very well in school and, as her husband was fond of reminding her, won many prizes for "general excellence." In 1941, she won a bursary competition, which enabled her to go to the University of Glasgow, where she majored in mathematics and physics. "It was during the war," she reminded me, "and we all had to take courses in radio, circuits, and the like. At the end of my third year, I was drafted to work in the radar lab at Malvern, where, I must say, I didn't do a lot." After the war ended, she finished her degree in Glasgow and was, like John, hired by the Atomic Energy Research Establishment and soon sent to Malvern, where she joined the same accelerator design group that had hired John.

As it happened, the Bells' arrival in the accelerator group at Malvern more or less coincided with a revolutionary breakthrough in the design of accelerators, known as the principle of strong focusing, which was invented in the United States by Ernest Courant, M. Stanley Livingston, and Hartland Snyder and independently by a Greek physicist-inventor named Nicholas Christofilos. A beam of charged particles that is being guided around the interior of an accelerator by electric and magnetic fields tends to try to "get away." Unless it is focused by these fields, the beam squirts off uncontrollably in all directions. For a while, it appeared that this problem would be insurmountable for the new generation of machines being designed for the 1950s. But, as has happened so often in the accelerator business, there was an unexpected discovery that salvaged the situation. In this case, Courant, Livingston, and Snyder realized that if one used suitably varying electric and magnetic fields, rather than steady ones, as had previously been used to confine the beam, stability could be achieved. John Bell told me that prior to this discovery, a few designs had been generated in computer simulations that seemed to exhibit stability, but no one understood why.

Bell very quickly became an expert on the mathematics of strong focusing. He is best known among theoretical physicists for his abstruse work on the theory of elementary particles and quantum mechanics, so he was very pleased to have in his curriculum vitae such "engineering" items as "Stability of Perturbed Orbits in the Synchrotron," written in 1954. (*Synchrotron* is the generic term for the kind of accelerator that uses strong focusing.) As young as he was, Bell became a consultant to the British delegation that was beginning the design studies that would lead, in 1959, to the construction of the first accelerator at CERN—the so-called Proton Synchrotron. It was a landmark in postwar European science, as it was the first major scientific project carried out after the war by the European community as a whole. Bell began consulting for the project in 1952. That, as it happened, was also the year he got an unexpected job bonus—"a bolt from the blue," as he put it. It turned out that the Atomic Energy Research Establishment had the very enlightened policy of selecting some of their young people and sending them back to the universities for a year's study. "It was proposed to *me*," Bell recalled, still with a tone of surprise. "It didn't come from me, that I might go back to university for a year. And so I said, 'Fine,' and off I went to Birmingham, and there I became a quantum field theorist." In the meantime, the Bells had both moved from Malvern to Harwell and had gotten married.

John went off to Birmingham, and Mary remained at Harwell. "Our marriage was a thing of weekends for a while," he noted. In the British system at the time, there was in each department a single "professor" who functioned essentially as the chairman of the department. Peierls was *the* professor of theoretical physics at Birmingham, and he assigned the young people who came to work with him both problems to work on and junior members of the faculty to work with. Bell, having already worked as a professional physicist for three years, was somewhat older than the rest of the students. After noting that he did not intend to treat him as a "beginner," Peierls suggested a general area for Bell to look into and assigned him an adviser, a young British theorist named Paul Matthews. It was not clear whether the work was meant to lead to a degree or even if a second year was possible. "I just didn't open up that question," Bell told me. "I thought a year was fine. I thought I was very lucky to get it. I just accepted that."

Within a few months, Bell discovered a very deep theorem in quantum field theory known as the TCP theorem. In this acronym, T stands for time reversal, C for charge conjugation, and P for parity. These are quantum-mechanical symmetries. The theorem states that the combined operation of these symmetries is a valid one even in theories where the individual symmetries may break down. It implies, among other things, that particles and antiparticles, such as electrons and positrons, have the same mass.

"Unfortunately for me," Bell explained, "when I was writing that up, there appeared not just a preprint but a *reprint* from Gerhard Lüders, who had made the same discovery. So I was a year behind him. [Lüders's work was later generalized by Wolfgang Pauli and is often referred to as the Lüders-Pauli theorem.] And I cannot exclude that there was some garbled rumor of Lüders's work that had reached Birmingham and that Peierls had asked me to look into that. Anyway, I thought I could make a paper out of my stuff and also part of a thesis."

When his year was up, Bell returned to Harwell to a new group that had been formed to do fundamental research in areas such as elementary-particle physics. Bell was able to do a second problem, which completed his thesis. "From time to time," he explained to me, "I had a remark to make to my old accelerator group. Mary was still there. I hadn't given much thought to my future. When I went to Harwell at the age of twenty-one, I already had a tenure position. I didn't have to worry about anything. I just went along. So long as I was happy, I didn't think of going anywhere. But towards the end of the fifties I began feeling uncomfortable because there was a growing soul-searching at Harwell. What was that establishment supposed to be doing? They were not supposed to be doing nuclear weapons, though I believe now that there was some weapons work going on. Harwell had been set up to develop peaceful uses of atomic energy, but by that time, the nuclear power stations had already been built, and other, more applied-research establishments had grown up to do that kind of work. Harwell had sort of lost its sense of direction. Although I was in a very particular corner of Harwell, doing relatively fundamental work, this malaise was felt everywhere. It also began to look as if the fundamental research would be one of the things that would disappear in a reorientation of the establishment. So I started to think about going somewhere else." The "somewhere else" was CERN.

CERN—which stands for Conseil Européen pour la Recherche Nucleaire—had its formal beginnings in 1954. It was in 1989 a consortium of thirteen member states: Austria, Belgium, Great Britain, Denmark, France, Greece, Italy, the Netherlands, Norway, Spain, Sweden, Switzerland, and West Germany. Neither the United States nor the Soviet Union is a member, although both Americans and Soviets work in the laboratory. No classified work of any kind is done at CERN. In 1953, a referendum was held by the canton of Geneva, and voters ratified the decision of the Swiss government to give the nascent laboratory its site near the border. From the beginning, it was decided that CERN would have only a very small permanent staff of physicists compared with the visitors. For example, in the theory division, which consisted in 1989, the time of my visit, of about a hundred and forty people, less than 10 percent were staff members. Typically, a staff member is offered a three-year contract with the understanding that as a rule, a second three-year contract will be offered. Sometime during those six years, a decision is made about whether the staff member will be offered one of the few jobs with unlimited tenure.

There did not seem to be any university in Britain that could accommodate both Mary Bell's interests in accelerator design and John Bell's growing commitment to research in elementary-particle physics. CERN seemed ideal, except that they would be giving up the security of John's tenured job for three-year contracts. "I am amazed," he commented, "at how easily we left a tenured situation at Harwell and went to an untenured one. I remember that Mary's parents were a bit worried, but neither of us was worried. When we first got here in 1960, they already had the tradition that new people were more or less ignored and that you had to find your way to other people. Of course, when I first showed up in the theory division, they shook my hand and said, 'Welcome,' but after that, they left me alone. Nobody was coming into my office or anything. I got quite lonely for the first months. It's my understanding that newcomers here can still feel like this for the first months. From time to time, we think that we must do something about it, and we have tried various schemes. But the trouble is that the staff members who are here are already so saturated that it is not easy to cold-bloodedly say I must go and spend time with this person because he or she is new. In any case, Mary and I settled into Geneva very easily. We got to know people we work with in CERN, and we didn't try to get to know many other people. So the fact that we were surrounded by Swiss rather than English and that they spoke French wasn't very important to us. And of course, we were very happy with the new landscape around here. While we liked being in Berkshire, where Harwell is—there are a lot of beautiful things in Berkshire—the novelty of the high mountains here pleased us very much." Except for a very occasional sabbatical leave, the Bells had not left Geneva since 1960.

I once asked Bell whether during the years he was studying quantum theory it ever occurred to him that the theory might simply be wrong. He thought a moment and answered, "I hesitated to think it might be wrong, but I *knew* that it was rotten." Bell pronounced the word *rotten* with a good deal of relish and then added, "That is to say, one has to find some decent way of expressing whatever truth there is in it."

The attitude that even if there is not something actually wrong with the theory, there is something deeply unsettling—rotten—about it was common to most of the creators of quantum theory. Bohr was reported to have remarked, "Well, I think that if a man says it is completely clear to him these days, then he has not really understood the subject." He later added, "If you do not get *schwindlig* [dizzy] sometimes when you think about these things then you have not really understood it."

My teacher Philipp Frank used to tell about the time he visited Einstein in Prague in 1911. Einstein had an office at the university that overlooked a park. People were milling around in the park, some engaged in vehement gesture-filled discussions. When Professor Frank asked Einstein what was going on, Einstein replied that it was the grounds of a lunatic asylum, adding, "Those are the madmen who do not occupy themselves with the quantum theory."

Max Planck, whom one may consider either the father or the grandfather of quantum theory, depending on one's analysis of the history, was hardly a "lunatic." He was a conservative German, in the best sense of the term, from an ancient family of scholars, public servants, and lawyers. His father was a professor of law at Kiel, where Planck was born in 1858. Planck died in 1947, having lived with dignity in Germany during the Nazi regime. His son Erwin was executed by the Nazis after he took part in the July 1944 plot against Hitler.

What appealed to Planck about physics was the possibility of finding absolute laws that would retain their meaning, as he once wrote, "for all times and all cultures." One such law appeared to be the one governing what is known as black-body or cavity radiation. A so-called black body can be made with a hollow, thin-walled cylinder of some metal such as tungsten. The walls of the cylinder are heated by, for example, passing an electric current through them. Radiation is then produced from the heated walls, and it collects within the hollow cylinder. If a small hole is drilled in the cylinder, enough of this radiation gets out that one can measure its characteristics. (Incidentally, if the cylinder is kept at room temperature, the hole looks perfectly black from the outside, since any radiation falling into it is trapped inside the cylinder—hence the name *black body*.) In particular, black-body radiation has a very characteristic distribution of wavelengths—colors. It turns out that this distribution depends only on the temperature to which the cylinder is heated and not on the material of which the cylinder is made. Cavities made of, say, tantalum or molybdenum will exhibit the same black-body spectrum as