Chapter 2

"Spooky Actions at a Distance"

In a theory in which parameters are added to quantum mechanics to determine the results of individual measurements, without changing the statistical predictions, there must be a mechanism whereby the setting of one measuring device can influence the reading of another instrument, however remote. Moreover, the signal involved must propagate instantaneously.

—John S. Bell, 1964

One recent development dominated the Fundamental Fysiks Group's deliberations: a striking theorem published in the mid-1960s by the Irish physicist John S. Bell. The iconoclastic Bell had long nursed a private disquietude with quantum mechanics. His physics teachers—first at Queen's University in his native Belfast during the late 1940s, and later at Birmingham University, where he pursued doctoral work in the mid-1950s—had shunned matters of interpretation just as vehemently as their American colleagues did at the time. The "ask no questions" attitude frustrated Bell, who remained unconvinced that Bohr had really vanquished the last of Einstein's critiques long ago and that there was nothing left to worry about. At one point in his undergraduate studies, his red shock of hair blazing, he even engaged in a shouting match with a beleagured professor, calling him "dishonest" for trying to paper over genuine mysteries in the foundations, such as how to interpret the uncertainty principle. Certainly, Bell would grant, quantum mechanics worked impeccably "for all practical purposes," a phrase he found himself using so often that he coined the acronym "FAPP." But wasn't there more to physics than FAPP? At the end of the day, after all the wavefunctions had been calculated and probabilities plotted, shouldn't quantum mechanics have something coherent to say about nature?[1]

In the years following his impetuous shouting matches, Bell tried to keep these doubts to himself. At the tender age of twenty-one he realized that if he continued to indulge these philosophical speculations, they might well scuttle his physics career before it could even begin. He dove into mainstream topics, working on nuclear and particle physics at Harwell, Britain's civilian atomic energy research center. Still, his mind continued to wander. He wondered whether there were some way to push beyond the probabilities offered by quantum theory, to account for motion in the atomic realm more like the way Newton's physics treated the motion of everyday objects. In Newton's physics, the behavior of an apple or a planet was completely determined by its initial state—variables like position (where it was) and momentum (where it was going)—and the forces acting upon it; no probabilities in sight. Bell wondered whether there might exist some set of variables that could be added to the quantum-mechanical description to make it more like Newton's system, even if some of those new variables remained hidden from view in any given experiment. Bell avidly read a popular account of quantum theory by one of its chief architects, Max Born's *Natural Philosophy of Cause and Chance* (1949), in which he learned that some of Born's contemporaries had likewise tried to invent such "hidden variables" schemes back in the late 1920s. But Bell also read in Born's book that another great of the interwar generation, the Hungarian mathematician and physicist John von Neumann, had published a proof as early as 1932 demonstrating that hidden variables could not be made compatible with quantum mechanics. Bell, who could not read German, did not dig up von Neumann's recondite proof. The say-so of a leader (and soon-to-be Nobel laureate) like Born seemed like reason enough to drop the idea.[2]

Imagine Bell's surprise, therefore, when a year or two later he read a pair of articles in the *Physical Review* by the American physicist David Bohm. Bohm had submitted the papers from his teaching post at Princeton University in July 1951; by the time they appeared in print six months later, he had landed in São Paulo, Brazil, following his hounding

by the House Un-American Activities Committee (HUAC). Bohm had been a graduate student under J. Robert Oppenheimer at Berkeley in the late 1930s and early 1940s. Along with several like-minded friends, he had participated in freewheeling discussion groups about politics, worldly affairs, and local issues like whether workers at the university's laboratory should be unionized. He even joined the local branch of the Communist Party out of curiosity, but he found the discussions so boring and ineffectual that he quit a short time later. Such discussions might have seemed innocuous during ordinary times, but investigators from the Military Intelligence Division thought otherwise once the United States entered World War II, and Bohm and his discussion buddies started working on the earliest phases of the Manhattan Project to build an atomic bomb. Military intelligence officers kept the discussion groups under top-secret surveillance, and in the investigators' eyes the line between curious discussion group and Communist cell tended to blur. When later called to testify before HUAC, Bohm pleaded the Fifth Amendment rather than name names. Over the physics department's objections, Princeton's administration let his tenure-track contract lapse rather than reappoint him. At the center of a whirling media spectacle, Bohm found all other domestic options closed off. Reluctantly, he decamped for Brazil.[3]

In the midst of the Sturm und Drang, Bohm crafted his own hidden variables interpretation of quantum mechanics. As Bell later reminisced, he had "seen the impossible done" in these papers by Bohm. Starting from the usual Schrödinger equation, but rewriting it in a novel way, Bohm demonstrated that the formalism need not be interpreted only in terms of probabilities. An electron, for example, might behave much like a bullet or billiard ball, following a path through space and time with well-defined values of position and momentum every step of the way. Given the electron's initial position and momentum and the forces acting on it, its future behavior would be fully determined, just like the case of the trusty billiard ball—although Bohm did have to introduce a new "quantum potential" or force field that had no analogue in classical physics. In Bohm's model, the quantum weirdness that had so captivated

Bohr, Heisenberg, and the rest—and that had so upset young Bell, when parroted by his teachers—arose because certain variables, such as the electron's initial position, could never be specified precisely: efforts to measure the initial position would inevitably disturb the system. Thus physicists could not glean sufficient knowledge of all the relevant variables required to calculate a quantum object's path. The troubling probabilities of quantum mechanics, Bohm posited, sprang from averaging over the real-but-hidden variables. Where Bohr and his acolytes had claimed that electrons simply did not possess complete sets of definite properties, Bohm argued that they did—but, as a practical matter, some remained hidden from view.[4]

Bohm's work had captivated members of Hans Freistadt's 1954 discussion group, that bunch of bedraggled leftist physicists who dove into quantum physics and philosophy as a welcome break from their run-ins with HUAC and related red-baiters. In fact, Freistadt devoted his long review article to Bohm's approach to hidden variables. Quite independently, Bohm's papers fired Bell's imagination as well. Soon after discovering them, Bell gave a talk on Bohm's papers to the Theory Division at Harwell. Most of his listeners sat in stunned (or perhaps just bored) silence: Why was this young physicist wasting their time on such philosophical drivel? Didn't he have any real work to do? One member of the audience, however, grew animated: Austrian émigré Franz Mandl. Mandl, who knew both German and von Neumann's classic study, interrupted several times; the two continued their intense arguments well after the seminar had ended. Together they began to reexamine von Neumann's no-hidden-variables proof, on and off when time allowed, until they each went their separate ways. Mandl left Harwell in 1958; Bell, dissatisfied with the direction in which the laboratory seemed to be heading, left two years later.[5]

Bell and his wife Mary, also a physicist, moved to CERN, Europe's multinational high-energy physics laboratory that had recently been established in Geneva. Once again he pursued cutting-edge research in particle physics. And once again, despite his best efforts, he found himself pulled to his hobby: thinking hard about the foundations of quantum

mechanics. Once settled in Geneva, he acquired a new sparring partner in Josef Jauch. Like Mandl, Jauch had grown up in the Continental tradition and was well versed in the finer points of Einstein's, Bohr's, and von Neumann's work. In fact, when Bell arrived in town Jauch was busy trying to strengthen von Neumann's proof that hidden-variables theories were irreconcilable with the successful predictions of quantum mechanics. To Bell, Jauch's intervention was like waving a red flag in front of a bull: it only intensified his resolve to demonstrate that hidden variables had not yet been ruled out. Spurred by these discussions, Bell wrote a review article on the topic of hidden variables, in which he isolated a logical flaw in von Neumann's famous proof. At the close of the paper, he noted that "the first ideas of this paper were conceived in 1952"—fourteen years before the paper was published—and thanked Mandl and Jauch for all of the "intensive discussion" they had shared over that long period.[6]

Still Bell kept pushing, wondering whether a certain type of hidden-variables theory, distinct from Bohm's version, might be compatible with ordinary quantum mechanics. His thoughts returned to the famous thought experiment introduced by Einstein and his junior colleagues Boris Podolsky and Nathan Rosen in 1935, known from the start by the authors' initials, "EPR." Einstein and company had argued that quantum mechanics must be incomplete: at least in some situations, definite values for pairs of variables could be determined at the same time, even though quantum mechanics had no way to account for or represent such values. The EPR authors described a source, such as a radioactive nucleus, that shot out pairs of particles with the same speed but in opposite directions. Call the left-moving particle "A," and the right-moving particle "B." A physicist could measure A's position at a given moment, and thereby deduce the value of B's position. Meanwhile, the physicist could measure B's momentum at that same moment, thus capturing knowledge of B's momentum and simultaneous position to any desired accuracy. Yet Heisenberg's uncertainty principle dictated that precise values for certain pairs of variables, such as position and momentum, could never be known simultaneously.[7]

Fundamental to Einstein and company's reasoning was that quantum objects carried with them—on their backs, as it were—complete sets of definite properties at all times. Think again of that trusty billiard ball: it has a definite value of position and a definite value of momentum at any given moment, even if we choose to measure only one of those properties at a time. Einstein assumed the same must be true of electrons, photons, and the rest of the furniture of the microworld. Bohr, in a hurried response to the EPR paper, argued that it was wrong to assume that particle B had a real value for position all along, prior to any effort to measure it. Quantum objects, in his view, simply did not possess sharp values for all properties at all times. Such values emerged during the act of measurement, and even Einstein had agreed that no device could *directly* measure a particle's position and momentum at the same time. Most physicists seemed content with Bohr's riposte—or, more likely, they were simply relieved that someone else had responded to Einstein's deep challenge.[8]

Bohr's response never satisfied Einstein, however; nor did it satisfy John Bell. Bell realized that the intuition behind Einstein's famous thought experiment—the reason Einstein considered it so damning for quantum mechanics—concerned "locality." To Einstein, it was axiomatic that something that happens in one region of space and time should not be able to affect something happening in a distant region—more distant, say, than light could have traveled in the intervening time. As the EPR authors put it, "since at the time of measurement the two systems [particles A and B] no longer interact, no real change can take place in the second system in consequence of anything that may be done to the first system." Yet Bohr's response suggested something else entirely: the decision to conduct a measurement on particle A (either position or momentum) would *instantaneously* change the properties ascribed to the faraway particle B. Measure particle A's position, for example, and—*bam!*—particle B would be in a state of well-defined position. Or measure particle A's momentum, and—*zap!*—particle B would be in a state of well-defined momentum. Late in life, Bohr's line still rankled Einstein. "My instinct for physics bristles at this," Einstein wrote to a friend in March 1948. "Spooky actions at a distance," he huffed.[9]

Fresh from his wrangles with Jauch, Bell returned to EPR's thought experiment. He wondered whether such "spooky actions at a distance" were endemic to quantum mechanics, or just one possible interpretation among many. Might some kind of hidden variable approach reproduce all the quantitative predictions of quantum theory, while still satisfying Einstein's (and Bell's) intuition about locality? He focused on a variation of EPR's setup, introduced by David Bohm in his 1951 textbook on quantum mechanics. Bohm had suggested swapping the values of the particles' spins along the x- and y-axes for position and momentum.[10]

"Spin" is a curious property that many quantum particles possess; its discovery in the mid-1920s added a cornerstone to the emerging edifice of quantum mechanics. Quantum spin is a discrete amount of angular momentum—that is, the tendency to rotate around a given direction in space. Of course many large-scale objects possess angular momentum, too: think of the planet Earth spinning around its axis to change night into day. Spin in the microworld, however, has a few quirks. For one thing, whereas large objects like the Earth can spin, in principle, at any rate whatsoever, quantum particles possess fixed amounts of it: either no spin at all, or one-half unit, or one whole unit, or three-halves units, and so on. The units are determined by a universal constant of nature known as Planck's constant, ubiquitous throughout the quantum realm. The particles that make up ordinary matter, such as electrons, protons, and neutrons, each possess one-half unit of spin; photons, or quanta of light, possess one whole unit of spin.[11]

In a further break from ordinary angular momentum, quantum spin can only be oriented in certain ways. A spin one-half particle, for example, can exist in only one of two states: either spin "up" or spin "down" with respect to a given direction in space. The two states become manifest when a stream of particles passes through a magnetic field: spin-up particles will be deflected upward, away from their previous direction of flight, while spin-down particles will be deflected downward. Choose some direction along which to align the magnets—say, the z-axis—and

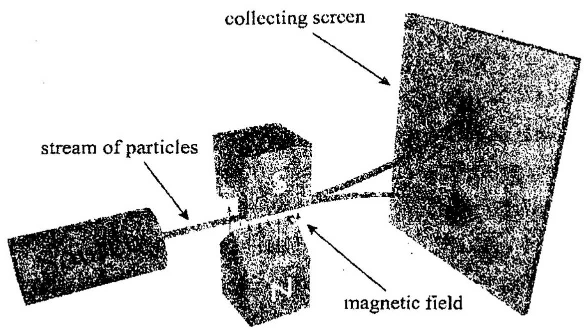

FIGURE 2.1. Device for measuring quantum particles' spin. Spin one-half particles, such as electrons, emerge from the source on the left and travel through the magnetic field, which points up from the north pole, *N*, of the magnet toward the south pole, *S*. Particles with spin up will be deflected upward from the original direction of flight and collect in one region of the collecting screen (or photographic plate); particles with spin down will be deflected downward. (Illustration by Alex Wellerstein.)

the spin of any electron will only ever be found to be up or down; no electron will ever be measured as three-quarters "up" along that direction. Now rotate the magnets, so that the magnetic field is pointing along some different direction. Send a new batch of electrons through; once again you will only find spin up or spin down along that new direction. For spin one-half particles like electrons, the spin along a given direction is always either +1 (up) or –1 (down), nothing in between.[12] (Fig. 2.1.)

No matter which way the magnets are aligned, moreover, one-half of the incoming electrons will be deflected upward and one-half downward. In fact, you could replace the collecting screen (such as a photographic plate) downstream of the magnets with two Geiger counters, positioned where the spin-up and spin-down particles get deflected. Then tune down the intensity of the source so that only one particle gets shot out at a time. For any given run, only one Geiger counter will click: either the upper one (indicating passage of a spin-up particle) or the lower one (indicating spin down). Each particle has a fifty-fifty chance of being

measured as spin up or spin down; the sequence of clicks would be a random series of +1s (upper counter) and –1s (lower counter), averaging out over many runs to an equal number of clicks from each detector. Neither quantum theory nor any other scheme has yet produced a successful means of predicting in advance whether a given particle will be measured as spin up or spin down; only the probabilities for a large number of runs can be computed.

Bell realized that Bohm's variation of the EPR thought experiment, involving particles' spins, offered two main advantages over EPR's original version. First, the measurements always boiled down to either a +1 or a –1; no fuzzy continuum of values to worry about, as there would be when measuring position or momentum. Second, physicists had accumulated decades of experience building real machines that could manipulate and measure particles' spin; as far as thought experiments went, this one could be grounded on some well-earned confidence. And so Bell began to analyze the spin-based EPR arrangement. Because the particles emerged in a special way—spat out from a source that had zero spin before and after they were disgorged—the total spin of the two particles together likewise had to be zero. When measured along the same direction, therefore, their spins should always show perfect correlation: if A's spin were up then B's must be down, and vice versa. Back in the early days of quantum mechanics, Erwin Schrödinger had termed such perfect correlations "entanglement."[13]

Bell demonstrated that a hidden-variables model that satisfied locality—in which the properties of A remained unaffected by what measurements were conducted on B—could easily reproduce the perfect correlation when A's and B's spins were measured along the same direction. At root, this meant imagining that each particle carried with it a definite value of spin along any given direction, even if most of those values remained hidden from view. The spin values were considered to be properties of the particles themselves; they existed independent of and prior to any effort to measure them, just as Einstein would have wished.

Next Bell considered other possible arrangements. One could

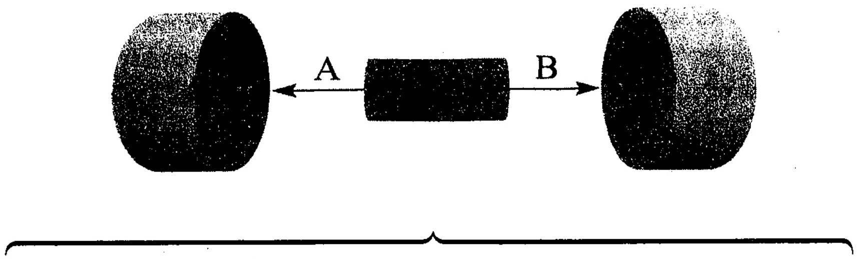

FIGURE 2.2. Bell's updated thought experiment, based on Bohm's version of the EPR setup. A source shoots out pairs of particles, A and B. Each detector has two directions along which it can measure a particle's spin, corresponding to the orientation of the magnets used to separate particles with spin up from those with spin down. As shown here, the apparatus is set to measure the spin of particle A along one direction (setting *a*) and the spin of particle B along a different direction (setting *b′*). (Illustration by Alex Wellerstein.)

choose to measure a particle's spin along any direction: the *z*-axis, the *y*-axis, or any angle in between. All one had to do was rotate the magnets between which the particle passed. What if one measured A's spin along the *z*-axis and B's spin along some other direction? (Fig. 2.2.) Bell homed in on the expected correlations of spin measurements when shooting pairs of particles through the device, while the detectors on either side were oriented at various angles. He considered detectors that had two settings, or directions along which spin could be measured. To keep track of all the possible combinations, he labeled the settings on the left-hand detector—which would measure the spin of particle A—as *a* and *a′*: *a* for when the left-hand detector was oriented along the *z*-axis, and *a′* for when that detector was oriented along its other direction. Same for the right-hand detector, toward which particle B careened: *b* when the right-hand detector was oriented along the *z*-axis, and *b′* when it was oriented along its other direction. (Bell took the settings *a′* and *b′* to lie in the same direction: when the detectors were set to *a′* and *b′*, every pair of particles would be measured as having opposite spin; same for when both detectors were set to *a* and *b*.)

Bell labeled the outcomes of each of these measurements. He denoted the measured outcome of the spin of particle A when the

left-hand detector was in setting *a* as *A*, and the outcome when the left-hand detector was set to *a′* as *A′*; similarly for *B* and *B′* for the measurements on particle B. All of these measurement outcomes—*A*, *A′*, *B*, and *B′*—were just plain numbers. In fact, they were particularly simple ones: because every spin measurement, along any direction, could only ever result in spin up or spin down, *A*, *A′*, *B*, and *B′* could only ever equal +1 or −1. Bell could then consider various combinations of measurements, such as *AB*, the product of outcomes when the left-hand detector was set to *a* and the right-hand detector to *b*; or *AB′*, which arose when the left-hand detector was set to *a* and the right-hand detector to *b′*. Since each measurement outcome (*A*, *A′*, *B*, *B′*) could only equal +1 or −1, the pairs—*AB* or *AB′*, and so on—would likewise just equal +1 or −1. One could then consider a particular combination, *S*, built from all the various correlations that could arise:

$$S = AB - A'B + AB' + A'B' = (A - A')\,B + (A + A')\,B'$$

One of the terms in parentheses would always vanish, and the other would always equal +2 or −2. Perhaps in one instance $A = +1$ and $A' = +1$; then $(A - A') = 0$, and $(A + A') = 2$. Or it could be that $A = -1$ and $A' = +1$, so that $(A - A') = -2$ and $(A + A') = 0$. Since *B* and *B′* always equal +1 or −1, the combination, *S*, must always equal +2 or −2; no other value could ever arise. Bell imagined emitting a large number of particle pairs from the source, one pair at a time, and recording the measured outcomes at each detector (noting carefully the settings at each detector for each particular run). After many pairs of particles had been measured, one would expect to find the average value for *S*, $S_{average}$, to fall within the range $-2 \leq S_{average} \leq +2$: sometimes *S* would equal +2 and other times −2, so that the average of large numbers of runs should give some value in between.[14]

So far, so good. But Bell wasn't finished yet. As he demonstrated next, quantum mechanics made unambiguous predictions for the probabilities of various correlations between the spins of particles A and B as one varied the direction along which they were measured. For various choices of the angle between detector settings *a* and *b′* (or, equivalently,

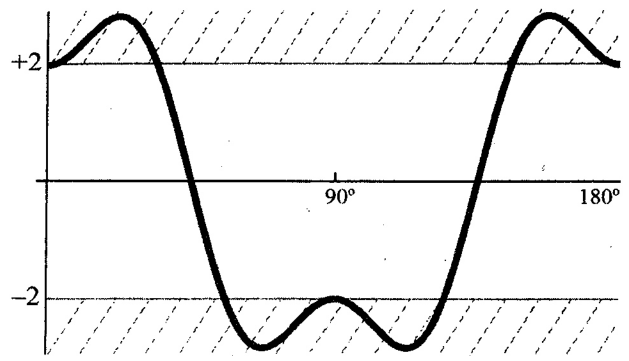

FIGURE 2.3. Predicted values for the quantity *S*, made up of combinations of spin measurements on particles A and B along various directions. The horizontal axis shows the angle between detector settings *a* and *b′* (or, equivalently, between *a′* and *b*). As Bell demonstrated, the assumption that particles A and B carried definite values for spin along each direction prior to measurement—as Einstein and his collaborators had urged—limited *S* to lie between +2 and -2. Yet the quantum-mechanical prediction for the correlation violated that bound by more than 40 percent for certain choices of angle. (Illustration by Alex Wellerstein, based on Aspect [2002], 130.)

between settings *a′* and *b*), quantum mechanics predicted clear violations of the innocuous-looking inequality, $-2 \leq S \leq +2$. In fact, for judicious choices of angle, the quantum predictions exceeded this bound by a sizable amount—more than 40 percent. In effect, quantum mechanics predicted that particles A and B should be *more strongly correlated* than the bound on *S* would allow. (Fig. 2.3.)

Using only a few lines of algebra, Bell thus proved that *no* local hidden-variables theory could ever reproduce the same degree of correlations as one varied the angles between detectors. The result has come to be known as "Bell's theorem." Simply *assuming* that each particle carried a full set of definite values on its own, prior to measurement—even if most of those values remained hidden from view—necessarily clashed with quantum theory. Nonlocality was indeed endemic to quantum mechanics, Bell had shown: somehow, the outcome of the measurement on

particle B depended on the measured outcome on particle A, even if the two particles were separated by huge distances at the time those measurements were made. Any effort to treat the particles (or measurements made upon them) as independent, subject only to local influences, necessarily led to predictions different from those of quantum mechanics. Here was what Bell had been groping for, on and off since his student days: some quantitative means of distinguishing Bohr's interpretation of quantum mechanics from other coherent, self-consistent possibilities. The problem—entanglement versus locality—was amenable to experimental test. In his bones he hoped locality would win.[15]

In the years since Bell formulated his theorem, many physicists (Bell included) have tried to articulate what the violation of his inequality would mean, at a deep level, about the structure of the microworld. Most prosaically, entanglement suggests that on the smallest scales of matter, the whole is more than the sum of its parts. Put another way: one could know *everything* there is to know about a quantum system (particles A + B), and yet know *nothing* definite about either piece separately. As one expert in the field has written, entangled quantum systems are not even "divisible by thought": our natural inclination to analyze systems into subsystems, and to build up knowledge of the whole from careful study of its parts, grinds to a halt in the quantum domain.[16]

Physicists have gone to heroic lengths to translate quantum nonlocality into everyday terms. The literature is now full of stories about boxes that flash with red and green lights; disheveled physicists who stroll down the street with mismatched socks; clever Sherlock Holmes–inspired scenarios involving quantum robbers; even an elaborate tale of a baker, two long conveyor belts, and pairs of soufflés that may or may not rise.[17] My favorite comes from a "quantum-mechanical engineer" at MIT, Seth Lloyd. Imagine twins, Lloyd instructs us, separated a great distance apart. One steps into a bar in Cambridge, Massachusetts, just as her brother steps into a bar in Cambridge, England. Imagine further (and this may be the most difficult part) that neither twin has a cell phone or any other device with which to communicate back and forth. No matter what each bartender asks them, they will give opposite answers. "Beer or whiskey?"

The Massachusetts twin might respond either way, with equal likelihood; but no matter which choice she makes, her twin brother an ocean away will respond with the opposite choice. (It's not that either twin has a decided preference; after many trips to their respective bars, they each wind up ordering beer and whiskey equally often.) The bartenders could equally well have asked, "Bottled beer or draft?" or "Red wine or white?" Ask *any* question—even a question that no one had decided to ask until long after the twins had traveled far, far away from each other—and you will *always* receive polar opposite responses. Somehow one twin always "knows" how to answer, even though no information could have traveled between them, in just such a way as to ensure the long-distance correlation.[18]

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From today's vantage point, Bell's theorem is of unparalleled significance. His proof that quantum mechanics necessarily implied nonlocality—that a measurement of particle A would instantaneously affect particle B, even if they were a galaxy apart—dramatized the philosophical stakes involved when trying to make sense of quantum reality. Bell's short article has accumulated more than 3200 citations in the professional scientific literature, an astonishing level of interest rivaled by roughly 1 out of every 10,000 physics papers ever published. Today Bell's theorem, and the entangled states at its core, is the centerpiece of everything from quantum computing, to quantum encryption, to quantum teleportation. (The special beams of light at the heart of the 2004 money transfer in Vienna consisted of entangled pairs of photons.) Without question, physicists, philosophers, and historians now see Bell's theorem, entanglement, and nonlocality as among the most important developments in quantum theory. As authors of a recent textbook put it, Bell's theorem and entanglement have become "a fundamentally new resource in the world that goes essentially *beyond* classical resources; iron to the classical world's bronze age."[19] (Fig. 2.4.)

All that lay far in the future when Bell was puzzling through his short paper back in the early 1960s. Bell worked out his theorem not at CERN,

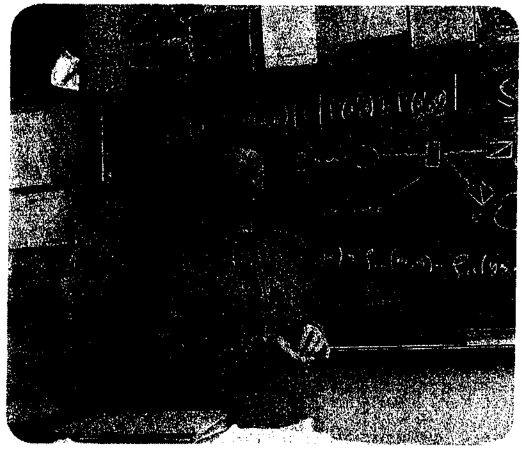

FIGURE 2.4. John S. Bell in his office at CERN, 1982. (Courtesy CERN.)

but while on sabbatical in the United States. Indeed, he later recalled that it was only in the United States—where so few physicists showed any signs of interest in such topics—where he could achieve the isolation required to push through his thoughts and write up his papers. Bell left CERN in November 1963—arriving in the United States one day after John F. Kennedy had been assassinated, as it happened—and spent the year visiting the Stanford Linear Accelerator Center, the University of Wisconsin at Madison, and Brandeis University near Boston. He completed his review article on hidden variables first, and mailed it off to the *Reviews of Modern Physics*, in whose editorial office the manuscript mysteriously vanished, leading to an unheard-of two-year delay in its publication.[20]

At Brandeis he completed his second paper, "On the Einstein Podolsky Rosen paradox," containing his proof that quantum mechanics cannot be squared with locality. At the time, authors had to pay steep fees to cover the cost of publishing their articles in the venerable *Physical Review*, long the standard-bearer among the world's physics research journals. Bell was too shy to ask his American hosts to pay for an article so far removed from their research interests. So he submitted it to a brand-new journal with the curious title *Physics Physique Fizika*. Not only did the new journal waive page fees, but it actually *paid* authors to publish there—although the honoraria turned out to be nearly equal to the cost of ordering reprints. The journal's editors had high hopes that their new venture would help alleviate the information overload and hyperspecialization then afflicting the field, comparing it to a general-interest magazine like *Harper's*. In their opening editorial, the editors pledged to "try their very best to present a selection of papers which are worth the attention of all physicists." Bell's article appeared in the third issue of the fledgling journal, in November 1964.[21]

And then . . . nothing. No activity or acknowledgment whatsoever. Bell's paper, deemed worthy of "the attention of all physicists" by the journal's editors, did not receive so much as a single citation in the literature for four long years—and then it was passing mention in a one-page article. Slowly, slowly, citations to Bell's paper began to appear, like the irregular clicks of a Geiger counter: six in 1971, seven in 1972, three in 1973. A burst of sustained activity began only in 1976, when twenty to thirty new articles on the topic began to appear each year. By 1980, a quite respectable 160 articles had been published in the physics literature on Bell's theorem.[22]

During the mid- and late 1970s, pockets of interest coalesced, usually led by physicists who held a longtime interest in hidden variables and the interpretation of quantum mechanics. An active group emerged around hidden-variables theorist David Bohm, whose long journey following his McCarthy-era dismissal from Princeton had ended with him settled at Birkbeck College in London, following hops and skips to São Paulo, Brazil; the Technion Institute in Haifa, Israel; and Britain's Bristol University. A separate group clustered around Louis de Broglie and Jean-Paul Vigier in Paris; and a third group, spearheaded by Franco Selleri, shuttled among Bari, Catania, and Florence in Italy. Most of these physicists had been working on hidden variables and the interpretation of quantum mechanics for decades; Bell's theorem appeared an obvious extension of their long-standing interests. Acknowledgments in these many articles show a tight fabric of social interactions: members of each of these groups knew each other, frequently traded tips and critiques, and saw each other's latest papers as preprints long before they appeared in the journals. By 1980, in other words, an "invisible college" devoted to Bell's theorem had emerged, with centers of activity dotted throughout Western Europe.[23]

Surprisingly, the largest share of articles on Bell's theorem during this period came from physicists working in the United States—27 percent of all the articles, in fact, compared with 7 percent, 14 percent, and 19 percent from authors based in Britain, France, and Italy, respectively. All this despite the absence of any deep interest in foundational topics on American soil, hidden variables or otherwise. Nearly three-quarters of these U.S.-based articles (72 percent), meanwhile, came from regular participants in the Fundamental Fysiks Group, the earliest sessions of which had been devoted to Bell's work and quantum nonlocality. (If one includes authors who acknowledged help from members of the Fundamental Fysiks Group, the proportion rises to 86 percent.) Members of the ragtag discussion group proved to be among the most prolific early authors on Bell's theorem in the world. Against all odds, the earliest champions of Bell's theorem congregated in that most unphilosophical of spaces: a large seminar room in the Lawrence Berkeley Laboratory.[24]

5 Central Intelligence Agency, unclassified "memorandum for the record," December 4, 1979, copy in *JAW*, Sarfatti folders; Wilson (1979). *Scientific American* also ran a feature article on the topic that same year: d'Espagnat (1979). As we will see in chapter 5, the *Scientific American* article's author crossed paths with the physicists I focus on here. On *Oui* magazine, see Anon. (1981).
6 See esp. Holton (1988), Feuer (1974), Beller (1999), and Gilder (2008).
7 Kevles (1995), Forman (1987), Schweber (1994), and Galison (1997).
8 Holton (1998), Pais (1991), Cassidy (1992), and Moore (1989).
9 Kaiser (2004, 2007a).
10 Kaiser (2002). See also Kevles (1995), chap. 25; Leslie (1993), chap. 9; and Moore (2008).
11 Gustaitus (1975). On changes at the magazine, see also Anon. (1975).
12 Sirag (1977a, b) and Sarfatti (1977a). On Leary's acceptance of Sirag's and Sarfatti's essays for *Spit in the Ocean*, see Sirag (2002), 111. On Leary's and Kesey's counterculture exploits, see Wolfe (1968), Lee and Shlain (1992), and Lattin (2010).
13 Gold (1993), 15–17, 38, 115.
14 Anon. (1974a), Woodward and Lubenow (1979), Garfinkel (1982), Anon. (1977a), Carroll (1981), and Roosevelt (1980). On the 1977 humanistic psychology conference, see also Jerry Diamond to Sarfatti, n.d. (ca. January 1977), copy in *JAW*, Sarfatti folders.
15 Heirich (1976), 697. Heirich referred to work by Jack Sarfatti, Fred Alan Wolf, Henry Stapp, and Evan Harris Walker, among others.
16 See esp. Pamplin and Collins (1975); Pinch (1979); Collins and Pinch (1979, 1982); and Collins (1992), chap. 5.
17 Popper (1963). See also Popper (1976), esp. chap. 8.
18 See esp. Collins (1992), Gieryn (1999), and Laudan (1983). See also Ross (1991), 23–30.
19 Reagan quoted in Braunstein and Doyle (2002), 6.
20 Rorabaugh (1989), chap. 4; Doyle (2001); Braunstein and Doyle (2002); Rossinow (2002); and Gosse (2005).
21 Lee and Shlain (1992), 119; and Nick Herbert, "Doctor Quantum drops acid," available at http://members.cruzio.com/~quanta/doctorquantum.html (accessed October 2, 2007). See also Novak (1997) and Wasserman (2000).
22 Rorabaugh (1989), 134, 135, 137; Lee and Shlain (1992), 154; and Braunstein (2002).
23 Novak (1997), 100.
24 Melton (1992), 20; and Kyle (1995), 157.
25 Lee and Shlain (1992), 148.
26 Lewis and Melton (1992); Kyle (1995), 13, 14, 65; and Schulman (2001), 96.
27 Roszak (1969), 48.
28 On similar intermingling of quintessential military-industrial sponsors with New Age and paranormal research, see also Burnett (2009, 2010), and Ronson (2004).
29 Cf. Peck (1985).
30 Nick Herbert as quoted in Physics/Consciousness Research Group, "A modest proposal to the Foundation for the Realization of Man," February 11, 1976, on p. 15; copy in *SPS*.
31 Cf. Forman (1971).
32 Lo, Popescu, and Spiller (1998), 15–17, 24, 77–88; Nielsen and Chuang (2000), 3, 24, 25, 528–531; and Jaeger (2007), 83, 147, 156, 157.
33 Cahill (1995).
34 Brisick (1995), Bernstein (1995), and Finn (1995).

Chapter 1: "Shut Up and Calculate"

1 Marin (1975), Heck and Thompson (1976), and Litwack (1976).
2 Erhard interview (2010).
3 Fred Alan Wolf, email to the author, November 12, 2007; Jack Sarfatti, email to the author, November 27, 2007 ("I think you're an asshole"); Wolf interview (2009); and Sarfatti interview (2009). Several sources documented the prevalence of the phrase "You're an asshole" in *est* trainings at the time: Heck and Thompson (1976), 20; Litwack (1976), 48, 50, 54; Fenwick (1976), 33, 34, 51, 96, 101, 165; and Hubner (1990a), 19.
4 Cf. Beller (1998).
5 The literature on the creation of quantum mechanics is vast. For an introduction, see esp. Jammer (1966); Darrigol (1992); Beller (1999); and Galison, Gordin, and Kaiser (2001).
6 Heisenberg (1971), 73–76.
7 Physicist N. David Mermin provides a fascinating and amusing genealogy of the phrase "shut up and calculate": Mermin (2004); see also Mermin (1989). I owe these references to Orzel (2009), 79, 80.
8 Bohr (1985), vol. 6; Bohr (1949), 215–20; and Heisenberg (1930). By no means did they always agree on what the double-slit experiment implied. See Beller (1999), chap. 11.
9 Moore (1989), 299.
10 Crease (2002), 19. For a classic presentation of the double-slit experiment, see Feynman with Leighton and Sands (1965), vol. 3, chap. 1.
11 Bohr (1949).
12 Albert Einstein to Erwin Schrödinger, June 17, 1935, as quoted in Fine (1986), 68 ("young whore"). On Einstein's use of the double-slit experiment for his criticisms of quantum theory, see Einstein to Schrödinger, April 26, 1926, in Przibram (1967), 28; and Jammer (1966), 360.
13 Wheaton (1983); Rodgers (2002), 15; and letters to the editor, *Physics World* 16 (May 2003): 20. For more recent experimental demonstrations, see Scully and Drühl (1981); Arndt et al. (1999); and Hillmer and Kwiat (2007).
14 Notes between Albert Einstein and Paul Ehrenfest, October 25, 1927, in *AE*, item 10-168. My translation; emphasis in original. See also Jammer (1966), 360. On Einstein's long-standing critique of quantum theory, see also Pais (1982), Fine (1986), Kaiser (1994), and Beller (1999).
15 Max Born introduced and refined his now-famous probability interpretation of the wavefunction in a series of short articles published in 1926. See, in particular, Jammer (1966), 283–290; and Beller (1990).
16 Albert Einstein to Erwin Schrödinger, May 31, 1928, in Przibram (1967), 31.
17 The same holds for the amount of energy exchanged in a given interaction and the time interval over which the interaction takes place. See Jammer (1966), chap. 7; Cassidy (1992), chap. 12; and Beller (1999), chaps. 4 and 5.
18 Heisenberg (1930), 76–79; see also Feynman with Leighton and Sands (1965), vol. 3, chap. 1, 6–9.
19 Einstein to Schrödinger, June 19, 1935 ("ridiculous little Talmudic philosopher," my translation of "talmudistische Philosoph"), in *AE*, item 22-047. On Bohr's complementarity, see esp. Holton (1988), 99–146; Folse (1985); Murdoch (1987); Kaiser (1992); and Beller (1999), chap. 6.
20 Albert Einstein to Max Born, undated, ca. January 1927, in Born (2005), 93; and Einstein (1934), 168, 169 ("transitory significance," "still believe").
21 Einstein to Born, December 4, 1926, in Born (2005), 88 ("Quantum mechanics is certainly

imposing"). See also Einstein to L. Cooper, October 31, 1949, in *AE*, item 411; Fine (1986); and Kaiser (1994).

22 Erwin Schrödinger (1935b), 812, as translated in Fine (1986), 65. On the correspondence between Einstein and Schrödinger that led to Schrödinger's article, see Fine (1986), chap. 5.

23 Wolfgang Pauli to Niels Bohr, February 11, 1924, in Pauli (1979), 143 ("very unphilosophical"). On Bohr's approach, see Kaiser (1992) and Faye (1991). On Heisenberg's philosophical pretensions, see also Carson (2010).

24 Bohr (1934, 1958); Born (1956), 107; and Pauli (1994). See also Beller (1998).

25 Albert Einstein to Erwin Schrödinger, June 17, 1935, as quoted in Fine (1986), 68 ("epistemology-soaked orgy"); Albert Einstein to Paul Bonofield, September 18, 1939, in *AE*, item 6-118-1 ("My own opinion"). See also Einstein to Maurice Solovine, April 10, 1938, in Einstein (1987), 85; Einstein (1949), esp. 671, 672.

26 Max Born to Albert Einstein, July 15, 1925, in Born (2005), 82; Erwin Schrödinger to Albert Einstein, May 30, 1928, in Przibram (1967), 30. See also Kragh (1999), 168–73.

27 Kuhn et al. (1967).

28 Haas (1928), chaps. 11 and 16; Heisenberg (1930), 65; Weyl (1931), 76; Born (1936), 82–85; and Sommerfeld (1930), 37, 257.

29 On American physicists' philosophical and pedagogical approaches to quantum mechanics in the 1930s, see Kaiser (2007a) and (forthcoming), chap. 4.

30 Einstein to Maurice Solovine, February 12, 1951, in Einstein (1987), 123.

31 Michael Cohen, entry of May 14, 1953, in Caltech "bone books," box 1, vol. 7 ("invested in analysis"), available in the Archives of the California Institute of Technology, Pasadena, California; Frederick Zachariasen, entry of May 27, 1953, ibid. ("usual spiel"); Romain (1960), 62 ("avoids philosophical discussion"); Falkoff (1952), 460, 461 ("philosophically tainted questions"); and Feshbach (1962), 514 ("musty atavistic to-do"). See also Kaiser (2007a).

32 Weiner (1969) and Rider (1984).

33 Edward Teller (with Robert F. Christy and Emil J. Konopinski), "Lecture notes on quantum mechanics," autumn 1945, on 79. A copy of these notes is available as part of "Notes on physics courses given at Los Alamos, 1943–1946," in *NBL*, call number AR31029. On effects of physicists' wartime projects, see esp. Forman (1987); Schweber (1994), chap. 3; and Galison (1997), chap. 4.

34 Smyth (1951); and Bureau of Labor Statistics report as quoted in Barton (1953), 6 ("If the research in physics").

35 Kaiser (2006a); cf. Forman (1987) and Leslie (1993).

36 Kaiser (2002).

37 Raymond T. Birge to E. W. Strong, August 30, 1950, in Raymond Thayer Birge correspondence and papers, call number 73/79c, Bancroft Library, University of California at Berkeley.

38 "Opinions of returning graduate students in physics," 86-pp report, 1948, call number UAV 691.448, in Harvard University Archives, Pusey Library, Cambridge, Massachusetts ("The classes are so large"). See also Kaiser (2004).

39 Gerjuoy (1956), 118 ("With these subjects"); and Uhlenbeck (1963), 886 ("easy to teach").

40 Eyvind Wichmann, "Comments on *Quantum Mechanics*, by L. I. Schiff (Second Edition)," n.d., ca. January 1965, in Leonard I. Schiff papers, call number SC220, Stanford University Archives, box 9, folder "Schiff: Quantum mechanics" ("The book kept me sufficiently busy").

41 Kaiser (2007a).

42 Hans Freistadt, who convened the group, thanked its members in Freistadt (1957), 65.

43 Schrecker (1986), 289, 290; and Wang (1999), chap. 7.

44 U.S. House, Committee on Un-American Activities (1953a), 190–212 and (1953b) 1795–99; Cattell (1960), s.v. "Darling, Byron T."; and Schrecker (1986), 207–9.

45 Freistadt (1953), 221, 229, 237 and (1957). Freistadt's review article received roughly one citation every other year over the next two decades, several of them in philosophy journals rather than physics ones: *Science Citation Index* (1961–). On political leanings of other group members, see Newman (2002); David Bohm to Melba Phillips, n.d., ca. June 1952, in David Bohm Archives, Birkbeck College, London, folder C46, and related correspondence in folders C3, C46, and C48; and John Stachel, email to the author, October 23, 2007.

46 Kevles (1995), chap. 25; Schweber (1988); Leslie (1993), chap. 9; and Moore (2008).

47 Kaiser (2002), 149–53.

Chapter 2: "Spooky Actions at a Distance"

1 Bernstein (1991a), 50, 51; Whittaker (2002), 14–17; and Gilder (2008), chaps. 27 and 28.

2 Bernstein (1991a), 53, 64, 65; Whittaker (2002), 17; cf. Born (1949). Von Neumann's proof appeared in von Neumann (1932), later translated as von Neumann (1955), on 305–24. On von Neumann's proof and the drawn-out debate it inspired, see Jammer (1966), 367–70; Pinch (1977); Bell (1982); and Jackiw and Shimony (2002), 84–87. Most of Bell's papers on the foundations of quantum mechanics were republished in Bell (2004b).

3 Scholars still debate the extent to which Bohm's political views shaped his own approach to physics, or the reception it received from others. See, e.g., Cross (1991); Olwell (1999); Mullet (1999, 2008); Kojevnikov (2002); and Freire (2005).

4 Bell (1982), 990 ("saw the impossible done"); and Bohm (1952a, b). See also Albert (1992), chap. 7; Cushing (1994); and Buchanan (2008). As is now well known, Bohm's 1952 papers bore strong similarity to a 1927 proposal by Louis de Broglie, which de Broglie had quickly abandoned in the face of criticism from Wolfgang Pauli. See, e.g., Jammer (1966), 291–93; and Cushing (1994), chaps. 7 and 8.

5 Bell (2002), 3–5; and Burke and Percival (1999), 9, 10. On the Bells' decision to leave Harwell, see Bernstein (1991a), 18–20.

6 Bell (1966), 452 ("first ideas"); Bernstein (1991a), 67, 68; cf. Jauch and Piron (1963). On Bell's significant contributions to "mainstream" nuclear and particle theory, see Burke and Percival (1999), 4–9; and Jackiw and Shimony (2002), 100–112.

7 Einstein, Podolsky, and Rosen (1935).

8 Bohr (1935). On the conditions surrounding Bohr's response, see Rosenfeld (1967). The literature on the Einstein-Bohr debate, and on the EPR thought experiment in particular, is enormous. See esp. Fine (1986), esp. chap. 3; Kaiser (1994); and Beller (1999), chap. 7.

9 Einstein, Podolsky, and Rosen (1935), 779 ("since at the time"); Albert Einstein to Max Born, March 18, 1948 ("bristles"), reprinted in Born (2005), 162; Einstein to Born, March 3, 1947, in Born (2005), 154, 155 ("spooky actions at a distance"). See also Howard (1985).

10 Bohm (1951), 614–22.

11 On the discovery of quantum-mechanical spin and its significant conceptual break from ordinary angular momentum, see, e.g., Jammer (1966), 133–56; and Tomonaga (1997).

12 The device pictured in figure 2.1 is a simplified version of a Stern-Gerlach apparatus, first conceived by Otto Stern in 1921 and put to use by Stern with Walther Gerlach a few months later. See Friedrich and Herschbach (2003).

13 Bell (1964). Erwin Schrödinger introduced the term "entanglement" in Schrödinger (1935a), 555.

14 Several other physicists derived this particular expression for *S*, building on Bell's work. It is often referred to as the CHSH inequality based on the authors' initials: Clauser, Horne, Shimony, and Holt (1969).
15 Bell (1964), 199; and Bernstein (1991a), 84, 85. On the quantum-mechanical calculation, see, e.g., Sakurai (1985), 223–232. Several authors have since simplified Bell's original proof, and popular treatments abound. Among the best are those by N. David Mermin, several of which are republished in Mermin (1990b), esp. chaps. 10–12. Philosopher Tim Maudlin's treatment is also particularly clear: Maudlin (1994), chap. 1.
16 D'Espagnat (2003), 112–14 ("divisibility by thought").
17 Mermin (1981, 1985, 1990a, 1994); Bell (2004a); Jacobs and Wiseman (2005); and Kwiat and Hardy (2000).
18 Lloyd (2006), 120, 121 ("beer or whiskey?").
19 Nielsen and Chuang (2000), 117 ("iron"), emphasis in original. Citation data from *Science Citation Index* (1961–). On the rarity of accumulating so many citations, compare with data on top-cited publications within high-energy physics available at http://www.slac.stanford.edu/spires (accessed January 15, 2008) and the data in Redner (2005). For recent assessments of the significance of Bell's theorem, see Jackiw and Shimony (2002) and Bertlmann and Zeilinger (2002).
20 Bell (1966); Bernstein (1991a), 67, 68; and Jackiw and Shimony (2002), 87.
21 Anderson and Matthias (1964); Bernstein (1991a), 74, 75; Wick (1995), 89, 90. On physicists' postwar anxieties about information overload and its effects on their research journals, see Kaiser (forthcoming), chap. 3. On page fees at the *Physical Review*, see Scheiding (2009).
22 Other than Bell's own 1966 review article on hidden variables, completed before his 1964 paper but published after it, the first article to cite Bell's 1964 paper was Clark and Turner (1968), 447. Citation data from *Science Citation Index* (1961–).
23 Clauser interview with Joan Bromberg (2002), 34, 51, 52; and Selleri interview with Olival Freire (2003), 23; both transcripts available in *NBL*. One member of Selleri's group, the experimentalist Vittorio Rapisarda, died in a car accident while driving from Catania to Bari for one of the group's regular meetings: 48, 49. For an indication of the tight-knit community working on Bell's theorem at the time, see the acknowledgments in Vigier (1974); Bohm and Hiley (1976b); Garuccio and Selleri (1976); Lamehi-Rachti and Mittig (1976); Baracca, Cornia, Livi, and Ruffo (1978); and Selleri (1978).
24 Based on data in *Science Citation Index* (1961–).

Chapter 3: Entanglements

1 This chapter draws inspiration from historians' grappling with scientists' "self-fashioning" and personae. See esp. Biagioli (1993); Daston and Sibum (2003); and Shapin (2008).
2 Clauser interview with Bromberg (2002), 11, 12, 14, 15; and Clauser interview (2009). See also Wick (1995), 104, 105; and Clauser (2002), esp. 71, 77, 78.
3 J. S. Bell to John Clauser, March 5, 1969 ("shake the world"); see also Clauser to Bell, February 14, 1969, and Bohm to Clauser, February 25, 1969, all in *JFC*, folder "Random correspondence." David Wick notes that Clauser's letter was the first direct response Bell had received to his work: Wick (1995), 106n124.
4 Clauser (1969), 578.
5 Most of Shimony's papers on foundations of quantum theory are collected in Shimony (1993), vol. 2.
6 Shimony interview with Bromberg (2002), 39, 40 (receipt of Bell's preprint), 49 ("kooky paper"), 51, 52 ("quantum archaeology"), and 53 ("whole thing on ice"). Shimony attributes the term "quantum archaeology" to his then-graduate student, Michael Horne (ibid., 51).

7 Shimony interview with Bromberg (2002), 54, 55, 71 ("civilized"); Clauser interview with Bromberg (2002), 34, 35; and Clauser (2002), 80, 81. See also Wick (1995), 103–13; and Aczel (2001), chap. 14.
8 Clauser interview with Bromberg (2002), 18, 35; and Clauser (2002), 62, 63, 70–72, 78. On the stigma at the time—especially its effects on graduate students and postdocs—see also Harvey (1980, 1981).
9 Clauser interview with Bromberg (2002), 35 ("some of which I picked up"); Clauser interview (2009); Shimony interview with Bromberg (2002), 73, 74; and Clauser, Horne, Shimony, and Holt (1969). The journal received the article submission on August 4, 1969.
10 On Townes, masers, lasers, and early skepticism about their compatibility with quantum mechanics, see Bromberg (1991), 17–19.
11 Clauser interview with Bromberg (2002), 12, 13, 69–71, 73, 74 ("Dumpster diving"); Freedman and Clauser (1972). On early experimental tests of Bell's theorem, see also Clauser and Shimony (1978); Freire (2006); and Gilder (2008), chaps. 29, 30.
12 Clauser interview with Bromberg (2002), 41, 42; Shimony interview with Bromberg (2002), 71, 74; Freedman and Clauser (1972); and *Science Citation Index* (1961–), s.v. "Bell, John S."
13 Shimony to Clauser, August 8, 1972, in *JFC*, reporting the attitude of the physics department chair at San Jose State College. See also Freire (2006), 604.
14 Rauscher interview (2008).
15 Rauscher interview (2008).
16 Rauscher interview (2008). See also Timothy Pfaff, "An interview with Elizabeth Rauscher," *California Monthly* (University of California Alumni Magazine), ca. 1979–80 (clipping in *EAR*). Rauscher's first article appeared as "Fundamentals of fusion" (1960). On cutbacks, see Anon. (1965), 16; and Clark (1966), 70.
17 Statistics on female physics degree recipients are calculated from data in Adkins (1975), 278–81.
18 Rauscher interview (2008). Rauscher likened her experiences during graduate school to those described in Keller (1977).
19 On the Livermore group, see E. A. Rauscher, flyer, "Ideals and purpose of the Tuesday night club" (1969), in *EAR*. On Rauscher's summer course, see *The Magnet* [Lawrence Berkeley Laboratory newsletter] 15 (December 1971): 5; "The philosophy of science," *The Magnet* 17 (June 1973): 1; "Philosophy of science course to start," *Beam Line* [Stanford Linear Accelerator Center newsletter] 3 (June 20, 1972): 2 ("rap sessions"); and R. B. Neal (acting director, SLAC), memo to "all hands," June 14, 1972, in *EAR*. On the late-1960s and early-1970s protests against physicists' facilities, see Kaiser (forthcoming), chap. 5. On more recent community-building efforts at Livermore, see Gusterson (1996).
20 Rauscher interview (2008). On Arthur Young and his institute, see also Mishlove (1975), 263–78.
21 Federici (1967), 5; and Anon. (1967a), back page.
22 See the photographs accompanying Pasolli (1966), 19; and Novick (1966), 17.
23 Saul-Paul Sirag, email to the author, July 18, 2010.
24 Saul-Paul Sirag, email to the author, December 11, 2007; Sirag (2002), esp. 97, 118. See also the list of seminars for the Institute for the Study of Consciousness, winter 1976, several of them led by Sirag; copy in *SPS*.
25 Nick Herbert, emails to the author, November 28, 2007, and December 1, 2007 ("no-