QUANTUM LEGACIES

Dispatches from an Uncertain World

DAVID KAISER

With a Foreword by Alan Lightman

The University of Chicago Press
Chicago and London

The University of Chicago Press, Chicago 60637
The University of Chicago Press, Ltd., London

Published 2020
Printed in the United States of America

29 28 27 26 25 24 23 22 21 20 1 2 3 4 5

ISBN-13: 978-0-226-69805-2 (cloth)
ISBN-13: 978-0-226-69819-9 (e-book)
DOI: https://doi.org/10.7208/chicago/9780226698199.001.0001

Library of Congress Cataloging-in-Publication Data

Names: Kaiser, David, author.
Title: Quantum legacies : dispatches from an uncertain world / David Kaiser ; with a foreword by Alan Lightman.
Description: Chicago : University of Chicago Press, 2020. | Includes bibliographical references and index.
Identifiers: LCCN 2019037201 | ISBN 9780226698052 (cloth) | ISBN 9780226698199 (ebook)
Subjects: LCSH: Quantum theory. | Physics—History.
Classification: LCC QC173.98.K358 2020 | DDC 530.12—dc23
LC record available at https://lccn.loc.gov/2019037201

♾ This paper meets the requirements of ANSI/NISO Z39.48–1992 (Permanence of Paper).

For Ellery and Toby:
quantum wondertwins

CONTENTS

8

Training Quantum Mechanics

In the autumn of 1961, Richard Feynman launched a new experiment. Together with several colleagues at Caltech, he aimed to overhaul the curriculum for physics students. Their main goal was to introduce students to some of the most exciting—yet abstruse—aspects of modern physics as early as possible, right in their first year as undergraduates. That way, they hoped, they could fire the young students' imaginations, rather than making them wade through important but staid topics first. The capstone of the new syllabus, filling the final third of the yearlong course, centered on quantum theory.[1]

Feynman and his colleagues, Robert Leighton and Matthew Sands, feverishly composed the new lectures. Feynman delivered each one with his usual gusto, after which Leighton and Sands transcribed the recordings. Before long, rumors of the new course reached several textbook publishers. Feynman and his colleagues got to name their own terms. Leighton drew up a form letter, instructing interested publishers to submit written proposals to the authors within three weeks—a reversal of the normal procedure, in which authors submitted proposals to the publishers! The publishers were to describe how quickly they would be able to produce the books, at what price the new textbooks would be sold, what share of the royalties would be paid to Caltech, and what additional expenses the publishers proposed to absorb.[2]

In the end, Feynman and his colleagues chose to work with Addison-Wesley. Before the books came out, a sales representative took galleys on a tour to gauge interest among other physics faculty. Writing to the president of the press, the sales rep could hardly contain himself. "Comments: Great enthusiasm," began his long memo. "Where? In *every* department of physics, of course." Several faculty seemed to be amazed by the new book. "It took me a life time to leave his room *with* the Feynman book, he just wanted to read another chapter and another one!!" Another professor tried to brush him off, until the crafty salesman flashed the book's red covers. "Well . . . we had a nice talk for fifteen minutes and made an appointment for next spring. Of course he wanted a copy of the book." And so it went, town after town during the sales representative's two-week tour. "Give me a Feynman once or twice a year and I will do my job!" he closed. "I do not know who signed up Feynman, but I suggest that you owe him (not Feynman) a fine Turkey for his Christmas dinner!"[3]

The sales rep's instincts proved accurate. *The Feynman Lectures on Physics* sold more than 130,000 copies within six years of publication—even though Feynman himself later conceded that the pedagogical experiment had been a bit too ambitious. Some of the material really did prove to be

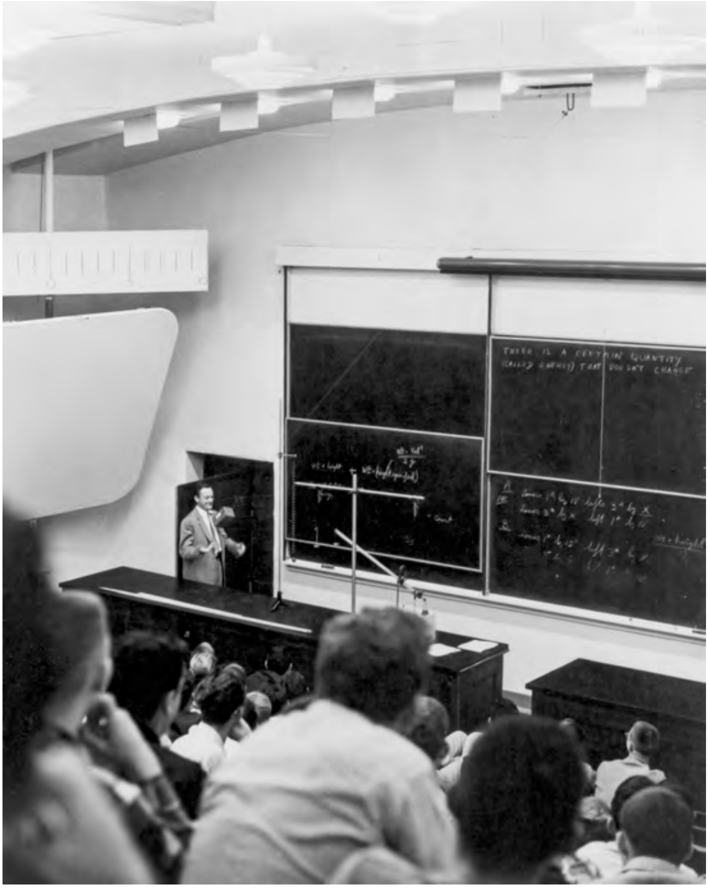

Figure 8.1. Richard Feynman lectures before a large undergraduate class at Caltech, ca. 1956. Recordings of lectures like these formed the basis for *The Feynman Lectures on Physics*, first published in 1963–65. (*Source*: Courtesy of the Archives, California Institute of Technology. Used with permission of the Melanie Jackson Agency, LLC.)

too advanced for first-year undergraduates. Yet sales remained brisk—indeed, the books remain in print today—driven largely by demand from more advanced students, and even faculty, who have continued to snatch up copies for self-study.[4]

Though Feynman, Leighton, and Sands might have gotten a bit ahead of the curve, by the early 1960s most of their colleagues shared their impulse to teach quantum theory to younger and younger students. Given how slowly university curricula usually evolve, the changes typified by *The Feynman Lectures* were extraordinary. Just twenty years before Feynman began lecturing on quantum theory to first-year undergraduates, many physicists in the United States had earned their PhDs without taking a single course in the subject.[5]

Amid the rapid-fire changes, some expressed alarm that too much was changing too quickly. A professor at Vanderbilt University, calling himself "one of those old-fashioned persons," suggested that "children should eat a reasonably good meal before partaking of dessert"—and, for this instructor at least, quantum theory was "distinctly dessert," which "could easily cause intellectual indigestion if not preceded by a properly balanced diet."[6]

Others, like J. Robert Oppenheimer, observed a more subtle shift: not just in what was being taught but how. Ever since the earliest work on quantum theory by the likes of Einstein and Schrödinger, Heisenberg and Dirac, the subject had inspired heated debate. So many of its core notions—the uncertainty principle, Schrödinger's cat, quantum entanglement—seemed to be at odds with other leading physical theories, let alone common sense. Yet when Oppenheimer surveyed how his colleagues taught quantum theory just a few years before Feynman, Leighton, and Sands launched their new course, he noted that the subject was by then "taught not as history, not as a great adventure in human understanding, but as a piece of knowledge, as a set of techniques, as a scientific discipline to be used by the student in understanding and exploring new phenomena."

Quantum mechanics had become "an instrument of the scientist to be taken for granted by him, to be used by him, to be taught as a mode of action, as we teach our children to spell and add."[7]

How much things had changed. Oppenheimer had been among the first to bring working knowledge of the still-new quantum theory back to the United States, following his studies in Europe in the mid-1920s; before long, his course on the subject at Berkeley had become legendary. Yet by the time most of his colleagues began to offer their own courses on the subject, after the Second World War, the dramas of the wartime projects and the ensuing hyperinflation of physics enrollments had affected nearly every aspect of young physicists' training. The transition that Oppenheimer noted, perhaps a bit wistfully, in the 1950s—teaching quantum mechanics more as a toolkit than an adventure—became emblematic of broader shifts in the field after the war. Facing runaway enrollments, many physicists across the United States winnowed the range of what would count as "quantum mechanics" in the classroom. Where once-fabled teachers like Oppenheimer had relished talking through thorny conceptual challenges with small groups of students, instructors after the war—their intimate classrooms by then replaced by large lecture halls, tiered rows of seats teeming with students—increasingly aimed to train quantum *mechanics*: skilled calculators of the atomic domain.

: : :

Oppenheimer's own entry into physics had been meteoric. Born in 1904 to a family of wealthy Jewish immigrants in New York City, he skipped several grades during his secondary schooling and entered Harvard for his undergraduate studies. (He later described his young self as "an unctuous, repulsively good little boy.") At Harvard he piled on extra courses each semester, graduating in just three years. During his first year as an undergraduate, he was invited to skip the introductory physics courses and dive directly into doctoral-level coursework.[8]

As was typical at the time for the best American students who were interested in theoretical physics, Oppenheimer next set off for Europe to pursue his PhD, studying first at Cambridge before transferring to Göttingen. There he studied under Max Born, just as Born was collaborating with Werner Heisenberg and others to craft the new quantum mechanics and working frantically to try to make sense of the strange new formalism. Oppenheimer absorbed the emerging material quickly, publishing a dozen research articles while in Göttingen. He completed his PhD in the spring of 1927, a month shy of his twenty-third birthday.[9]

After a brief postdoctoral fellowship, Oppenheimer accepted teaching appointments in 1929 at both the University of California at Berkeley and at Caltech—more than 370 miles apart. Each department was so eager to hire him that they reached a compromise: Oppenheimer would teach at Berkeley in the fall and then decamp to Caltech for the winter and spring terms. During his first semester at Berkeley, he taught an elective course for graduate students on quantum mechanics. One student registered for credit, while twenty-five signed on to listen. During that first course, Oppenheimer raced through the material so quickly that students complained to the department chair; Oppenheimer grumbled, in turn, that he had to crawl so slowly through the syllabus. Before long, however, he developed an engaging lecturing style.[10] Graduate students routinely sat through his Berkeley course on quantum mechanics more than once; one desperate student staged a hunger strike until Oppenheimer relented and allowed her to attend the class for a fourth time.[11]

As late as 1939—the year that one of Oppenheimer's graduate students transcribed the lectures and made hectographed copies, which quickly saw wide circulation—Oppenheimer still introduced quantum mechanics as a "radical solution" to problems that were as much philosophical as physical. Lecture after lecture, he focused not only on the new mathematical formalism, centered on Schrödinger's wave function, ψ, but also on its curious physical interpretation. He lingered over Born's interpretation of $|\psi|^2$ as yielding probabilities for various outcomes, emphasizing the remarkable conceptual break from the rigid determinism of classical physics. Wielding Newton's laws or even Einstein's relativity, physicists had long since been able to calculate that *B* strictly followed *A*. In the newer world of quantum theory, on the other hand, physicists could calculate only likelihoods: *B* had certain odds to follow *A*, and physicists remained utterly stymied from saying more. Oppenheimer even indulged in Einstein-styled attempts to circumvent Heisenberg's uncertainty principle—revealing, with a flourish each time, how such clever efforts were destined to fail—all before walking his students through the first practical calculations with the formalism.[12]

Oppenheimer's pedagogical approach was hardly unique at the time. Felix Bloch, a Jewish émigré from Switzerland who had studied with Heisenberg before fleeing Nazism in 1934, taught his graduate-level course on quantum mechanics at Stanford University in a remarkably similar way. Throughout the 1930s, meanwhile, Caltech graduate students faced tough questions about the interpretation of quantum mechanics on their qualifying examinations. For years, beginning in 1929, the Caltech students kept communal notebooks in which they recorded how they had prepared for their oral exams and what questions various examiners had posed. Well into the late 1930s, faculty had pressed students to talk "all about [the] ψ function, physical meaning, etc." or had asked, "What is [the] interpretation of $\psi(x)$? Does the Schrödinger equation describe the rate of change for *all* time?"—a subtle question about how the range of probabilities encoded in the wave function reduces to a single, measured result. Then came the follow-up: "Discuss the nature of observation in quantum mechanics and in classical mechanics."[13]

The first textbooks on quantum mechanics by physicists in the United States likewise emphasized in their opening pages that students would need to confront "philosophical difficulties," which could not be "exorcised." Some even paused, in the midst of what would soon become a standard calculation of an electron's energy levels within a hydrogen atom, to assess whether various mathematical solutions could be considered physically meaningful if no experiment could distinguish between them. Others included entire chapters with titles like "Observation and Interpretation." Reviewers of the textbooks during the 1930s agreed that an overtly philosophical register was appropriate when it came to teaching quantum mechanics. They often disagreed with specific points of interpretation in the books under review, but not with the notion that textbooks should broach such interpretive issues.[14]

: : :

Soon after the war, as more physics departments across the country began offering courses on quantum mechanics, the style of instruction began to shift. Few instructors during the 1950s lingered over how best to interpret the uncertainty principle or the place of probabilities in the quantum-mechanical formalism. Fewer still paused to dissect the philosophical standing of various hydrogenic wave functions.

The changes came on quickly. Some Caltech students, having studied reports of earlier oral exams, were caught by surprise. One complained in 1953 that the effort he had "invested in analysis of paradoxes and queer logical points was of no use in the exam." Instead, he had faced "straightforward questions" about then-standard calculations. Others similarly advised their fellow students to "give the usual spiel" or the "standard response" when asked to perform various quantum-mechanical calculations. One student suggested that his peers should simply "memorize" and "rehearse" answers to what had by then emerged as the standard calculations. Across the country, graduate students experienced a similar shift. Expansive essay-style questions about matters of interpretation, which had been common as late as the 1940s on the written qualifying exams, from Stanford and Berkeley to the Universities of Chicago and Pennsylvania, Columbia University, and MIT, were replaced by the mid-1950s by a collection of standard calculations.[15]

The pedagogical shift was closely correlated with enrollment patterns. Whereas Oppenheimer had lectured to about two dozen students at a time in his Berkeley course during the 1930s, after the war enrollments rapidly began to rise. By the mid-1950s, courses on quantum mechanics aimed at first-year graduate students typically had forty to sixty students enrolled; in the nation's largest departments, at Berkeley and MIT, the number edged over one hundred, "a disgrace [that] should not be tolerated at any respectable university," Berkeley's department chair complained to the dean. In a handful of departments, however, enrollments in first-year graduate-level quantum mechanics courses remained fairly small during the early 1950s or began to grow only later. Reading through lecture notes from these various departments reveals some remarkable differences. In short, an increase by a factor of three in enrollments was correlated with a decrease by a factor of five in the proportion of time devoted to the conceptual puzzles or philosophical challenges of quantum theory.[16]

Beyond the numbers and statistics, the lecture notes themselves provide some stark contrasts. Consider the course that Lothar Nordheim taught at Duke University in the spring of 1950. Like so many of his colleagues across the country, Nordheim had spent the war years working on the Manhattan Project. He had served as section chief at the Oak Ridge laboratory in Tennessee (principal site for isolating the fissionable isotope of uranium, U-235), rising to direct its physics division between 1945 and 1947. He left Oak Ridge for Duke in 1947 but did not stay long: by autumn 1950, he had begun to work full-time on the top-secret hydrogen-bomb project and later chaired the theoretical physics division at the major nuclear-related defense contractor General Atomics. In short, Nordheim was no stranger to the new realities of the military-industrial complex, and he excelled at wringing practical results, often under extreme time pressures, from the equations of quantum theory.[17]

Yet when he taught his course on quantum mechanics at Duke in 1950, Nordheim insisted that his students focus on its conceptual challenges. Working with a small class of a dozen students, he launched into the stubborn strangeness of quantum mechanics in his very first lecture. Given the new restriction to probabilities, he asked his students:

"What does this do to causality?" A student recorded in his notes, simply, "Ans[wer]. It Fucks it!" To drive the point home, Nordheim devoted two more lectures to the fabled double-slit experiment—a favorite example that Heisenberg and Schrödinger had each introduced in his own teaching, back in the early days of quantum theory, to emphasize such quintessential quantum features as wave-particle duality, superposition, and the uncertainty principle. Likewise for Nordheim's treatment of a quantum particle tunneling through a barrier. As he described the counterintuitive process, he pressed his students, "it is meaningless to ask, 'Is there causality?,' because we can never know the state completely at any time, because of [the] uncertainty relation. Hence, we discard the classical physical ideas of idealized observations."[18]

In other classrooms across the country, physicists who had shared many of Nordheim's worldly experiences—the secret, massive wartime projects, major consulting for defense projects after the war—charted a very different course when lecturing on quantum mechanics to their own graduate students. Where Nordheim lectured to a dozen students, most of these others faced classes that had already grown several times larger. At Chicago, Enrico Fermi spent twice as long deriving properties of the Laguerre polynomials—mathematical functions that quantify the behavior of an electron in a hydrogen atom—as he did on Heisenberg's uncertainty principle. At Cornell, Hans Bethe observed, with one passing remark, that trying to circumvent the uncertainty principle was as fruitless as designing perpetual motion machines, full stop. Even Richard Feynman, full of exuberance about bringing quantum theory to younger and younger students, made clear in his own classroom that the real purpose was to learn to calculate. In the lecture notes from his graduate-level course on quantum mechanics, he admonished that interpretive issues—of the sort that had filled Oppenheimer's lectures before the war and Nordheim's lectures after it—were all "in the nature of philosophical questions. They are not necessary for the further development of physics." While Nordheim had paused to consider conceptual sticking points of quantum tunneling, Freeman Dyson, lecturing to a class at Cornell with nearly three times as many students as Nordheim's class at Duke, plowed forward, adapting the usual calculation to treat various states of nuclear matter, such as deuterons. Dyson made clear, in his first lecture, that he would not follow the chosen textbook very closely. "Too much philosophy."[19]

: : :

Two well-known textbooks, both published soon after the war, further illustrate the trend: Leonard Schiff's *Quantum Mechanics* (1949) and David Bohm's *Quantum Theory* (1951). Schiff and Bohm had each studied with Oppenheimer in Berkeley during the 1930s; both authors acknowledged how influential Oppenheimer's course had been for their own teaching. Yet what seemed like complementary models for teaching the subject—remarkably different in their emphases, yet equally hailed as great successes upon publication—soon collapsed under the pressure of rising student numbers.[20]

Leonard Schiff had been a postdoc with Oppenheimer between 1937 and 1940. He later joined the faculty at Stanford, and his *Quantum Mechanics* first appeared in 1949 to rave reviews. Schiff's book exemplified the toolkit approach to quantum mechanics. Whereas Oppenheimer had made his way slowly to the details of the Schrödinger equation, pausing at length to entertain many of the conceptual quandaries that arose along the way, Schiff largely dispensed with such philosophical niceties. ("We shall discuss physics, *not* philosophy," he announced on the first day of one of his courses in 1959.)[21] What had occupied nearly 20 percent of Oppenheimer's lecture notes, Schiff dispatched in a few opening pages of his book. In its place, Schiff provided what was widely hailed as the best collection of homework problems to calculate, of just the right level of difficulty for his target readers.[22]

David Bohm completed his PhD under Oppenheimer's direction in 1942 and published his *Quantum Theory* in 1951 after teaching at Princeton for a few years. He had tested out the material for his book in classes during 1947 and 1948, before Princeton's physics department had swelled too large. (His enrollments in those years were around twenty students in each class, similar in size to Oppenheimer's course at Berkeley in the 1930s.) Like Schiff's book, Bohm's book received glowing reviews at first—"a rare example of expressive, clear scientific writing," proclaimed one satisfied reviewer. In contrast to Schiff's approach, Bohm devoted several opening chapters to the kinds of philosophical challenges and conceptual puzzles that Oppenheimer, too, had emphasized. The Schrödinger equation didn't even appear until page 191 in Bohm's book; Schiff had first introduced the equation on page 21.[23]

The conceptual care that Bohm had taken when composing his textbook impressed several of his earliest reviewers. One praised "the concise and well balanced interplay, point-counterpoint, between formalism and interpretation." Another compared Bohm's and Schiff's books side by side—Schiff's being the only obvious American competitor published since the war—and offered the following balance sheet. Though only two-thirds as long, Schiff's book treated many more applications of the formalism in greater detail. Yet for those topics treated by both authors, this reviewer continued, "it is to the credit of Bohm's book that, for example, it gives the clearer and more physically understandable explanation."[24]

Despite their equally promising starts, the two books—like their authors—suffered quite different fates. Schiff became department head at Stanford and soon editor of the influential textbook series published by McGraw-Hill in which his own book had appeared. Bohm, meanwhile, was forced from his position at Princeton—and soon forced out of the country—just months after his book had been published. He had refused to name names when subpoenaed to testify before the House Un-American Activities Committee, during its headline-grabbing investigation into alleged "Communist infiltration" of the wartime Manhattan Project. Bohm fled to Brazil—where, in between crippling bouts of nausea, he was compelled to forfeit his US passport—before moving a few years later to Israel, eventually settling in London. Schiff's book saw two widely heralded, updated editions (in 1955 and 1968); Bohm's book was never reissued during his lifetime, and his efforts to publish a follow-up textbook on quantum mechanics were rebuffed.[25]

It fell to a third veteran of Oppenheimer's Berkeley group, Edward Gerjuoy, to make sense of the diverging paths. He took up the comparison in a review of the second edition of Schiff's book, in the mid-1950s. In expanding his book, Schiff had devoted even less space to conceptual or interpretive discussion; to Gerjuoy's taste, each edition of Schiff's book devoted too little attention to "such questions as correspondence, uncertainty, complementarity, and causality"—precisely the topics that had filled so much of Bohm's book. (Gerjuoy noted that "the contrast with Bohm's

Quantum Theory is interesting, even amusing.") But Gerjuoy could understand Schiff's decision not to amplify these topics in his revised edition. "With these subjects lecturing is of little avail—the baffled student hardly knows what to write down, and what notes he does take are almost certain to horrify the instructor, who perspicaciously usually resolutely refuses to question his students on these topics." So, instead, Schiff focused on a cache of worked examples: too soon, the student "is well into detailed algebraic complexities verifying which, he readily persuades himself to believe, means he is learning quantum mechanics." Though Gerjuoy could understand Schiff's pedagogical choices, he wondered—perhaps thinking back to his experiences as a student in Oppenheimer's famous course at Berkeley—whether it was "necessary, as Schiff does, to leap so rapidly over the philosophical issues raised by quantum mechanics that the student never has a chance to gauge their depth."[26]

Despite Gerjuoy's cautions, Schiff's textbook rapidly became the standard-bearer, its collection of homework problems especially well geared to teaching large classes of students. When asked to evaluate whether a third edition of Schiff's book would be warranted, a professor at Berkeley responded with a sixteen-page memorandum on why the previous two editions had been so successful. "I believe that the explanation is that Schiff is a very *practical* book," the reviewer began. "The reader who goes through the book really obtains a working knowledge of quantum mechanics." A student using the book, this reviewer continued, is "taken through a number of well chosen applications, and he is shown, through these examples how it all works out." It was an approach that the Berkeley physicist could appreciate; he had learned the subject from the first edition of Schiff's book. "As a student I was perfectly happy with this mode of presentation, and the book kept me sufficiently busy to prevent pseudo-philosophical speculations about the True Meaning of quantum mechanics."[27]

Many other physicists across the United States offered similar appraisals. Where once reviewers had evaluated textbooks on quantum mechanics at least in part on the basis of their philosophical stance, reviewers throughout the 1950s and 1960s routinely praised the latest offerings for "avoid[ing] philosophical discussion" and for omitting "philosophically tainted questions" that distracted from the business of learning to calculate. Enough with the "musty atavistic to-do about position and momentum," stormed MIT's Herman Feshbach.[28]

The new approach shaped the contents of the books as well. Between 1949 and 1979, physicists in the United States published thirty-three textbooks on quantum mechanics aimed at first-year graduate students. Together, these books included 6,261 homework problems (including, of course, many duplicate problems that appeared in several books). Most required students to manipulate the equations in the text: make a change of variables in the Schrödinger equation or evaluate various integrals. Only about 10 percent of the problems pressed students to go beyond the equations, to discuss their calculations in words. The pattern alarmed at least some older physicists, who, like Oppenheimer, had witnessed the remarkable conceptual transformations of quantum theory firsthand. In the early 1960s, one grumbled that with the spate of new textbooks, his colleagues had confused what was "easy to teach"—the "technical mathematical aspects of the theory"—with the conceptual understanding that students needed most.[29]

After the enrollments had crashed, however, newer textbooks began to appear, with a markedly different mix of homework problems. For example, Robert Eisberg and Robert Resnick pulled together a draft of their massive book, *Quantum Physics of Atoms, Molecules, Solids, Nuclei, and Particles*, in the early 1970s. By the time their book was published in 1974, first-year graduate enrollments in physics—the population to whom the book was directed—had fallen more than 60 percent from their 1960s peak. Eisberg and Resnick's book reflected the new classroom realities. In addition to hundreds of quantitative problems, akin to the classics that filled all three editions of Leonard Schiff's book, Eisberg and Resnick also included long lists of "discussion questions" at the end of each chapter. "Does a blackbody always appear black? Explain the term blackbody" was one early example. "What is the fallacy in the following statement? 'Since a particle cannot be detected while tunneling through a barrier, it is senseless to say that the process actually happens'"—hearkening back to one of Lothar Nordheim's favorite examples from his course at Duke. In a similar way, more than half of the homework problems within *Quantum States of Atoms, Molecules, and Solids* (1976), written by a trio of physicists at Rice University, were of this qualitative, essay-type form.[30]

: : :

During the early 1950s, a young theoretical physicist at Berkeley learned the hard way how bloated class sizes could affect research and teaching styles. Having been on the faculty in Berkeley's physics department for a year and a half, the theorist was let go, not because he was unproductive in research or unconscientious in his teaching—the department chair insisted that the young professor had performed more than adequately at both. Rather, the theorist's chosen research topic fit poorly with the new pedagogical realities.

He had focused on rather abstruse points in quantum field theory. Though the topic could well prove important—the department chair considered it too early to say—it had failed an important test. Junior faculty members, the chair explained, needed to select research topics for themselves that could provide appropriate spin-off projects for their graduate students: "subjects that are not trivial, but at the same time are not unduly difficult or too time-consuming." Whether or not the young physicist's research would pan out in the long term, "it is not the sort of work that can readily be used for Ph.D. theses." With more than two hundred graduate students enrolled, Berkeley's physics department needed "someone who will be more useful to us." Only recently, in fact, the department chair had fast-tracked the promotion case for a different junior faculty member largely on the basis of his ability to craft appropriate problems for his many graduate students.[31]

Though few departments swelled as large or as quickly as Berkeley's, most felt the strain of the postwar enrollment boom. At nearby Stanford, physics faculty had prided themselves on the small-group intimacy their department could offer, compared with the "factory" at Berkeley. During the early 1950s, when the incoming cohorts included ten to twelve new students each year, Stanford faculty kept detailed notes on how individual students fared on their oral exams, the standard gateway between coursework and dissertation research: "Rather limited knowledge; shy, hesitant in answers; nervous," for example, or "well composed and thinks on his feet." Yet as the number of incoming students rose—soon up to thirty per year by the late 1950s, peaking at thirty-seven in 1969—the individualized notetaking stopped. The written exams shifted from essays to problems to calculate; faculty even flirted with administering true-false exams, to keep the burden of grading under control.[32] Physicists at the University of Illinois, facing similar pressures, jokingly called for a “test-ban treaty” in 1963—between faculty and students rather than the United States and Soviet Union—while students there lobbied for a “flunk-out shelter.”[33]

Then the bottom fell out: only eighteen graduate students entered Stanford’s department in 1970, and sixteen in 1972. Just as suddenly, the department once again undertook a sweeping reform of its comprehensive exams, restoring “a significant fraction of essay and discussion questions.” In September 1972, the revised exam featured short-answer or essay questions in 40 percent of the problems, nearly double the proportion in the previous decade’s exams. That same year, the department introduced a new, informal seminar on “speculations in physics”—just the sort of thing that had cost the young theorist at Berkeley his position twenty years earlier.[34] Richard Feynman took similar advantage of the transformed pedagogical realities at Caltech. He began to offer an informal course known as “Physics X,” open to any undergraduates who were eager to puzzle through juicy scientific questions. One of my favorite photographs shows Feynman gesturing at the blackboard in 1976—the suit and tie from his early-1960s *Feynman Lectures* days replaced by an open, wide collar—while a handful of students look on, some sporting headbands, feet propped up on a desk.[35]

There has never been one “best” way to teach quantum mechanics. In particular, the enrollment-driven pragmatism, so stark in American physics departments after the Second World War, was anything but a “dumbing down.” The second and third editions of Leonard Schiff’s acclaimed textbook, for example, contained homework problems aimed

Figure 8.2. Richard Feynman teaching his informal "Physics X" course at Caltech in 1976. (*Source*: Photograph by Floyd Clark, courtesy of the Archives, California Institute of Technology. Used with permission of the Melanie Jackson Agency, LLC.)

at entry-level graduate students that would have stumped leading physicists only a decade or two earlier. Yet that tremendous accumulation of calculating skill came with some unnoticed trade-offs. For every additional calculation of baroque complexity that physics students learned to tackle during the 1950s and 1960s, they spent correspondingly less time puzzling through what those fancy equations meant—what they implied about our understanding of the quantum world.[36] Different ideals—about quantum theory, about what it meant to be a physicist—flourished while enrollments bulged, and after they went bust.

24. David Berliner and Bruce Biddle, *The Manufactured Crisis: Myths, Fraud, and the Attack on America's Public Schools* (New York: Basic, 1995), 95–102; Daniel Greenberg, *Science, Money, and Politics: Political Triumph and Ethical Erosion* (Chicago: University of Chicago Press, 2001), chaps. 8–9; Eric Weinstein, "How and Why Government, Universities, and Industry Create Domestic Labor Shortages of Scientists and High-Tech Workers," unpublished working paper, http://www.nber.orb/~peat/Papers/Folder/Papers/SG/NSF.html; and Lucena, *Defending the Nation*, 104–12, 133. See also Teitelbaum, *Falling Behind?*

25. See esp. Lucena, *Defending the Nation*, chap. 4.

26. Berliner and Biddle, *Manufactured Crisis*; Greenberg, *Science, Money, and Politics*; and Lucena, *Defending the Nation*.

27. Cf. Jeremy Bernstein, *Physicists on Wall Street and Other Essays on Science and Society* (New York: Springer, 2008).

Chapter 8

1. Richard Feynman, Robert Leighton, and Matthew Sands, *The Feynman Lectures on Physics*, 3 vols. (Reading, MA: Addison-Wesley, 1963–65).

2. Feynman, Leighton, and Sands, *Feynman Lectures*, 1:3–5. See also Richard C. M. Jones to Robert B. Leighton, 16 April 1962, and Leighton to Earl Tondreau, 27 March 1963, both in box 1, folder 1, Robert B. Leighton Papers, California Institute of Technology Archives, Pasadena, CA.

3. Leo Bauer to M. W. Cummings, 7 November 1963, in box 1, folder 2, Leighton Papers (emphasis in original).

4. On sales figures, see unsigned memo, ca. November 1968, in box 1, folder 2, Leighton Papers. On the enduring interest in the books, see Robert P. Crease, "Feynman's Failings," *Physics World* 27 (March 2014): 25.

5. Hans Bethe, "30 Years of Physics at Cornell" (ca. 1965), 10, in HAB box 3, folder 21; A. Carl Helmholz, interview with the author, Berkeley, 14 July 1998; and W. C. Kelly, "Survey of Education in Physics in Universities in the United States," 1 December 1962, in box 9, American Institute of Physics, Education and Manpower Division records, collection number AR15, Niels Bohr Library, American Institute of Physics, College Park, MD. See also Victor F. Weisskopf, "Quantum Mechanics," *Science* 109 (22 April 1949): 407–8; and David R. Inglis, "Quantum Theory," *American Journal of Physics*

20 (November 1952): 522–23. See also Stanley Coben, "The Scientific Establishment and the Transmission of Quantum Mechanics to the United States, 1919–32," *American Historical Review* 76 (1971): 442–60; Gerald Holton, "On the Hesitant Rise of Quantum Mechanics Research in the United States," in *Thematic Origins of Scientific Thought*, 2nd ed. (Cambridge, MA: Harvard University Press, 1988), 147–87; and Katherine Sopka, *Quantum Physics in America: The Years through 1935* (New York: American Institute of Physics, 1988).

6. Francis G. Slack, "Introduction to Atomic Physics," *American Journal of Physics* 17 (November 1949): 454.

7. J. Robert Oppenheimer, *Science and the Common Understanding* (New York: Simon and Schuster, 1953), 36–37.

8. See esp. Kai Bird and Martin J. Sherwin, *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (New York: Knopf, 2005), chaps. 1–2; and Charles Thorpe, *Oppenheimer: The Tragic Intellect* (Chicago: University of Chicago Press, 2006), chap. 2. Oppenheimer quoted in "The Eternal Apprentice," *Time*, 8 November 1948, 70–81, on 70 ("unctuous").

9. Bird and Sherwin, *American Prometheus*, chaps. 2–3.

10. Raymond T. Birge, "History of the Physics Department," 5 vols., in vol. 3, chap. 9, p. 31. Birge's "History" is available in the Bancroft Library, University of California–Berkeley.

11. Bird and Sherwin, *American Prometheus*, 84. See also David Cassidy, "From Theoretical Physics to the Bomb: J. Robert Oppenheimer and the American School of Theoretical Physics," in *Reappraising Oppenheimer: Centennial Studies and Reflections*, ed. Cathryn Carson and David A. Hollinger (Berkeley: University of California Press, 2005), 13–29.

12. Copies of Bernard Peter's notes from Oppenheimer's 1939 Berkeley course (Physics 221) are available in several university libraries, including Caltech and Berkeley. As late as 1947, administrative staff in Berkeley's physics department still fielded repeated requests for copies of Oppenheimer's 1939 lecture notes; see correspondence in box 4, folder 16, University of California–Berkeley, Department of Physics records, collection number CU-68, Bancroft Library, University of California–Berkeley.

13. Felix Bloch's handwritten lecture notes from the mid-1930s are available in FB box 16, folders 13–14. The Caltech communal notebooks were called the "Bone Books" and span 1929–69; they are available in the Caltech archives, Pasadena, CA. See esp. entries by

Sherwood K. Haynes, 6 January 1936, in box 1, vol. 2; and by Martin Summerfield, 10 March 1939, in box 1, vol. 3 (emphasis in original).

14. Edward Condon and Philip Morse, *Quantum Mechanics* (New York: McGraw-Hill, 1929), 1, 2, 7, 10, 17–21, 83; and Edwin Kemble, "The General Principles of Quantum Mechanics, Part 1," *Physical Review Supplement* 1 (1929): 157–215, on 157–58, 175–77. Cf. Arthur Ruark and Harold Urey, *Atoms, Molecules, and Quanta* (New York: McGraw-Hill, 1930); Alfred Landé, *Principles of Quantum Mechanics* (New York: Macmillan, 1937); and Edwin Kemble, *The Fundamental Principles of Quantum Mechanics* (New York: McGraw-Hill, 1937). On reviews, see Paul Epstein, "Quantum Mechanics," *Science* 81 (28 June 1935): 640–41; E. U. Condon, "Quantum Mechanics," *Science* 31 (31 January 1935): 105–6; "Foundations of Physics," *American Physics Teacher* 4 (September 1936): 148; Karl Lark-Horovitz, "Quantum Mechanics," *Science* 87 (1 April 1938): 302; L. H. Thomas, "Quantum Mechanics," *Science* 88 (2 September 1938): 217–19; and "The Fundamental Principles of Quantum Mechanics," *American Physics Teacher* 6 (October 1938): 287–88. My discussion of interwar trends in teaching quantum mechanics within the United States is indebted to several pioneering works, though I find much greater emphasis upon philosophical engagement in the extant teaching materials than has previously been noted. See esp. Silvan S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States, 1920–1950," *Historical Studies in the Physical Sciences* 17 (1986): 55–98; Nancy Cartwright, "Philosophical Problems of Quantum Theory: The Response of American Physicists," in *The Probabilistic Revolution*, ed. Lorenz Krüger, Gerg Gigerenzer, and Mary S. Morgan (Cambridge, MA: MIT Press, 1987), 2:417–35; Alexi Assmus, "The Molecular Tradition in Early Quantum Theory," *Historical Studies in the Physical and Biological Sciences* 22 (1992): 209–31; and Alexi Assmus, "The Americanization of Molecular Physics," *Historical Studies in the Physical and Biological Sciences* 23 (1992): 1–34.

15. Caltech Bone Book entries: Michael Cohen, 14 May 1953, in box 1, vol. 7; Frederick Zachariasen, 27 May 1953, in box 1, vol. 7; and Kenneth Kellerman, 10 April 1961, in box 1, vol. 9. Copies of the written comprehensive and qualifying exams may be found in LIS box 9, folder "Misc. problems"; in FB box 10, folder 19; in box 3, folder 4, University of California–Berkeley, Department of Physics records, collection number CU-68, Bancroft Library; and in Kelly, "Survey of Education," appendix 19.

16. Raymond T. Birge to E. W. Strong, 30 August 1950 ("disgrace"), in RTB. See also David Kaiser, *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival* (New York: W. W. Norton, 2011), 18–19.

17. Jacques Cattell, ed., *American Men of Science*, 10th ed. (Tempe, AZ: Jacques Cattell Press, 1960), s.v. "Nordheim, Dr. L(othar) W(olfgang)." See also William Laurence, "Teller Indicates Reds Gain on Bomb," *New York Times*, 4 July 1954; and John A. Wheeler with Kenneth Ford, *Geons, Black Holes, and Quantum Foam: A Life in Physics* (New York: W. W. Norton, 1998), 202–4.

18. Paul F. Zweifel's handwritten notes on Nordheim's 1950 course at Duke University are available in the Niels Bohr Library, American Institute of Physics; see pp. 8–11, 38–39, 58.

19. Freeman Dyson's handwritten lecture notes from his courses at Cornell (1952) and Princeton (1961), in Professor Dyson's possession, Institute for Advanced Study, Princeton; Enrico Fermi, *Notes on Quantum Mechanics* (1961), 2nd ed. (Chicago: University of Chicago Press, 1995), which reproduces the mimeographed handwritten lecture notes that Fermi distributed to his class at the University of Chicago (1954); Elisha Huggins's handwritten notes on Richard Feynman's course at Caltech (1955), in Professor Huggins's possession, Dartmouth College; Hans Bethe's handwritten lecture notes from Cornell (1957), in HAB box 1, folder 26; Evelyn Fox Keller's handwritten notes on Wendell Furry's course at Harvard (1957), in Professor Keller's possession, MIT; Saul Epstein, "Lecture Notes in Quantum Mechanics" (1958), mimeographed typed lecture notes, available in the University of Nebraska Physics Library, Lincoln; and Edward L. Hill, "Lecture Notes on Quantum Mechanics" (1958), mimeographed typed lecture notes, available in the University of Minnesota Physics Library, Minneapolis. For each course, I was able to estimate enrollments based on PhD conferrals from those departments four and five years later (taking into account average degree-completion times from that era). In those cases for which archival information about actual enrollments remains available, the estimates based on later PhD conferrals matched actual enrollments: Julia Gardner (reference librarian, University of Chicago), email to the author, 16 September 2005; Bethe's course grade sheet available in HAB box 1, folder 26; Roger D. Kirby (chair, Department of Physics, University of Nebraska), email to the author, 15 September 2005; and Mary N. Morley (registrar, Caltech), email to the author, 13 September 2005.

20. Leonard I. Schiff, *Quantum Mechanics* (New York: McGraw-Hill, 1949), xi; and David Bohm, *Quantum Theory* (New York: Prentice Hall, 1951), v.

21. Schiff's handwritten lecture notes from fall 1959, in LIS box 8, folder "Sr. Colloquium 'Relativity and Uncertainty'" (emphasis in original).

22. See reviews of various editions of Schiff's textbook: Weisskopf, "Quantum Mechanics"; Morton Hammermesh, "Quantum Mechanics," *American Journal of Physics* 17 (November 1949): 453–54; Abraham Klein, "Quantum Mechanics," *Physics Today* 23 (May 1970): 70–71; and John Gardner, "Quantum Mechanics," *American Journal of Physics* 41 (1973): 599–600.

23. E. M. Corson, "Quantum Theory," *Physics Today* 5 (February 1952): 23–24 ("rare example").

24. Corson, "Quantum Theory," 23–24 ("concise and well balanced"); and Inglis, "Quantum Theory," 522–23 ("credit of Bohm's book").

25. On Bohm's case, see esp. Ellen Schrecker, *No Ivory Tower: McCarthyism and the Universities* (New York: Oxford University Press, 1986), 135–37, 142–44; F. David Peat, *Infinite Potential: The Life and Times of David Bohm* (Reading, MA: Addison-Wesley, 1997), chaps. 5–8; Russell Olwell, "Physical Isolation and Marginalization in Physics: David Bohm's Cold War Exile," *Isis* 90 (1999): 738–56; Olival Freire, "Science and Exile: David Bohm, the Cold War, and a New Interpretation of Quantum Mechanics," *Historical Studies in the Physical and Biological Sciences* 36 (2005): 1–34; and Shawn Mullet, "Little Man: Four Junior Physicists and the Red Scare Experience" (PhD diss., Harvard University, 2008), chap. 4. On sales of Schiff's book, see Malcolm Johnson to Leonard Schiff, 11 March 1964, in LIS box 9, folder "Schiff: Quantum mechanics." In 1989, Dover Publications issued a reprint of Bohm's 1951 textbook. On Bohm's failed efforts to publish a follow-up textbook, see the correspondence in LIS box 13, folder "Bohm."

26. Edward Gerjuoy, "Quantum Mechanics," *American Journal of Physics* 24 (February 1956): 118.

27. Eyvind Wichmann, "Comments on Quantum Mechanics, by L. I. Schiff (Second Edition)," n.d. (ca. January 1965), in LIS box 9, folder "Schiff: Quantum mechanics" (emphasis in original).

28. Jacques Romain, "Introduction to Quantum Mechanics," *Physics Today* 13 (April 1960): 62 ("avoids philosophical discussion"); D. L. Falkoff, "Principles of Quantum Mechanics," *American Journal of Physics* 20 (October 1952): 460–61 ("philosophically tainted ques-

tions"); and Herman Feshbach, "Clear and Perspicuous," *Science* 136 (11 May 1962): 514 ("musty atavistic to-do").

29. George Uhlenbeck, "Quantum Theory," *Science* 140 (24 May 1963): 886. Statistics on textbook publications come from keyword and call-number searches in the online catalog of the US Library of Congress: http://www.loc.gov. With the aid of several research assistants, I copied every homework problem within this set of textbooks and coded each by whether the problem required students to perform a calculation or to describe a physical effect in short-answer or essay form.

30. Robert Eisberg and Robert Resnick, *Quantum Physics of Atoms, Molecules, Solids, Nuclei, and Particles* (New York: Wiley, 1974), vi, 25, 245, 322; Michael A. Morrison, Thomas L. Estle, and Neal F. Lane, *Quantum States of Atoms, Molecules, and Solids* (Englewood Cliffs, NJ: Prentice-Hall, 1976), xv; Robert Eisberg, email to the author, 7 October 2005; and Robert Resnick, email to the author, 11 October 2005. Enrollment changes calculated from data in the annual American Institute of Physics graduate-student surveys, 1961–75, available in American Institute of Physics, Education and Manpower Division records, collection number AR15, Niels Bohr Library. One may find a similar shift in the types of homework problems included in textbooks on quantum mechanics aimed at undergraduates. Compare, e.g., A. P. French, *Principles of Modern Physics* (New York: Wiley, 1958), with A. P. French and Edwin F. Taylor, *An Introduction to Quantum Physics* (New York: W. W. Norton, 1978). Moreover, one finds that textbooks written by physicists in other countries fit the same pattern regarding correlations between pedagogical style and enrollments. Textbooks on quantum mechanics after the Second World War by authors in the United Kingdom and the Soviet Union, which each experienced surges in physics enrollments, included only a small proportion of discussion-style homework problems until the enrollments fell. Physicists in other European countries, such as France, West Germany, and Austria—which did not experience a large spike in physics enrollments after the war—continued to publish textbooks similar to the interwar models, with lengthy chapters on philosophical interpretations of the quantum-mechanical formalism.

31. Raymond T. Birge to E. B. Roessler, 29 November 1952 ("subjects that are not trivial"); Birge to K. T. Bainbridge, 11 February 1953 ("not the sort of work"); and Birge to Alfred Kelleher, 3 November 1954, all in RTB. On the other promotion case, see Birge to Dean A. R. Davis, 9 April 1951, in RTB.

32. On incoming graduate-student enrollments in Stanford's physics department, see faculty meeting minutes, 12 January 1970, in FB box 12, folder 1; and unsigned memo, "Graduate Enrollment and Projection," 2 February 1972, in FB box 12, folder 8. On fears of becoming a "factory," see Paul Kirkpatrick, memo to Stanford physics department faculty, 19 January 1956, in FB box 10, folder 2; and Ed Jaynes, memo to department faculty, 27 April 1956, in FB box 10, folder 3. See also the anonymous memos on comprehensive exam results, 7 and 14 April 1956, in FB box 10, folder 3 ("Rather limited knowledge"); 2 February 1958, in FB box 10, folder 8; W. E. Meyerhof, minutes of Graduate Study Committee meeting, 4 November 1959, in FB box 10, folder 12; and Felix Bloch, "Oral Examinations" memorandum, 9 May 1961, in FB box 10, folder 16.

33. "Faculty Skit 1963," available in University of Illinois at Urbana–Champaign, Department of Physics, Faculty Skits, 1963–73, deposited in the Niels Bohr Library.

34. A. L. Fetter memo to department faculty, 28 February 1972, in FB box 12, folder 8; and comprehensive exam (21–22 September 1972) in FB box 12, folder 10. On the new seminar, see W. E. Meyerhof, memo to Stanford's physics graduate students, 29 September 1972, in FB box 12, folder 10.

35. On Feynman's "Physics X" course, see James Gleick, *Genius: The Life and Science of Richard Feynman* (New York: Pantheon, 1992), 398–99.

36. Kaiser, *How the Hippies Saved Physics*, 19–20.

Chapter 9

Versions of this essay appeared in David Kaiser, *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival* (New York: W. W. Norton, 2011), chap. 7; and in *Isis* 103 (2012): 126–38.

1. See also David Kaiser and W. Patrick McCray, eds., *Groovy Science: Knowledge, Innovation, and American Counterculture* (Chicago: University of Chicago Press, 2016).

2. Fritjof Capra, *The Tao of Physics: An Exploration of the Parallels between Modern Physics and Eastern Mysticism* (Boulder, CO: Shambhala, 1975).

3. Fritjof Capra, *Uncommon Wisdom: Conversations with Remarkable People* (New York: Simon and Schuster, 1988), 22–25. The Santa Cruz physicist who invited Capra was Michael Nauenberg; see Nauenberg