Training Quantum Mechanics:
Enrollments and Epistemology in Modern Physics

David Kaiser

Program in Science, Technology, and Society, MIT
and Department of Physics, MIT

Building E51-185
Massachusetts Institute of Technology
77 Massachusetts Avenue
Cambridge, MA 02139
dikaiser@mit.edu

Abstract[*]. The classrooms of American colleges and universities bulged like never before after World War II. Several major changes, including the G. I. Bill, brought over two million veterans into the nation's institutions of higher education. Enrollments in nearly every department rose exponentially. Yet graduate enrollments in physics departments grew fastest, at almost twice the rate of all other fields combined. Twenty-five years later, enrollments in nearly all fields underwent a major contraction; again, physics led the way, falling faster and deeper than any other field. This paper examines some effects of these violent demographic swings on the subject of physics itself. How, in short, did the enrollment bubble affect the stuff of knowledge? I focus on how the subject of quantum mechanics—physicists' description of matter at the atomic scale, the cornerstone of modern physics—was taught in the decades after World War II. Faced with runaway enrollments, American physicists re-crafted the subject in the classroom, accentuating those elements that could lend themselves to high throughput pedagogy—a cache of exemplars that could be routinized into problem sets and exam questions—while quietly dropping the last vestiges of "philosophy" or "interpretation" that had long been considered crucial to understanding the topic.

Introduction: The Cold War Bubble

"Physical scientists are in vogue these days," announced a commentator in *Harper's* a few years after the end of World War II. "No dinner party is a success without at least one physicist." Wartime projects, including the atomic bomb and radar, thrust American physicists out of their backwater status and into new relationships with

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federal (mostly military) patrons. In the afterglow of the atomic blasts over Hiroshima and Nagasaki—which most Americans credited with ending World War II—physicists moved to center stage in ways that no group of academics had ever done in this country. Physicists' mundane travels became draped with strange new fanfare. Police motorcades escorted twenty young physicists on their way to a private conference near Long Island in 1947, and a local booster sponsored a steak dinner en route for the startled guests of honor. B-25 bombers began to shuttle highly placed physicists-turned-government-advisors when civilian modes of transportation proved inconvenient. During the postwar years, military officials and heads of state sought out physicists for advice, often on topics far removed from the scientists' main areas of specialty. Long before President Kennedy began tapping Ivy League social scientists to be his advisors, physicists in the United States were already treated as "the best and the brightest."[1]

Driven by this sudden sea-change in physicists' intellectual, political, and cultural roles, the number of graduate students pursuing higher degrees in physics ballooned like never before. More than two million veterans cashed in on the G. I. Bill after the war, helping to drive a massive expansion at all levels of American higher education, across nearly all fields. Yet physicists encountered the vast demographic shift first and most acutely—they experienced the extremes of what would quickly become the norm. The growth rate of graduate-level enrollments in American physics departments outstripped those of every other field after the war: during 1945-51, for example, the rate at which U.S. institutions granted physics doctorates rose nearly twice as quickly as all other fields combined. By the outbreak of fighting in the Korean War, American physics departments were producing three times as many new Ph.D.s in a given year as the prewar highs—a number that would only climb higher, by another factor of three, following the surprise launch of Sputnik in 1957. Only in the Soviet Union did physicist "manpower" rise at a comparable rate.[2]

Just as quickly, enrollments across all disciplines in the United States fell sharply during the early 1970s, a product of economic recession, détente, and massive cuts in education and defense spending. Again the physicists served as a bellwether, their enrollments plummeting faster and deeper than any other field: down fully one-third from their peak in just four years, falling to just one-half by decade's end. The bubble in physics enrollments rose and fell much the way financial bubbles have always done, from the seventeenth-century tulip craze to the tech stocks debacle of 2001: fed by honest and earnest decisions based on imperfect information, while also driven by hope and hype with little discernible grounding in fact.[3] By the early 1970s, the "Cold War bubble" had burst, and physicists in the United States faced the worst crisis their discipline had ever seen.[4] (See fig. 1.)

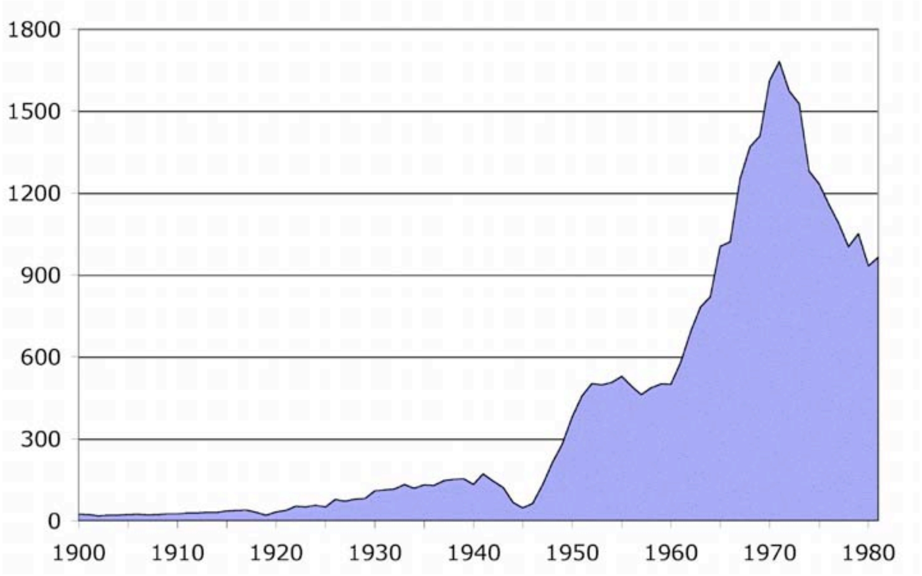

Figure 1. Number of physics Ph.D.s granted by U.S. institutions per year, 1900-1980.

American physics during the Cold War thus provides an especially useful window onto broader shifts in American higher education during the past half-century: rising fastest and dropping hardest, physics set the trend both in good times and bad for larger transitions in American intellectual life. Physicists across the country spent more time worrying about pedagogical issues than almost anything else. Struggling to find ways to manage the unprecedented numbers of students—or their equally unprecedented disappearance—came to define everyday life for the physics community. What effects did these pedagogical pressures have on physics itself—on what counted as "appropriate" subjects to study, with what means, and toward what ends? More generally, what can the physicists' example teach us about how creative, cutting-edge science becomes mundane, the stuff of stolid textbooks and routine assignments? What roles do institutions and infrastructure play in that transition?

The transitions and trade-offs are thrown into starkest relief by tracking how American physicists chose to teach quantum mechanics, physicists' description of matter and forces on the atomic scale. More than any other subject after the war, formal coursework in quantum mechanics became the *sine qua non* for earning a degree in physics. Everyone from undergraduates at tiny liberal-arts colleges to graduate students at the bustling research universities passed through a series of courses on the subject. By the 1950s, knowledge of quantum mechanics had become a litmus test: only those who had studied it could claim to be a physicist.

But how to teach it? Writing in 1953, J. Robert Oppenheimer captured some of the recent changes. Nowadays, Oppenheimer explained, quantum theory "is 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."[5]

As anyone who has tried to teach children to spell and add knows, the approach one takes (and the success to be had) depends strongly on the conditions of instruction. Some approaches work well with small groups; other means are required for large classrooms. This obvious point, quantified and refined by decades of research by education specialists, shaped the way American physicists grappled with quantum mechanics during the decades after World War II.[6] In the face of run-away enrollments, they re-crafted the subject, accentuating those elements that could lend themselves to high-throughput pedagogy. The subject became virtually synonymous with a cache of exemplars that could be routinized into problem sets and examination questions—those elements deemed most readily teachable to overstuffed classrooms. The last vestiges of qualitative, interpretive issues—the "what does it all mean?" musings that had so exercised the subject's founders—were quietly dropped. A kind of curricular arteriosclerosis set in, winnowing the range of what would count as "quantum mechanics" in the classroom. The goal became to train quantum *mechanics*: students would be more like engineers or mechanics of the atomic domain, rather than otherworldly philosophers.[7]

We may pinpoint the role of enrollments in helping to drive the transition in several ways: by comparing quantum mechanics courses taught at a variety of institutions throughout the United States at around the same time; by studying the long-term evolution of textbooks on the topic written by American authors; and by comparing these textbook trends with those written by physicists working outside the United States. In each case, physicists who faced smaller average enrollments tended to approach the teaching of quantum mechanics rather differently than their peers in large-enrollment settings.

Quantum Mechanics in the Classroom

Even more than relativity—with its talk of shrinking meter sticks, slowing clocks, and twins who age at different rates—quantum mechanics is a science of the bizarre. Particles tunnel through walls. Cats become trapped, half-dead and half-alive. Objects separated lightyears apart retain telepathic links with one another. The seeming solidity of the world evaporates into an evanescent play of likelihoods.

Quantum mechanics emerged over the first quarter of the twentieth century, honed primarily by Europeans working in the leading centers of theoretical physics: Göttingen, Munich, Copenhagen, Cambridge. Most of its creators—figures like Werner Heisenberg, Erwin Schrödinger, Niels Bohr, Max Born, Wolfgang Pauli, Pascual Jordan—famously argued that quantum mechanics was first and foremost a new way of thinking. Ideas that had led scientists for centuries were to be cast aside; Bohr constantly spoke of the "general epistemological lesson" of the new quantum era. The fall of determinism, Heisenberg's uncertainty principle, wave-particle duality—all seemed best understood, to this interwar generation, alongside neo-Kantian philosophy, Eastern mysticism, even Jungian psychoanalysis as newfound guides to deeper meaning. The subject's leading detractors, such as Albert Einstein, likewise agreed that quantum mechanics had to meet stringent philosophical tests. Mathematical self-consistency or agreement with experiments were important, but hardly sufficient.[8]

The subject's Central European architects foregrounded these deep philosophical issues in their classrooms and textbooks. No clean line separated calculation from interpretation. Some authors, like Vienna-based Arthur Haas, included entire chapters on "Quantum mechanics and philosophy." Werner Heisenberg wrapped up a long section on the uncertainty principle in his 1930 textbook by arguing that scientists were now "compelled" to treat "seriously" the "philosophy of past centuries"; only then could they make sense of their equations and the world. Hermann Weyl pronounced the death of the Kantian *Ding an sich*; Max Born emphasized the need to revise the "boundaries" of various concepts in the light of recent developments. Even Arnold Sommerfeld, so much more interested in practical calculation than philosophical rectitude, paused within his massive addendum to *Atombau und Spektrallinien* on wave mechanics to discuss the

Machian underpinnings of Heisenberg's uncertainty principle, and its attendant challenge to objectivity.[9] Weighty matters, indeed.

Even when breaking with the particular philosophical approaches of their Continental colleagues, several American physicists—including some of the most influential quantum theorists of their generation—agreed that the new material required sustained attention to interpretation, indeed to philosophy. During the late 1920s and 1930s, young American physicists such as Edwin Kemble, Edward Condon, Philip Morse, Arthur Ruark, Henry Margenau, Wendell Furry and others paraded their philosophical convictions in the *Physical Review* and in their textbooks.[10] Reviewers of these books agreed that an overtly philosophical register was appropriate. They often disagreed with specific points of interpretation in the books under review, but not with the notion that such interpretive issues belonged in the textbooks in the first place. Indeed, it became common for reviewers to compare and contrast the latest American quantum mechanics textbooks based on their philosophical approaches.[11]

Many of the interwar generation's most influential teachers likewise focused on philosophical material in their classrooms. Emblematic was Robert Oppenheimer's popular course at Berkeley. Graduate students routinely sat through Oppie's quantum mechanics course more than once; one desperate student even went on a hunger strike until Oppenheimer relented and let her attend the class for a fourth time. As late as 1939—the year that Oppenheimer's student, Bernard Peters, transcribed the lectures and made hectographed copies, which quickly went into wide circulation—Oppenheimer still introduced quantum mechanics as a "radical solution" to pressing physico-philosophical issues. Page after page, Oppenheimer focused not only on the new formalism, centered around Schrödinger's wavefunction, ψ, but also on its physical interpretation, lavishing attention on the origins and meaning of probabilistic interpretations. He even indulged in Einstein-styled attempts to circumvent the uncertainty principle (each, Oppenheimer showed in detail, destined to fail), long before walking through the first practical calculation with the new formalism. These discussions continued well beyond the classroom, as Oppenheimer and his tight circle of students talked into the night at their favorite San Francisco restaurants, or at parties at his Eagle Hill house in Berkeley. On his porch overlooking the bay, catalyzed no doubt by Oppenheimer's famously strong martinis, he and his students plunged into further discussions about the grand mysteries of quantum mechanics.[12]

Oppenheimer was hardly alone. Stanford's Felix Bloch taught his graduate-level course on quantum mechanics in a remarkably similar way. Throughout the 1930s, meanwhile, Caltech students faced tough questions about the interpretation of quantum mechanics on their qualifying oral examinations. In 1936, for example, one of Sherwood Haynes's examiners "wanted to know all about [the] ψ function, physical meaning, etc." Other students faced similar questioning. "When do we use quantum mech. rather than classical mechanics?," Martin Summerfield was asked during his 1939 exam. "What is [the] interpretation of $\psi(x)$? Does the Schrödinger equation describe the rate of change for *all* time?"—a subtle question about how a wave-packet gets reduced from a superposition of possibilities to the single measured result. Then the follow-up: "Discuss the nature of observation in quantum mechanics and in classical mechanics." And so on.[13]

Of course, not all American physicists paid so much attention to these interpretive and philosophical issues. Vladimir Rojansky, a professor at Union College in Schenectady, New York, wrote a well-known textbook on quantum mechanics in 1938 that hewed close to the mathematics. He encouraged his readers to do the same, sprinkling more than five hundred exercises and problems throughout his lengthy tome. Almost all were of the form, "Show that…" or "Verify that…"—that is, students were to practice manipulating equations until both sides matched Rojansky's own calculations. No hang-ups about determinism, probabilities, or wave-particle duality here.[14]

Yet for each pedagogical presentation like Rojansky's before the war, there was at least one like Condon and Morse's, Kemble's, or Oppenheimer's. Quantum mechanics meant many things to this interwar generation of American physicists. Some eagerly took up questions of philosophical interpretation—and encouraged their students to do so as well—while others remained happy enough to drill their students in how to handle the new equations, content to leave issues of ultimate interpretation for another day.

By the early 1950s, this spectrum of approaches had all but collapsed to a single pole. Where the earlier generation had turned explicitly to interpretive matters, most American physicists after the war denied there were any problems that needed interpreting. Indeed, they denied that "interpretation" (much less "philosophy") was a fit exercise for physicists or their students. Few lingered over how best to interpret the uncertainty principle or the place of probabilities. Not a single postwar textbook author paused, as Condon and Morse had done in their 1929 textbook, to assess the philosophical standing of various hydrogenic wavefunctions.[15]

The change came on quickly. Some Caltech students, having assiduously studied reports of earlier oral exams, were caught by surprise. Michael Cohen 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" on specific types of quantum-mechanical calculations. That same year, Frederick Zachariasen was asked to perform a calculation using the WKB method (a standard approximation scheme named for its originators, Gregor Wentzel, Hendrik Kramers, and Léon Brillouin), to which he "gave [the] standard response." Next came a question about the effect of an external electric field on the first excited state of hydrogen; Zachariasen "gave usual spiel again." A few years later, Kenneth Kellerman was put through the paces of hydrogen's energy spectrum "in all its gory detail": fine structure, hyperfine structure, effects of external electric and magnetic fields. At no point in this litany was he asked to give an interpretation of ψ or to describe the nature of observation in quantum mechanics.

Indeed, Kellerman's parting wisdom to future examinees was to "memorize" and "rehearse" answers to what had emerged as the stock problems, such as the hydrogenic spectrum. The same pattern is clear in the written comprehensive and qualifying exams at Stanford, the University of Pennsylvania, the University of Chicago, Columbia, Berkeley, and MIT: expansive essay-type questions about matters of interpretation through the late 1940s, replaced by a coterie of standard calculations by the mid-1950s.[16]

Several explanations for this shift might be offered. Perhaps the field had simply matured, and the conundra that had stumped the interwar generation—both in Europe and the United States—had been solved satisfactorily with the passage of time. Not so: most of the interpretive puzzles that had emerged in the 1920s remained puzzling after the war. Physicists around the world continued to work squarely on them during the 1950s and 1960s—in such places as Copenhagen, Paris, Pavia, Milan, Trieste, Frankfurt, São Paulo, and Mexico City, but only rarely in the United States. Years later, a frustrated American physicist accused her colleagues of "cognitive repression," as they blithely ignored the major philosophical questions at the core of quantum mechanics.[17]

Or perhaps American physicists' crash course in "gadgetry" during the war turned all practitioners into pragmatists.[18] Evidence for this explanation is easier to come by. Consider the case of "Los Alamos University." Finding themselves with unexpected time on their hands after Japanese surrender, senior physicists at the Los Alamos laboratory offered a series of lectures during the autumn of 1945 for the many lab hands who had seen their formal schooling interrupted by the war. Among the offerings was Edward Teller's course on quantum mechanics. Referring to Rojansky's book on occasion, Teller showed little interest in high-brow epistemology. "Many phenomena can be looked at equally well," he lectured his charges, "by considering them as wave or particle phenomena." Full stop: no hand-wringing over the ultimate meaning of wave-particle duality. The uncertainty relations likewise warranted only a single, casual paragraph. Even more striking is the way Teller introduced the probabilistic interpretation of Schrödinger's wavefunction. Already by the 1930s, physicists had often introduced the topic with the example of a double-slit: a wall with two tiny slits through which elementary particles could pass. Upon being registered on a screen behind the wall, the particles would display a characteristic interference pattern that had no classical analog for ordinary matter. (See fig. 2.) Teller, in his haste, devoted only a single paragraph to the topic. More amazing still, he replaced the fabled double-slit with a *single* slit on the blackboard, from which the crucial interference pattern would never arise! Clearly his interests lay elsewhere.[19]

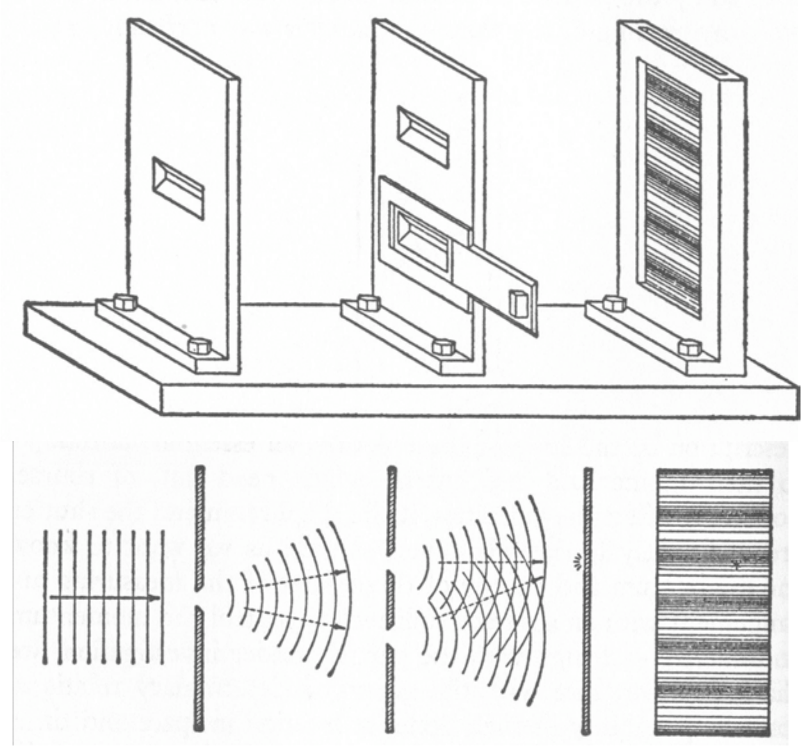

Figure 2. Double-slit and quantum interference. Quanta approach the apparatus from the left, passing through the first (single) slit, before impinging on the wall with two slits. A screen behind the slits registers where the quanta eventually arrive. If both slits on the second wall are open, then a characteristic interference pattern will develop on the back screen. Rather than only showing bright spots (where many quanta arrived) clustered behind each of the two slits—as one would expect if shooting ordinary objects, like bullets, toward two holes in a wall—bright and dark spots alternate, with many bright spots located far from the projected positions of the slits. The bottom panel displays a schematic interpretation of the phenomenon in terms of interfering waves. Such interference patterns are familiar from macrosopic waves, such as ocean waves passing through gaps in a corral reef. Yet the wave-like interference pattern persists even if only one quantum particle is sent toward the slits at a time—as if each quantum traversed both slits and interfered with itself on the other side. As physicists quickly realized, using Heisenberg's uncertainty principle, any attempt to determine through which slit the lone quantum traveled would destroy the

interference pattern. (Source: Niels Bohr, "Discussion with Einstein on epistemological problems in atomic physics," in *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp [Evanston, IL: Library of Living Philosophers, 1949], 201-241, on 216, 219.)

Though his course included twenty-five lectures and the handwritten notes fill 148 pages—certainly comparable in length and detail to an ordinary one-semester course on the subject—Teller's approach atop the mesa, just weeks after the full-tilt effort to build nuclear weapons, couldn't have been more different from that taken years earlier by Kemble or Oppenheimer. Here, it seems, is the quintessential example of war-forged pragmatism. Yet several other physicists emerged from their war experiences (as we will see) with different attitudes than Teller's toward interpretive issues. No, the war alone is not sufficient to explain the dramatic changes in Americans' classrooms during the 1950s and 1960s.

More than individual style or wartime experiences, the single best predictor of how American physicists taught quantum mechanics after the war was class size. Enrollment pressures—or their occasional absence—shaped the kind and degree of attention devoted to interpretive or philosophical issues. Consider first the evidence from across the nation's classrooms. Unfortunately, the ephemera of daily classroom life rarely find their way into university archives or personal collections; it remains far easier to track down failed grant proposals from fifty years ago than successful lecture notes from the same period. I have been able to locate nine sets of lecture notes from courses aimed at the same type of student (first-year graduate students). Some notes were taken by students in the classes; others were prepared by the professors. Several derive from courses given by some of the discipline's most celebrated teachers: Enrico Fermi, Hans Bethe, Richard Feynman. On the face of it, these notes should be remarkably similar to each other: each course was aimed at the same level of difficulty; each purported to cover the same basic material; and several relied on the same textbooks. Yet reading through the notes, one is struck by their remarkable variation—not in equations derived or applications analyzed, but in the amount of attention devoted to interpretive or philosophical issues. As table 1 indicates, those classes that had small enrollments included relatively large proportions of discussion dedicated to interpretive issues, while those classes that had large enrollments featured strikingly less discussion.[20]

Table 1. Graduate courses on quantum mechanics, 1950-1961

Instructor	Institution	Term	Estimated Enrollment	Length of Notes	Interpretive material
Lothar Nordheim	Duke University	Spring 1950	12	112 pp.	12.5%
Freeman Dyson	Cornell University	Fall 1952	31	230	2.2
Enrico Fermi	University of Chicago	Winter 1954	21	171	1.2
Richard Feynman	Caltech	Fall 1955	30	215	1.9
Hans Bethe	Cornell University	Fall 1956	42	72	5.6
Wendell Furry	Harvard University	Fall 1957	59	210	3.3
Saul Epstein	University of Nebraska	Fall 1958	6	357	14.6
Edward Hill	University of Minnesota	Fall 1958	20	257	11.3
Freeman Dyson	Princeton University	Fall 1961	53	128	1.6

The three courses with smallest enrollments (Nordheim's, Epstein's, and Hill's) averaged *five times* more attention to the puzzles and paradoxes of quantum mechanics than the six courses with larger enrollments (12.8 ± 1.4% versus 2.6 ± 1.5%). The "philosophical" classrooms averaged just under thirteen students per class (12.7 ± 5.7 students); the "pragmatic" classrooms averaged nearly forty students per class (39.3 ± 13.4).

The numbers alone fail to give a flavor for how different the course discussions really were. Consider Lothar Nordheim's course. Like Teller and so many of his colleagues, Nordheim spent the war working on the Manhattan Project. He served as section chief at the Oak Ridge laboratory in Tennessee beginning in 1943 (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 to teach at Duke University in 1947, but did not stay long: he was among the earliest recruits to heed Teller's call to begin working full-time on the top-secret hydrogen-bomb project. In fact, Nordheim left Duke for H-bomb work at Los Alamos immediately after his spring 1950 course on quantum mechanics. He later left academia altogether to chair the theoretical physics department at the major nuclear-related defense-contractor, General Atomics. It would be difficult to find a theorist more deeply ensconced in the very "gadgetry" that so many have claimed turned American physicists into narrow-minded pragmatists.[21]

Yet unlike Teller, Nordheim insisted (during his brief hiatus at Duke) that his students focus intensely on the bizarre, surprising, qualitative aspects of quantum mechanics. Already in his first lecture, Nordheim launched into the probabilistic interpretation and its strange features. The passion with which Nordheim broached the topic left its mark on his student's notes. "What does this do to causality?," Nordheim asked. "Ans. It Fucks it!" To drive the point home, Nordheim devoted portions of two lectures in a row to analyzing the double-slit phenomenon—the selfsame example that Teller had dispatched in a single, sloppy paragraph—to introduce his students to wave-particle duality, superposition, probabilities, and the uncertainty principle.[22]

Nordheim showed similar care several weeks later when returning to the uncertainty principle. This time he emphasized the special role of measurements in quantum theory, and the conceptual break that had to be made from classical assumptions. After going through many of the same thought experiments that Oppenheimer had displayed for his own students a decade earlier, Nordheim concluded that "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." Surpassing even Oppenheimer's treatment, Nordheim then parroted Bohr's famous analogy to the situations in psychology and biology (at least as Bohr imagined them). One could analyze, for example, a person's specific thoughts on a given topic, or the process of thinking in general, but not at the same time. The activities were just as complementary—so Bohr and Nordheim emphasized—as trying to measure an object's position and simultaneous momentum. We must learn to live with the trade-offs, lectured Nordheim; older concepts simply won't do. Later Nordheim demonstrated that the uncertainty principle destroyed the possibility of observing a particle as it tunnelled through a barrier: yet another example that cut to the core of how quantum theory differed from classical physics. The example did little to help students complete specific calculations; it was aimed, instead, to adjust their patterns of thought.[23]

Saul Epstein's and Edward Hill's classes showed similar attention to these interpretive details. Epstein—among the youngest professors in the sample, having earned his Ph.D. at MIT in 1947—assigned readings from Bohr's and Heisenberg's philosophical writings, in addition to some of the standard textbooks. He lectured repeatedly on what he called "the 'reality problem'"—namely, how the quantum-mechanical formalism might or might not reflect reality, over and above its internal self-consistency. He also dissected the concept of "measurement": one must distinguish, he lectured his students, between preparing a system, measuring an attribute, and conducting an experiment. Such activities were not the same; each required separate consideration. Next he broached the Einstein-Bohr debate, evaluating both sides by invoking Harvard physicist Percy Bridgman's philosophy of "operationalism." (Like Nordheim, he also described Bohr's analogies to psychology and biology.) He critiqued the Einstein-Podolsky-Rosen argument at length; introduced Schrödinger's paradox about the half-dead cat; debated with his students whether or not the probabilistic interpretation of the wavefunction was inherently subjective; and presented two tightly-packed pages on "Rumblings of discontent," in which he discussed recent work on rival interpretations of quantum mechanics (such as David Bohm's "hidden variables").[24]

Hill's notes followed a remarkably similar route. Hill was by this time a long-time veteran of teaching quantum mechanics; he had co-authored the second half of Kemble's early review article as a Harvard postdoc in 1929. He returned to teaching the subject after serving as a research physicist and consultant for the U.S. Navy during World War II. In his 1958 lecture notes, he acknowledged the "influence of classroom discussion" in shaping what topics to cover, and in what manner. The intimate setting encouraged him to try "clarifying some of the dark corners of the theory, even if no major mysteries are solved." Like Epstein, Hill dwelled at length on the double-slit example, devoting seventeen pages to wave-particle duality alone. He lingered over the notion of superposition of matter waves, declaring it to be the greatest conceptual break from classical physics: "The importance of this step in the future development of the theory cannot be overestimated." He also briefly introduced Bohm's rival interpretation. Only with sufficient practice thinking through these ornery, interpretive issues, Hill declared, could "the methods of quantum mechanical theory be appreciated with anything approaching intuitive pleasure."[25]

The pedagogical approach found in these smaller classes, with its heavy emphasis upon interpretive matters, simply did not scale to larger groups. Back-and-forth discussion (of the kind Hill had noted) was difficult to sustain in classes of thirty students, let alone fifty or sixty as became increasingly common. (Exactly for this reason, Stanford's physics department chair tried in 1960 to limit his department's graduate courses to twenty-five students; yet by 1962, Stanford's first-year quantum mechanics course had swelled to forty-two students. Comparable courses in several other departments contained fifty or more students by this time; MIT's had ninety-two.[26]) With such large groups, lecturing fared much better with other types of material, such as how to perform various unitary transformations, diagonalize matrices, or execute Taylor-series approximations. The properties of the Hermite polynomials—standard mathematical fare that arose when treating one of the stock examples, the one-dimensional harmonic oscillator—might be tedious, but they supplied ample fodder for student exercises. If the Schrödinger equation with a particular potential could be transformed into the Bessel equation, the outcome could be useful but rarely *puzzling*. Teaching assistants and graders could coach younger students through problem sets, but were ill-equipped to delve into philosophical thickets. Some older physicists, such as George Uhlenbeck—European émigré and early architect of quantum theory—might grumble that his colleagues had confused what was "easy to teach"—that is, the "technical mathematical aspects of the theory"—with the conceptual understanding students needed most.[27] But faced with skyrocketing enrollments, most physicists chose to drop the qualitative discussions in favor of more teachable, quantitative material.

Freeman Dyson, for example, strode into his lecture hall at Cornell in September 1952 and announced that he would chart a different course than the one used in the students' textbook. He had assigned the brand-new book by David Bohm, *Quantum Theory* (1951), whose approach we will examine below. "I will not follow it closely," Dyson declared. "Too much philosophy." "Quantum theory grew gradually from a state of greater confusion to one of less," he continued. "Bohm follows this in detail. I don't." And he was off: a few minutes on two important experiments (the photoelectric effect and Davisson-Germer electron-interference measurements), and then straight to the formalism. By the eighth page he had produced the Schrödinger equation; three pages later he used it to analyze the well-worn example of a square-well potential and barrier penetration. Where Nordheim had paused to note the strange metaphysics of a particle during its transit through a potential barrier, Dyson plowed on, adapting his first calculation to treat various states of the deuteron. In a similar way, Dyson derived the uncertainty principle formally (using the Schwartz inequality), but barely lingered to discuss what it all meant.[28]

Fermi, Bethe, Feynman, and Furry proceeded quite similarly. Fermi devoted two pages to the uncertainty principle in his class notes—supplying a loose derivation based on finite wave packets, but no qualitative discussion—while spending twice as long on the niceties of the Laguerre polynomials (as part of his derivation of the hydrogen atom's wavefunctions). Bethe noted dismissively that trying to circumvent the usual interpretation of the uncertainty principle was as foolish, and as wasteful, as chasing perpetual motion machines. Feynman began his course on a wildly different track—introducing his idiosyncratic sum-over-histories approach, which he had developed in his dissertation a decade earlier—but abruptly reverted to the standard approach after his first few lectures and never wavered from the familiar trail. Ten years later, when he finally published his lecture notes in textbook form, he closed his introductory chapter by admonishing students that interpretive issues were all "in the nature of philosophical questions. They are not necessary for the further development of physics." After a few cursory remarks about complementarity, meanwhile, Furry, too, delved into the details of eigenvalues, eigenvectors, and expectation values, marching through the same series of examples—particles in a square well, one-dimensional harmonic oscillators, and the hydrogen atom—that had occupied so much of his colleagues' class time. A decade after teaching his first course on quantum mechanics—and now facing a substantially larger class at Princeton—Dyson devoted even less time to problems of interpretation.[29]

These large-enrollment classes were by no means poorly executed. On the contrary, many of these lecture notes reveal the clarity and attention to detail that earned people like Fermi, Bethe, and the others their well-deserved reputations. The classes were obviously carefully thought through: each example built on the last; exercises emphasized techniques that students would capitalize upon in later work. Yet for every additional example that Dyson or Feynman or Furry marched through at the blackboard—how to approximate the effects of an electric field on the hydrogen atom's spectrum, say, or how to calculate the scattering cross-section for a given potential—these professors spent correspondingly less time encouraging their students to think hard about what all those fancy equations really said about the world. Perhaps without realizing it, they were teaching a particular epistemology in those bloated classrooms. Day in and day out, they inculcated an instrumentalism quite different from the approach fostered in many American classrooms before the war, and in a handful of small classrooms after it.

Textbook Trends

As the enrollment boom hardened into a daily fact of life for American physicists, the textbooks they wrote on quantum mechanics likewise began to respond in characteristic ways. The transition is illuminated best by following the fates of two well-known books published a few years after the war: Leonard Schiff's *Quantum Mechanics* (1949) and David Bohm's *Quantum Theory* (1951). Both were written by veterans of Oppenheimer's intense interwar group at Berkeley; both acknowledged how influential his Berkeley course had been in charting their own pedagogical paths.[30] Yet what seemed like two 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. The differing treatments meted out to Schiff's and Bohm's textbooks became metonymic for changes throughout the profession at large.

Leonard Schiff had been a postdoc with Oppenheimer between 1937 and 1940; he later began teaching at Stanford, and his *Quantum Mechanics* first appeared in 1949 to rave reviews. Schiff's book exemplified the straight-and-narrow pragmatic approach to quantum mechanics. Whereas Oppenheimer had built his way slowly to the details of the Schrödinger equation and a plethora of examples that could be treated with it—pausing at length to entertain many of the philosophical quandaries that arose along the way—Schiff largely dispensed with these conceptual niceties. What had occupied one-sixth of

Oppenheimer's original lecture notes filled but the opening sixteen pages of Schiff's textbook—a mere four percent of the whole. That proportion continued to fall each time Schiff undertook revisions to his book, as more and more techniques for making new types of calculations crowded out what had already been paltry attention to interpretive issues. In the meantime, Schiff included what was widely hailed as the best collection of numerous, well-chosen problems, of just the right level of difficulty for his target readers.[31]

David Bohm completed his Ph.D. under Oppenheimer's guidance in 1942, and published his *Quantum Theory* in 1951 after teaching for several years at Princeton. He had tested out the material in classes during 1947 and 1948, before Princeton's department had swelled too large. (The enrollments both years were likely twenty students or less, gauging from later degree conferrals.) Bohm's book, too, received glowing reviews at first—"a rare example of expressive, clear scientific writing," proclaimed one satisfied reviewer. In contrast to Schiff's treatment, Bohm's book lovingly lingered over each philosophical twist and turn, expounding at greater length than even his mentor had done on the sweeping epistemological challenges offered up by quantum mechanics. What Schiff dispatched in his opening sixteen pages, Bohm meticulously analyzed over his first two hundred: the Schrödinger equation didn't even appear until page 191, and the first standard example (the one-dimensional square well) wasn't broached until page 229 (compared to pages 21 and 34, respectively, in Schiff). The conceptual care Bohm had taken 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 in much greater detail. Yet for those applications 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."[32]

Despite their equally promising starts, the two books, like their authors, suffered quite different fates. Schiff became department chair at Stanford and soon editor of the influential textbook series published by McGraw-Hill: the "International Series in Pure and Applied Physics," in which his own book had appeared. Bohm, meanwhile, was forced out of his job at Princeton—and soon forced out of the country—just months after his book had been published. He had refused to name names when subpoened to testify before the House Un-American Activities Committee, during its headline-grabbing investigation into alleged "Communist infiltration" of the wartime Radiation Laboratory at Berkeley (a major contracting site for the Manhattan Project). Summarily dismissed from Princeton for having had the temerity to plead the Fifth, he found no other doors open to him in the United States, so he fled to Brazil—where, in between bouts of crippling nausea, he was compelled to forfeit his U.S. passport—before moving a few years later to Israel and eventually the United Kingdom.[33] Meanwhile, Schiff's book went through two more widely heralded editions (in 1955 and 1968), each reprinting of which sold handily. Bohm's book was never reissued, and his further efforts to write a new quantum mechanics textbook were rebuffed.[34]

It fell to a third member of Oppenheimer's interwar Berkeley crew, Edward Gerjuoy, to make sense of these diverging paths. The occasion for his reflections was a review of the second edition of Schiff's textbook. Gerjuoy focused directly on the new pedagogical landscape. Schiff had done nothing to bolster the anemic interpretive discussions in the first edition of his book. To Gerjuoy's taste, Schiff's book still devoted too little attention to "such questions as correspondence, uncertainty, complementarity, and causality"—precisely the topics that had filled so much of Bohm's bloated book (not to mention Nordheim's, Epstein's, and Hill's lecture notes). Indeed, Gerjuoy noted, "the contrast with Bohm's *Quantum Theory* is interesting, even amusing." But Gerjuoy could understand Schiff's decision not to amplify these topics in the 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 leapt to the technical apparatus; too soon, the student "is well into detailed algebraic complexities verifying which, he readily persuades himself to believe, means he is learning quantum mechanics." While Gerjuoy certainly thought that too much philosophical exegesis could "utterly surround and befog the student," he was left wondering in the end, "is it 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?"[35] As the boom in enrollments continued, Gerjuoy's colleagues answered again and again in the affirmative.

Very quickly Schiff's textbook became the standard-bearer for new books aimed at advanced undergraduates and first-year graduate students. While some physicists continued to snipe that the book did not "contain much beauty, charm, or elegance," others held it up as a model especially well-tuned to the new pedagogical realities.[36] When asked whether a third edition of the book would be warranted, Berkeley's Eyvind Wichmann replied to the editors at McGraw-Hill with a sixteen-page memorandum on the previous two editions, and why they had been so successful. "I believe that the explanation is that Schiff is a very *practical* book," Wichmann began, reflecting on his own experiences with the first edition as a student. "The reader who goes through the book really obtains a working knowledge of quantum mechanics."

> He is taken through a number of well chosen applications, and he is shown, through these examples how it all works out; how one can actually compute things with the theory. One accordingly builds up a reserve of problems successfully attacked, and this knowledge can then somehow be used in attacking new problems. […] The discussion in Schiff is never overloaded with formalism for which there is no immediate need: The discussion stays close to physics (i.e., to the physics discussed in the 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.[37]

What Gerjuoy had seen as a limitation, Wichmann clearly believed should be emulated widely: problem-solving should replace "pseudo-philosophical speculations" altogether.

The year after Schiff's second edition appeared, Bohm proposed to write a new textbook, again aimed (as his and Schiff's had been) at advanced undergraduates and first-year graduate students. He sent the proposal to McGraw-Hill, for consideration in the very textbook series that Schiff now oversaw. Rather, Bohm's long-time friend and fellow Oppenheimer student, Melba Phillips, sent in the materials, since by this time Bohm had left Brazil and was getting settled at the Technion in Haifa.

The reviews on Bohm's outline and sample chapters were mixed. What had seemed a few years earlier (at least to some reviewers) to be Bohm's special talents in writing textbooks—fastidious attention to conceptual issues—now struck reviewers as inappropriate for a textbook aimed at an American audience. Schiff wrote to his associate at the press that Bohm needed to "keep off his own special hobby-horse with respect to quantum mechanics"—an intriguing new interpretation Bohm had published just as he was fleeing the country four years earlier, in which "hidden variables" guided particles on their paths in a fully causal manner. Today Bohm's interpretation is recognized as one of the most significant developments in interpreting quantum mechanics during the twentieth century, repeatedly invoked alongside the interwar efforts of Bohr, Heisenberg, and the rest.[38] Yet at the time, most American physicists paid no heed to Bohm's new approach, drowning as they were in an overflow of students who needed to learn rudimentary means of calculating. (Of all the course notes I have found, only Epstein and Hill so much as acknowledged Bohm's interpretation in their lectures.) Hugh Wolfe, who had gamely leapt into the interpretive fray twenty years earlier with an early critique of the Einstein-Podolsky-Rosen paper, now saw no place for these kinds of considerations in a textbook. Reviewing Bohm's book proposal, he argued emphatically that neither Bohm's "pet idea" nor any other protracted discussion of interpretive issues belonged in the classroom. Bohm's textbook proposal went nowhere.[39]

As enrollments continued to climb, physicists repeatedly sided with the Schiff-Wichmann approach rather than the Bohm-Gerjuoy model. Where once reviewers had evaluated textbooks based (in part) on their specific philosophical stance, now reviewers routinely praised the newest 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," declared MIT's Herman Feshbach. In their place, American textbook authors added more material on several topics—scattering theory, formalism for handling angular momentum, and advanced approximation schemes—that were of most immediate relevance to nuclear and solid-state applications. Indeed, by 1960, the "usual topics" that reviewers expected textbooks to treat no longer included detailed exegesis on the uncertainty principle, probabilistic interpretation, superposition, complementarity, or the rest. Instead, the "usual topics" included a laundry list of calculating techniques: "angular momentum, the central force problem, perturbation theory both time independent and time dependent, the variation method, scattering, the WKB method, consequences of indistinguishability, and an introduction to field quantization."[40]

American physicists published thirty-three textbooks on quantum mechanics during the three decades after Schiff's first edition, all aimed at the same type of student (first-year graduate students). Twenty more books appeared for less advanced students. Authors and reviewers showed surprising passion when debating the best way to present the material. Some believed that students could make the quickest progress by focusing predominantly on Schrödinger's approach (since the mathematics of differential equations was most similar to techniques students had encountered in other coursework). Others mocked those books that "cling to Schrödinger's differential equation as to a lone friend in a strange land," when more powerful methods (Heisenberg's matrices, Dirac's bra-ket state vectors) could be had.[41] These books were anything but carbon copies of each other.

From the vantagepoint of managing large enrollments, however, the books did indeed start to look the same. By the mid-1950s, virtually every single book review in *Physics Today* and the *American Journal of Physics* assessed the latest quantum mechanics textbooks based on the number and appropriateness of their assignments. The embedded exercises and problems became the way to shine, rather than the skein of interpretation applied. Soon American physicists and their students had a sea of problems to sift through and practice solving: 17,404 problems in English-language books published between 1929 and 1980, to be exact. Some problems, stand-out favorites from the earliest textbooks—such as calculating the energy spectrum of a one-dimensional harmonic oscillator, or finding the tunneling rate for a particle in a box—enjoyed frequent repetition in new books for decades to come. Yet the types of problems hardly remained static during this long interval. Instead, the contours of this growing collection quickly came to reflect the postwar reality of overflowing enrollments.

Condon and Morse had included a sum total of sixteen problems in their 1929 textbook. Many asked students to perform straightforward manipulations of the equations from the text: make a change of variables in the Schrödinger equation, for instance, or evaluate a given integral. Yet others—fully one-quarter of them—called on students to go beyond the equations, to discuss their calculations in words. "Compute the charge and current-density expressions associated with the characteristic functions of Problem 2, and discuss their physical meaning," came one. Others called for purely textual responses: short essays rather than algebraic manipulations. Students were to explain why some quantum systems displayed qualitatively different behavior than others: "Why does the current vanish for the rectangular box, but not, necessarily, for the cylindrical and spherical enclosures?"[42] Much like the back-and-forth discussion reflected in Edward Hill's lecture notes, these types of interpretive problems—which, in general, required much more time to grade—could be assigned to small classes without becoming too large a burden on the instructors or their teaching assistants. They were a straightforward extension of the interpretive activities cultivated in classroom discussions.

Very quickly after the war, the number of problems included within American quantum mechanics textbooks leapt skyward. In place of Condon and Morse's sixteen problems—or the total lack of problems included in Ruark and Urey's 1930 textbook, or Alfred Landé's, Edwin Kemble's, and Saul Dushmann's textbooks from 1937 and 1938—the textbooks of the early 1950s averaged over one hundred problems each, climbing to nearly two hundred by the early 1960s.[43] As the total number of problems grew fast, the text-based, qualitative, short-answer-type problems began to disappear. Students were pushed through the paces of an impressive range of quantitative calculations—many of the "plug and chug" variety, to be sure (calling for students to substitute one algebraic expression into another), but many others requiring students to exercise far more thought and skill. The problems were by no means easy; but neither did they require students to step back from their algebra and describe (in words) what it all meant.

All told, these thirty-three textbooks contained a total of 6,261 problems (including, of course, many duplicate problems that appeared in several books). Just over one thousand of these were of the interpretive, short-answer variety—on the face of it, not a huge change from the one-in-four ratio featured in Condon and Morse's textbook. Yet the rhythm of these text-based problems was hardly uniform after the war. The proportion hovered around the ten-percent mark while American physics classrooms underwent their exponential bulge. Only after enrollments plummeted in the late 1960s did a new kind of textbook begin to appear, now featuring interpretive, text-based questions in nearly half of its problems. (See table 2.)

Table 2. Number and types of problems in first-year graduate textbooks on quantum mechanics by American authors, 1949-1978

Years	Number of books	Average number of problems per book	Average percentage short-answer problems per book
1949-53	5	135.0	8.7 ± 4.6%
1954-58	2	140.0	6.7 ± 5.7
1959-63	9	180.2	11.8 ± 9.7
1964-68	6	181.7	12.9 ± 5.7
1969-73	8	177.4	9.6 ± 6.8
1974-78	3	391.7	44.2 ± 18.5

In 1974, for example, Robert Eisberg (of the University of California, Santa Barbara) and Robert Resnick (at Rensselaer Polytechnic Institute) published their massive *Quantum Physics of Atoms, Molecules, Solids, Nuclei, and Particles*. Their authorship had been an arranged marriage, of sorts: their publisher, John Wiley & Sons, had suggested that they write the book together, based on their successful solo experiences writing textbooks in the past. As had long been common practice, they pulled together a rough draft and began "intensive classroom testing" of the draft at their own schools and in a few colleagues' departments—beginning, as it turned out, soon after the enrollment bubble had burst. By the time the book came out, total enrollments had fallen more than thirty percent from their late-1960s highs. The book reflected some of these changing classroom dynamics: in addition to hundreds of quantitative problems, akin to those that had long been a staple in American textbooks, Eisberg and Resnick also included long lists of "discussion questions" at the end of each chapter. Before the "ordinary" problems appeared, students first encountered twenty or so interpretive questions—each of which called for an explanatory (rather than algebraic) response. "Does a blackbody always appear black? Explain the term blackbody." "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.'" "Are there conceptual difficulties with the idea of a point electron?" And so on.[44]

Two years later, the enrollment curve having bottomed out, a similar book appeared by a trio of physicists at Rice University. Like Eisberg and Resnick's *Quantum Physics*, Michael Morrison, Thomas Estle, and Neal Lane's *Quantum States of Atoms, Molecules, and Solids* featured a huge proportion of essay-type questions (55.5%). As the authors explained, they had tested their materials in their Rice classrooms beginning in 1970. What worked best, they had decided—at least in the new classroom conditions—was a series of multi-part problems, each with a lengthy introduction to lay out the scope and motivation for the materials to come. Problem after problem pressed students to "discuss," "explain," or "justify your conclusions." Although the third book published during this period—Stephen Gasiorowicz's *Quantum Physics* (1974)—did not include nearly so many short-answer questions as the Eisberg-Resnick or Morrison-Estle-Lane books did, even the proportion in Gasiorowicz's book (18.1%) was nearly twice the average for those books written during the enrollment bubble.[45]

One might object that earlier textbook authors, writing during the 1950s and 1960s, had left out all those juicy, interpretive short-answer questions because they assumed students had already worked through such matters in earlier coursework. After all, by the mid-1950s students almost never got their first taste of quantum mechanics in graduate school. Looking at those textbooks written by American physicists aimed at beginner or intermediate undergraduate classes, however, reveals a similar pattern to that for the more advanced books: few short-answer questions during the boom (which crested a few years earlier than the boom in graduate enrollments), followed by rapid growth once average classroom numbers had fallen.[46] (See table 3.)

Table 3. Number and types of problems in beginner and intermediate undergraduate textbooks on quantum mechanics by American authors, 1949-1978

Years	Number of books	Average number of problems per book	Average percentage of short-answer problems per book
1949-53	1	0	N/A
1954-58	3	90.3	7.8 ± 3.1%
1959-63	5	187.6	12.3 ± 5.8
1964-68	4	176.0	12.3 ± 7.3
1969-73	5	192.2	24.9 ± 14.5
1974-78	2	369.0	26.0 ± 6.8

Books by Anthony French—MIT physicist and renowned textbook author—clearly illustrate the trend. French's earliest textbook, *Principles of Modern Physics*, which he published in 1958 while teaching at the University of South Carolina, included seventy-nine problems, only seven of which (8.9%) called on students to interpret the material in words. Twenty years later, French published *An Introduction to Quantum Physics* with Edwin F. Taylor. The new book bulged with problems: 244 altogether, fully eighty of which (32.8%) were interpretive, short-answer questions. French even highlighted several of these questions with special labels, as in problem 13 of chapter 2:

> […] (d) *Speculative question*: If the constants of nature were such that the de Broglie wavelength *was* of importance in decoding everyday experience, what forms would this experience take?
>
> (e) *Interpretive question*: Student A says that the wavelenghts calculated in this exercise are utterly meaningless, since they are incapable of being verified. Student B maintains that, although they are miniscule, these wavelengths have an indisputable meaning. What criteria would you use in judging these competing claims?[47]

The Cold War bubble having burst, faint echoes of philosophical engagement thus crept back into American physics classrooms.

International Textbooks

Physicists working in countries that faced little enrollment pressure after the war tended to write books that looked quite different from those of their American colleagues. For one thing, almost none of the postwar textbooks by authors in Germany, Austria, or Switzerland included any exercises or problems until well into the 1960s.[48] Moreover, postwar textbooks by German-speaking authors continued to emphasize the close ties to philosophy that the interwar German books had heralded. Twenty years after Heisenberg exhorted his readers to take philosophy seriously, for example, his long-time associate, Wolfgang Finkelnburg, did the same. In his *Einführung in die Atomphysik* (1948), Finkelnburg included a long section on "Achievements, limitations, and philosophical significance of quantum mechanics," culminating in his hope that the subject would shed new light "on Kant's 'a priori' concepts, still considered an essential prerequisite of any science."[49] A former assistant of Schrödinger's also wrote a new book on the subject soon after the war. Arthur March's *Quantum Mechanics of Particles and Wave Fields* originally appeared in English in 1951, even though March continued to teach in his native Innsbruck. Breaking with Continental tradition, March included fifty problems in his book—one of the first Central European authors to include any problems at all. Yet his book likewise stood out from its American counterparts: fully 28% of the problems were of the short-answer, interpretive type, more than four times the average proportion found in American books at that time. Zurich-based Walter Heitler, meanwhile, encouraged the "philosophically minded reader" of his *Elementary Wave Mechanics* (1945, revised edition 1956) to "decide for himself whether he would consider the wave field of an electron (ψ function) as part of an 'objective reality' or 'only' as a product of the human brain."[50]

Textbooks by French authors likewise foregrounded philosophy. The first volume of Albert Messiah's massive *Mécanique quantique* appeared in 1959, and in

English translation in 1961. Messiah's book out-Bohmed even David Bohm's textbook. It featured, for example, a fifty-page chapter on "Statistical interpretation of the wave-corpuscle duality and the uncertainty relations" that made prominent use of Bohr's notion of complementarity in its minute dissection of the concept of causality. (An American reviewer of Messiah's book singled out this long chapter—he called it "excessive" and "overdone"—as emblematic of his criticism of the book as a whole.) A few years later, a more sympathetic reviewer praised a different French textbook that betrayed similar features. In his book, the reviewer noted, Paris-based Olivier Costa de Beauregard discussed the "foundations and epistemological implications" of quantum theory with rare clarity, as "one who is thoroughly familiar with the conventional interpretations (often referred to as the Copenhagen school) as well as with the interpretations of Louis de Broglie, Albert Einstein, Erwin Schrödinger and Alfred Landé."[51]

Meanwhile, quantum mechanics textbooks in the Soviet Union—the one country that faced enrollment pressures most similar to those in the United States—showed greatest similarity with American textbooks. They, too, quickly began to feature large numbers of embedded exercises and problems; indeed, physicists the world over still celebrate the famous problems sprinkled throughout the "Course of Theoretical Physics" textbook series by Moscow theorists Lev Landau and Evgenii Lifshitz. Beginning in the early 1960s, a spate of translations of Soviet pedagogical materials began to hit American bookshelves. These books featured even more problems, on average, than their American counterparts did (about 200 each)—while including fewer interpretive, short-answer problems than even the American textbooks had done (4.0 ± 3.5%). The Soviets pushed the genre to the limit, publishing a series of "problem books" on quantum mechanics: collections of hundreds of problems (with detailed solutions in the back) with none of the usual textbook apparatus to accompany them. (These problem books included about the same small proportion of interpretive questions that the Soviet textbooks did.)[52]

Perhaps the Soviet physicists chose to downplay interpretation not (or not only) as a means of coping with large enrollments, but as a learned response after earlier, bitter struggles. During the 1920s and 1930s, after all, several Soviet quantum physicists had run afoul of the Party line when trying to interpret quantum theory. Similar confrontations decimated Soviet genetics research after the war, when apparatchiks installed Lysenkoism in its stead. But by the end of the 1940s, the state's interest in nuclear weapons far exceeded its concern over whether or not the academicians agreed that Schrödinger's wavefunction exemplified dialectical materialism. Indeed, several scholars have argued that Stalin's intense desire for nuclear weapons prevented him from meddling with physics the way he did with biology after the war.[53]

Moreover, at least some of the textbooks from the earlier era—when ideological tests were strongly in effect, but before enrollment pressures took off—placed heavy emphasis on philosophical interpretation. D. I. Blokhintsev's *Quantum Mechanics*, for example, first appeared in Russian in 1944, "during the days of Stalin," as one of its American reviewers later emphasized. By the time it was translated into English in 1964, Stalin had been dead for a decade; the book's Western reviewers found it "amusing to find Engels next to Einstein and Lenin between Landau and Lorentz" in its bibliography. Yet the book's consistent "philoso-physical attitude" earned it solid praise upon its translation. One American reviewer especially recommended the book because of its unusually clear discussion of "the physical meaning of some fundamental points such as the uncertainty relations (section 16), tunnel effect (section 97), and many body problems." The biggest drawback, in this reviewer's eyes, was that the book contained no problems.[54] Before the enrollment boom, at least some Soviet physicists found it pedagogically appropriate to emphasize difficult questions of interpretation.

Thus the pattern seems clear. Where enrollment pressures loomed largest, physicists on both sides of the Iron Curtain drilled their students to "turn the crank" and work through more and more quantitative problems, rather than spend their time philosophizing.

Conclusions

During the mid-1950s, a young Berkeley physicist learned the hard way how bloated class sizes could shape a specific approach to physics. Having been at Berkeley only a year and a half, Roland Good, Jr. was let go, not because he was unproductive in research or unconscientious in teaching—Berkeley's department chair, Raymond Birge, insisted that Good was more than adequate at both—but rather because his chosen research topic fit poorly with the new pedagogical realities. Good, a theorist, focused on rather abstruse points in quantum field theory; Birge likened it to Einstein's fruitless quest for a unified field theory. Though the research could well prove to be important (Birge thought it was still too early to say), it had failed a most important test. Junior faculty members, Birge explained, needed to select research topics for themselves that could provide appropriate spin-off problems 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 Good'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 couldn't afford such a luxury; they needed to find "someone who will be more useful to us." (Just a year and a half earlier, in fact, Birge had fast-tracked a promotion case for another young department member based largely on his ability to craft "do-able" problems for his many graduate students.) There was no room for theory for its own sake, for an abstract, speculative approach to fundamental physics—no room for it, that is, if it proved incapable of carrying dozens of dissertations on its coattails.[55]

Twenty years later, a radically different pedagogical imperative faced many American physicists. Entering graduate classes in Stanford's physics department had hovered around thirty students per year between 1959 and 1968, rising to a peak of thirty-seven students entering in 1969. (Some faculty members, eyeing these enrollment patterns, feared the department would become a mere "factory," producing standard-issue degree-holders.) Then the bottom fell out: only eighteen graduate students entered the department in 1970; only sixteen entered in 1972. Equally suddenly, the department undertook a sweeping revision of its comprehensive exam. Now the relevant departmental committee argued that the exam "should attempt to integrate and combine the various areas of physics, rather than test the ability to solve problems"—an immediate about-face from its own 1965 recommendations, which had called for renewed emphasis on problem-solving. "To achieve this aim," the committee continued, the exams should include "a significant fraction of essay and discussion questions." And so they did: on the revised comprehensive exam, administered during September 1972, more than forty percent of the problems were of short-answer or essay form—an increase of 150% over the previous decade's exams.[56]

Along with the new exams came other new features of department life, likewise made feasible by the sudden drop-off in enrollments. Walter Meyerhof, chair of the Stanford department's "Graduate Study Committee," alerted incoming students in September 1972 that the department would organize a new, informal seminar "in the general area of 'speculations in physics'"—just the sort of thing that had cost Good his Berkeley job twenty years earlier. Across the country, meanwhile, after its enrollments dropped by one-quarter in just four years, Harvard's department offered an entire graduate-level course on Bohm's version of quantum mechanics for the first time in 1975—heterodoxy having finally received an approving nod from the establishment.[57]

Physicists' choices of topics to discuss and problems to assign reflected deeper debates about the ideal type of physicist they sought to train. Should the new generation be philosophically engaged, concerned with minute details of conceptual interpretation? Or should they hone skills at calculating, pushing Heisenberg's and Schrödinger's equations into service of an ever more elaborate armamentarium of problems to solve and phenomena to analyze? Each ideal type relied on different types of skills—skills that did not inhere in students ahead of time, homunculus-style, but which needed to be primed, coached, worked on day in and day out, the way championship athletes rely on exercise drills.

Competing ideals flourished under different pedagogical conditions. As enrollments grew to unprecedented size, the skill sets that professors envisioned when looking out at all those faces in their crowded lecture halls shifted from earlier patterns. Some techniques were more amenable to "factory" training than others. With the right problem sets, aid from teaching assistants, and examples in lectures, large groups of students could learn to manipulate the Schrödinger equation for a variety of model situations. Much more difficult—in effect impossible, as far as most American physicists were concerned—was exercising the kind of interpretive work that had once occupied so much class discussion time. As one former physics graduate student later recalled, "I went to graduate school [at Harvard in the late 1950s] to learn about foundations. I was taught, instead, how to do physics. In place of wisdom, I was offered skills."[58]

If students emerged from these large classes with great facility in manipulating equations but limited interest in exploring those equations' deeper meanings, it was not because they had internalized the pragmatism of John Dewey or the operationalism of Percy Bridgman. Rather, the conditions of their instruction had favored certain skills over others. No one trains neo-Kantian philosophers by the auditorium-load. But with the careful selection of appropriate problems, American physicists found that they could train mechanics of the quantum domain.

Notes

The following abbreviations are used: *AJP*, *American Journal of Physics*; *BDP*, Department of Physics records, University of California at Berkeley (Bancroft Library); *FB*, Felix Bloch papers, Stanford University Archives; *HAB*, Hans A. Bethe papers, Cornell University Library; *HSPS*, *Historical Studies in the Physical and Biological Sciences*; *LIS*, Leonard I. Schiff papers, Stanford University Archives; *NBL*, Niels Bohr Library, American Institute of Physics, College Park, Maryland; *NYT*, *New York Times*; *PR*, *Physical Review*; *PT*, *Physics Today*; *RMP*, *Reviews of Modern Physics*; *RTB*, Raymond Thayer Birge papers, University of California at Berkeley (Bancroft Library).

[1] *Harper's* quoted in Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America*, 3rd ed. (Cambridge: Harvard University Press, 1995 [1978]), 375-76; on the 1947 Shelter Island conference, see Silvan S. Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton: Princeton University Press, 1994), chap. 3; on physicists' B-25 commutes, see Philip Morse, *In at the Beginnings: A Physicist's Life* (Cambridge: MIT Press, 1977), 247.

[2] David Kaiser, "Cold war requisitions, scientific manpower, and the production of American physicists after World War II," *HSPS* 33 (2002): 131-59; Alexander Brown and David Kaiser, "Political economy and the production of physicists: International comparisons," in preparation; and Catherine Ailes and Francis Rushing, *The Science Race: Training and Utilization of Scientists and Engineers, US and USSR* (New York: Crane Russak, 1982).

[3] Cf. Robert Schiller, *Irrational Exuberance*, 2nd ed. (Princeton: Princeton University Press, 2005 [2000]).

[4] Kaiser, "Cold war requisitions."

[5] J. R. Oppenheimer, *Science and Common Understanding* (New York: Simon and Schuster, 1953), 36-37.

[6] For recent studies of the effects of classroom size on style and efficacy of instruction, see the special issue on "Class size: Issues and new findings," *Educational Evaluation and Policy Analysis* 21 (Summer 1999): 93-248; Leonard Spring, Mary Elizabeth Stanne, and Samuel S. Donovan, "Effects of small-group learning on undergraduates in science, mathematics, engineering, and technology: A meta-analysis," *Review of Educational Research* 69 (1999): 21-51; and Ronald Ehrenberg, Dominic Brewer, Adam Gamoran, and J. Douglas Willms, "Class size and student achievement," *Psychological Science in the Public Interest* 2 (2001): 265-94.

[7] I borrow the metaphor of "arteriosclerosis" from Hugh Gusterson, "A pedagogy of diminishing returns: Scientific involution across three generations of nuclear weapons science," in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*, ed. David Kaiser (Cambridge: MIT Press, 2005), 75-107.

[8] The literature on the creation of quantum mechanics is vast. For an introduction, see esp. Max Jammer, *The Conceptual Development of Quantum Mechanics* (New York: McGraw-Hill, 1966); Olivier Darrigol, *From c-Numbers to q-Numbers: The Classical Analogy in the History of Quantum Theory* (Berkeley: University of California Press, 1992); Mara Beller, *Quantum Dialogue: The Making of a Revolution* (Chicago: University of Chicago Press, 1999); and Peter Galison, Michael Gordin, and David Kaiser, eds., *Quantum Histories* (New York: Routledge, 2001).

[9] Arthur Haas, *Wave Mechanics and the New Quantum Theory*, trans. L. W. Codd (London: Constable, 1928), chaps. 11, 16; Werner Heisenberg, *The Physical Principles of the Quantum Theory*, trans. Carl Eckart and F. C. Hoyt (Chicago: University of Chicago Press, 1930), 65; Hermann Weyl, *The Theory of Groups and Quantum Mechanics*, trans. H. P. Robertson (New York: Dutton, 1931), 76; Max Born, *Atomic Physics*, trans. John Dougall (New York: G. E. Stechert, 1936), 82-85; and Arnold Sommerfeld, *Wave Mechanics*, trans. H. L. Brose (London: Methuen, 1930), 37, 257. On Sommerfeld's style, see Suman Seth, *Principles and Problems: Constructions of Theoretical Physics in Germany, 1890-1918* (Ph.D. dissertation, Princeton University, 2003), esp. chaps. 4-5.

[10] See esp. Nancy Cartwright, "Philosophical problems of quantum theory: The response of American physicists," in *The Probabilistic Revolution*, ed. Lorenz Krüger, Gerd Gigerenzer, and Mary S.

Morgan (Cambridge: MIT Press, 1987), 2: 417-35; and David Kaiser, *American Physics and the Cold War Bubble* (Chicago: University of Chicago Press, in preparation), chap. 4. Many scholars have emphasized the Americans' pragmatic tendencies during this period: Silvan Schweber, "The empiricist temper regnant: Theoretical physics in the United States, 1920-1950," *HSPS* 17 (1986): 55-98; Gerald Holton, "On the hesitant rise of quantum physics research in the United States," in Holton, *Thematic Origins of Scientific Thought: Kepler to Einstein*, 2nd ed. (Cambridge: Harvard University Press, 1988 [1973]), 147-87; Katherine Sopka, *Quantum Physics in America: The Years through 1935* (New York: American Institute of Physics, 1988); Alexi Assmus, "The molecular tradition in early quantum theory," *HSPS* 22 (1992): 209-31; and Assmus, "The Americanization of molecular physics," *HSPS* 23 (1992): 1-34.

[11] See, *e.g.*, Edward Condon and Philip Morse, *Quantum Mechanics* (New York: McGraw-Hill, 1929); Edwin Kemble, "The general principles of quantum mechanics, Part I," *PR Supplement* 1 (1929): 157-215; Arthur Ruark and Harold Urey, *Atoms, Molecules, and Quanta* (New York: McGraw-Hill, 1930); Robert Lindsay and Henry Margenau, *Foundations of Physics* (New York: Wiley, 1936); and Kemble, *The Fundamental Principles of Quantum Mechanics* (New York: McGraw-Hill, 1937). On book reviews, see Kaiser, *American Physics*, chap. 4.

[12] Copies of Bernard Peters's notes from Oppenheimer's 1939 Berkeley course (Physics 221) are available in several libraries, including those at Caltech and Berkeley. On Oppenheimer's course, see also J. Robert Oppenheimer to Frank Oppenheimer, 10 Aug 1931, in *Robert Oppenheimer: Letters and Recollections*, ed. Alice Kimball Smith and Charles Weiner (Cambridge: Harvard University Press, 1980), 142-43; Robert Serber, "The early years," *PT* 20 (Oct 1967): 35-39, esp. 38; and Kai Bird and Martin Sherwin, *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (New York: Knopf, 2005), 82-85. As late as 1947, Berkeley's department secretary still fielded repeated requests for copies of Oppenheimer's 1939 lecture notes, which the department sold for $1.50 to cover reproduction and mailing costs. See the correspondence in *BDP* 4:16.

[13] Bloch's handwritten lecture notes, undated (ca. mid-1930s) may be found in *FB* 16:13, 16:14; Sherwood K. Haynes entry in Caltech "Bone Books," 6 Jan 1936, Box 1, vol. 2; Martin Summerfield entry, 10 Mar 1939, in *ibid*., Box 1, vol. 3; emphasis in original. The "Bone Books" are communal notebooks featuring students' handwritten reports on their qualifying oral examinations, written for the benefit of future examinees. This remarkable collection, spanning the period 1926-69, may be found in the Caltech archives.

[14] Vladimir Rojansky, *Introductory Quantum Mechanics* (New York: Prentice Hall, 1938); George H. Shortley, "Quantum mechanics" [review of Rojansky's textbook], *Science* 90 (3 Nov 1939): 420-22.

[15] Condon and Morse, *Quantum Mechanics*, 83.

[16] 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 Apr 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* 10:19; and in W. C. Kelly, "Survey of education in physics in universities in the United States," 1 Dec 1962, appendix 19; a copy of Kelly's report may be found in American Institute of Physics Education and Manpower division records, *NBL*, Box 9.

[17] Olival Freire, "A story without an ending: The quantum physics controversy, 1950-1970," *Science & Education* 12 (2003): 573-86; Freire, "The historical roots of 'Foundations of quantum physics' as a field of research (1950-1970)," *Foundations of Physics* 34 (2004): 1741-60; Freire, "Science and exile: David Bohm, the cold war, and a new interpretation of quantum mechanics," *HSPS* 36 (2005): 1-34; and Osvaldo Pessoa, Olival Freire, and Alexis De Greiff, "The Tausk controversy in the foundations of quantum mechanics," unpublished manuscript (2005). Cf. Evelyn Fox Keller, "Cognitive repression in contemporary physics," *AJP* 47 (Aug 1979): 718-21.

[18] Cf. Paul Forman, "Behind quantum electronics: National security as basis for physical research in the United States, 1940-1960," *HSPS* 18 (1987): 149-229; S. S. Schweber, "The mutual embrace of science and the military: ONR and the growth of physics in the United States after World War II," in *Science, Technology, and the Military*, ed. Everett Mendelsohn, M. Roe Smith, and Peter Weingart (Boston: Kluwer, 1988), 1-45; Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993); and Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), chap. 4.

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[19] Edward Teller (with Robert F. Christy and Emil J. Konopinski), "Lecture notes on quantum mechanics," autumn 1945, on 13, 25, 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. The double-slit example became a mainstay of physicists' popular descriptions of quantum mechanics. See, e.g., Niels Bohr, "Discussion with Einstein on epistemological problems in atomic physics," in *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp (Evanston, IL: Library of Living Philosophers, 1949), 201-41, on 211-20; J. Robert Oppenheimer, *Atom and Void: Essays on Science and Community* (Princeton: Princeton University Press, 1989), 126-28 (from Oppenheimer's 1962 Whidden Lectures at McMaster University); and Richard Feynman, *The Character of Physical Law* (Cambridge: MIT Press, 1965), 130-48.

[20] Sources for the notes are as follows: Paul F. Zweifel's handwritten notes on Lothar Nordheim's course, available in *NBL*; Freeman Dyson's handwritten lecture notes for his courses at Cornell and Princeton, in Prof. Dyson's possession (Institute for Advanced Study, Princeton); Enrico Fermi, *Notes on Quantum Mechanics*, 2nd ed. (Chicago: University of Chicago Press, 1995 [1961]), which reproduces the mimeographed handwritten lecture notes that Fermi distributed to his class; Elisha Huggins's handwritten notes on Richard Feynman's course, in Prof. Huggins's possession (Dartmouth College); Hans Bethe's handwritten lecture notes, in *HAB* 1:26; Evelyn Fox Keller's handwritten notes on Wendell Furry's course, in Prof. Keller's possesssion (MIT); Saul Epstein, *Lecture Notes in Quantum Mechanics*, mimeographed typed lecture notes, available in the University of Nebraska Physics Library; and Edward L. Hill, *Lecture Notes on Quantum Mechanics*, mimeographed typed lecture notes, available in the University of Minnesota (Minneapolis) Physics Library. Enrollments were estimated by adding the number of Ph.D.s granted in each department four and five years after the course in question (since graduate students usually took formal courses during their first two years of study, and average time to complete the Ph.D. in this period was five years). The possibilities for overcounting and undercounting by this procedure seem to balance out: since this staple subject was offered every year in these departments, students would not be expected to take it two years in a row; but counting only Ph.D. recipients neglects students who received master's degrees or left graduate school before attaining any degree. For the three courses in *Table 1* for which enrollment data is still available (Fermi 1954, Bethe 1956, and Epstein 1958), the estimated enrollments and the actual enrollments are an exact match: Julia Gardner (Reference Librarian, University of Chicago), email to the author, 16 Sep 2005; Bethe's course grade sheet, in the back of his lecture notes, *HAB* 1:26; and Roger D. Kirby (Chair, Physics Department, University of Nebraska), email to the author, 15 Sep 2005. Mary N. Morley (Registrar, Caltech) likewise confirmed that enrollment in Feynman's course was probably "30 or so," again matching the estimate based on degree conferrals: Morley, email to the author, 13 Sep 2005.

[21] Jacques Cattell, ed., *American Men of Science: A Biographical Dictionary*, 10th ed. (Tempe, AZ: Jacques Cattell Press, 1960), s.v. "Nordheim, Dr. L(othar) W(olfgang)"; William Laurence, "Teller indicates Reds gain on bomb," *NYT* (4 July 1954), 13; John A. Wheeler with Kenneth Ford, *Geons, Black Holes and Quantum Foam: A Life in Physics* (New York: Norton, 1998), 202-4.

[22] Paul Zweifel notes on Nordheim's course, 8-11.

[23] *Ibid*., 38-39, 58.

[24] M. Lois Marckworth, *Dissertations in Physics: An Indexed Bibliography of All Doctoral Theses Accepted by American Universities, 1861-1959* (Stanford: Stanford University Press, 1961), s.v. "Epstein, S. T."; Cattell, *American Men of Science*, s.v. "Epstein, Prof. Saul T(heodore); Epstein, *Lecture Notes in Quantum Mechanics*, 51-80. Of the faculty in this sample, only Freeman Dyson was younger than Epstein (by seven months).

[25] E. C. Kemble and E. L. Hill, "The general principles of quantum mechanics, Part II," *RMP* 2 (Jan 1930): 1-59; Cattell, *American Men of Science*, s.v. "Hill, Prof. Edward L(ee)"; Hill, *Lecture Notes on Quantum Mechanics*, vi, chap. 2: 8-9, chap. 3: 2-4, and chap. 4: 20. Hill completed his Ph.D. in 1928 at the University of Minnesota under John Van Vleck's direction: Marckworth, *Dissertations in Physics*, s.v. "Hill, Edward Lee."

[26] Leonard I. Schiff to Patrick Suppes (associate dean, Stanford), 15 Mar 1960, in *FB* 10:13; unsigned memo on Stanford graduate physics course enrollments, winter 1962-63, in *FB* 11:14; on course enrollments for first-year quantum mechanics at Berkeley, Columbia, and MIT, see Kelly, "Survey of education," appendix 15.

[27] George E. Uhlenbeck, "Quantum theory" [review of Tomonaga Sin-itiro, *Quantum Mechanics* (New York: Interscience, 1962)], *Science* 140 (24 May 1963): 886.

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[28] Dyson, Cornell 1952 notes, 1, 8, 11-20.

[29] Fermi, *Notes on Quantum Mechanics*, 19-22, 44-45; Bethe, "Quantum theory," lectures at teacher seminar, Albuquerque, New Mexico, July 1955, on lecture 4, p. 5 (copy available in *HAB*, 5:5); see also Bethe's 1956 lecture notes, and Philip Morrison to Hans Bethe, 24 Oct [1958], in *HAB* 1:26; Elisha Huggins's lecture notes on Feynman's course, 1-22; Richard Feynman and A. R. Hibbs, *Quantum Mechanics and Path Integrals* (New York: McGraw-Hill, 1965), 23; Evelyn Fox Keller's notes on Furry's course (pages not numbered); and Dyson, Princeton 1961 notes, 4.

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

[31] Schiff, *Quantum Mechanics*. See also Victor F. Weisskopf, "Quantum mechanics" [review of Schiff's textbook], *Science* 109 (22 Apr 1949): 407-8; Morton Hammermesh, "Quantum mechanics" [review of Schiff's textbook], *AJP* 17 (Nov 1949): 453-54; Abraham Klein, "Quantum mechanics" [review of Schiff's textbook, 3rd ed.], *PT* 23 (May 1970): 70-71; and John H. Gardner, "Quantum mechanics" [review of Schiff's textbook, 3rd ed.], *AJP* 41 (Apr 1973): 599-600. As Klein noted in his review, "Of the soluble examples for the motion of a single particle in a potential"—a staple topic for student exercises—"there are few problems worth consideration at this level that were not included." (Klein, 70.)

[32] E. M. Corson, "Quantum theory" [review of Bohm's textbook], *PT* 5 (Feb 1952): 23-24 ("rare example," "concise and well balanced interplay"); David R. Inglis, "Quantum theory" [review of Bohm's textbook], *AJP* 20 (Nov 1952): 522-23 ("goes a great deal further").

[33] 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; Shawn Mullet, *Political Science: The Red Scare as the Hidden Variable in the Bohmian Interpretation of Quantum Theory* (B.A. thesis, University of Texas at Austin, 1999); and Freire, "Science and exile."

[34] On sales of Schiff's book, see Malcolm Johnson (editor in chief for science and engineering, McGraw-Hill) to Leonard I. Schiff, 11 Mar 1964, in *LIS*, Box 9, Folder "Schiff: Quantum mechanics." In 1989, Dover Publications issued a reprint of Bohm's 1951 textbook.

[35] Edward Gerjuoy, "Quantum mechanics" [review of Leonard I. Schiff, *Quantum Mechanics*, 2nd ed. (New York: McGraw-Hill, 1955 [1949])], *AJP* 24 (Feb 1956): 118.

[36] Klein, "Quantum mechanics," 71; see also Philip M. Morse, "Quantum mechanics" [review of Schiff's textbook, 2nd ed.], *PT* 9 (Apr 1956): 26.

[37] Eyvind Wichmann, "Comments on QUANTUM MECHANICS, by L. I. Schiff (Second Edition)," n.d. (ca. Jan 1965), in *LIS* Box 9, Folder "Schiff: Quantum mechanics." Emphasis in original. The memo was forwarded to Schiff by Kenneth Zeigler (editorial director, McGraw-Hill) on 25 Jan 1965, and Schiff acknowledged Wichmann's lengthy comments on 5 Feb 1965, in the same folder.

[38] David Bohm, "A suggested interpretation of the quantum theory in terms of hidden variables," parts I and II, *PR* 85 (15 Jan 1952): 166-79, 180-92. When preparing a collection of the one hundred most important articles published in the *Physical Review* during its first century, the editors included both of these articles: H. Henry Stroke, ed., *The* Physical Review—*The First Hundred Years: A Selection of Seminal Papers and Commentaries* (New York: American Institute of Physics, 1995). See also James T. Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony* (Chicago: University of Chicago Press, 1994).

[39] Leonard I. Schiff to Melba Phillips, 6 Dec 1956 (with cc to Malcolm Johnson, McGraw-Hill); Hugh C. Wolfe (Cooper Union), review of Bohm's manuscript dated 29 Mar 1957, both in *LIS*, Box 13, Folder "Bohm." Schiff's comments quoted here were from a postscript he added only on the copy going to Johnson. Cf. Hugh C. Wolfe, "Quantum mechanics and physical reality," *PR* 49 (1936): 274. For more on American reactions to the 1935 Einstein-Podolsky-Rosen paper, see Kaiser, *American Physics*, chap. 4.

[40] Jacques Romain, "Introduction to quantum mechanics" [review of Chalmers W. Sherwin, *Introduction to Quantum Mechanics* (New York: Henry Holt, 1959)], *PT* 13 (Apr 1960): 62 ("avoids philosophical discussion"); D. L. Falkoff, "Principles of quantum mechanics" [review of William V. Houston, *Principles of Quantum Mechanics* (New York: McGraw-Hilll, 1951)], *AJP* 20 (Oct 1952): 460-61 ("philosophically tainted questions"); Herman Feshbach, "Clear and perspicuous" [review of Albert Messiah, *Quantum Mechanics*, vol. 1, trans. G. M. Temmer (New York: Interscience, 1961)], *Science* 136 (11 May 1962): 514 ("musty atavistic to-do"); and George L. Trigg, "Non-relativistic quantum mechanics"

[review of R. M. Sillitto, *Non-Relativistic Quantum Mechanics: An Introduction* (Chicago: Quadrangle Books, 1960)], *AJP* 29 (June 1961): 375-76 ("usual topics"). See also Philip Morse, "Quantum mechanics" [review of Franz Mandl, *Quantum Mechanics* (New York: Academic Press, 1954)], *PT* 8 (Oct 1955): 21-22. Cf. David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005), chap. 7.

[41] George H. Shortley, "Quantum mechanics" [review of Vladimir Rojansky, *Introductory Quantum Mechanics* (New York: Prentice-Hall, 1938)], *Science* 90 (3 Nov 1939): 420-22, on 421 ("lone friend"); see also J. L. Powell, "Quantum mechanics" [review of Schiff's 2nd edition], *Science* 123 (3 Feb 1956): 187; J. Gillis, "Quantum mecahnics for mathematicians and physicists" [review of Ernest Ikenberry, *Quantum Mechanics for Mathematicians and Physicists* (New York: Oxford University Press, 1962)], *PT* 15 (Dec 1962): 66; Eugene Guth, "Modern quantum theory" [review of Behram Kursunoglu, *Modern Quantum Theory* (San Francisco: W. H. Freeman, 1962)], *PT* 16 (Mar 1963): 61-62; Peter B. Kahn, "Introduction to wave mechancis" [review of Louis Harris and Arthur L. Loeb, *Introduction to Wave Mechanics* (New York: McGraw-Hill, 1963)], *AJP* 32 (Aug 1964): 648.

[42] Condon and Morse, *Quantum Mechanics*, 43-44, 52.

[43] Some of the American interwar textbooks had included more problems: Edward Condon and G. H. Shortley, *The Theory of Atomic Spectra* (Cambridge: Cambridge University Press, 1935) contained 27 problems; Linus Pauling and E. Bright Wilson's *Introduction to Quantum Mechanics* (New York: McGraw-Hill, 1935) contained 82 problems; and Rojansky's *Introductory Quantum Mechanics* contained 547 problems.

[44] Robert Eisberg and Robert Resnick, *Quantum Physics of Atoms, Molecules, Solids, Nuclei, and Particles* (New York: Wiley, 1974), vi, 25, 245, 322; Robert Eisberg, email to the author, 7 Oct 2005; and Robert Resnick, email to the author, 11 Oct 2005.

[45] Michael A. Morrison, Thomas L. Estle, and Neal F. Lane, *Quantum States of Atoms, Molecules, and Solids* (Englewood Cliffs, NJ: Prentice-Hall, 1976), xv; Stephen Gasiorowicz, *Quantum Physics* (New York: Wiley, 1974).

[46] Note that effects from the cross-over in enrollment patterns fell in the middle of one of the bins in Table 3 (1969-73), accounting for that time-period's large standard deviation.

[47] A. P. French and Edwin F. Taylor, *An Introduction to Quantum Physics* (New York: Norton, 1978), 100; cf. A. P. French, *Principles of Modern Physics* (New York: Wiley, 1958).

[48] An important exception was Siegfried Flügge, *Rechenmethoden der Quantentheorie* (Berlin: Springer, 1947), later translated and expanded as Flügge, *Practical Quantum Mechanics* (New York: Springer, 1971), 2 vols.

[49] Wolfgang Finkelnburg, *Atomic Physics*, trans. George E. Brown (New York: McGraw-Hill, 1950 [1948]), 245-46; see also 3-4, 211, 240-47. On Finkelnburg's efforts during the war and his relationship with Heisenberg, see Finkelnburg, "The fight against Party physics (ca. 1946)," in *Physics and National Socialism: An Anthology of Primary Sources*, ed. Klaus Hentschel and Ann M. Hentschel (Boston: Birkhäuser, 1996), 339-45; and David Cassidy, *Uncertainty: The Life and Science of Werner Heisenberg* (New York: W. H. Freeman, 1992), 446, 454.

[50] Arthur March, *Quantum Mechanics of Particles and Wave Fields* (New York: Wiley, 1951); Walter Heitler, *Elementary Wave Mechanics*, 2nd ed. (Oxford: Clarendon, 1956 [1945]), 16. On March's relationship with Schrödinger, see Walter Moore, *Schrödinger: Life and Thought* (New York: Oxford University Press, 1989), 163-64, 249, 272-79, 296, 318-24, 350, 418, 423, 456, 467-68.

[51] Albert Messiah, *Quantum Mechanics*, trans. G. H. Temmer and J. Potter, 2 vols. (New York: Interscience, 1961-1962 [1959]), 1: 115-161; Feshbach, "Clear and perspicuous," 514 ("excessive," "overdone"); Peter G. Bergmann, "Foundations of quantum theory" [review of John von Neumann, *Mathematische Grundlagen der Quantenmechanik* (Berlin: Springer, 1968), and O. Costa de Beauregard, *Precis de Mechanique Quantique Relativiste* (Paris: Dunod, 1967)], *PT* 22 (May 1969): 85-87 ("epistemological implications"). A number of American instructors adopted Messiah's book in the mid-1960s, using it piecemeal (much as Dyson had done with Bohm's book a decade earlier); and most of them gladly dropped Messiah's book when the third edition of Schiff's textbook appeared in 1968: John H. Gardner, "Quantum mechanics," [review of Schiff's 3rd ed.], *AJP* 41 (Apr 1973): 599-600.

[52] A. S. Davydov, *Quantum Mechanics*, trans. D. ter Haar (New York: Pergamon, 1965); L. D. Landau and E. M. Lifshitz, *Quantum Mechanics: Non-Relativistic Theory*, trans. J. B. Sykes and J. S. Bell (New York: Pergamon, 1965); A. A. Sokolov, Y. M. Loskutov, and I. M. Ternov, *Quantum Mechanics*,

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trans. Scripta Technica (New York: Holt, Rinehart, Wilson, 1966); L. D. Landau and E. M. Lifshitz, *Quantum Mechanics*, trans. J. B. Sykes and J. S. Bell (New York: Pergamon, 1974); A. S. Davydov, *Quantum Mechanics*, trans. D. ter Haar, 2nd ed. (New York: Pergamon, 1976); and L. D. Landau and E. M. Lifshitz with L. P. Pitaevskii, *Quantum Mechanics: Non-Relativistic Theory*, trans. J. B. Sykes and J. S. Bell, 3rd ed. (New York: Pergamon, 1977). "Problem books" included: I. I. Gol'dman, V. D. Krivchenkov, V. I. Kogan, and V. M. Galitskii, *Problems in Quantum Mechanics*, trans. D. ter Haar (New York: Academic Press, 1960); I. I. Gol'dman and V. D. Krivchenkov, *Problems in Quantum Mechanics*, ed. B. T. Geilikman, trans. E. Marquit and E. Lepa (New York: Dover, 1961); and V. I. Kogan and V. M. Galitskiy, *Problems in Quantum Mechanics*, trans Scripta Technica (Englewood Cliffs, NJ: Prentice-Hall, 1963). On the renowned Landau-Lifshitz textbook series, see also Karl Hall, *Purely Practical Revolutionaries: A History of Stalinist Theoretical Physics* (Ph.D. dissertation, Harvard University, 1999), chap. 13; and Hall, "'Think less about foundations': A short course on Landau and Lifshitz's *Course of Theoretical Physics*," in Kaiser, ed., *Pedagogy and the Practice of Science*, 253-86.

[53] Hall, *Purely Practical Revolutionaries*; Andrei Linde, "How physics fostered freedom in the USSR," *PT* 45 (June 1992): 13; David Holloway, *Stalin and the Bomb* (New Haven: Yale University Press, 1994), 26-28, 206-13; Alexei Kojevnikov, "President of Stalin's Academy: The mask and responsibility of Sergei Vavilov," *Isis* 87 (1996): 18-50.

[54] M. Mizushima, "Blokhintsev's textbook in translation" [review of D. I. Blokhintsev, *Quantum Mechanics* (New York: Gordon and Breach, 1964)], *Science* 154 (14 Oct 1966): 253.

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

[56] On incoming graduate-student enrollments in Stanford's physics department, see faculty meeting minutes, 12 Jan 1970, in *FB* 12:1; and unsigned memo, "Graduate enrollment and projection," 2 Feb 1972, in *FB* 12:8. See also A. L. Fetter (chair, Graduate Study Committee) memo to department faculty, 28 Feb 1972, in *FB* 12:8; and the comprehensive exam (21-22 Sep 1972) in *FB* 12:10. Fears of "factory" expressed in Paul Kirkpatrick, memo to Stanford physics department faculty, 19 Jan 1956, in *FB* 10:2; and in Ed Jaynes, memo to Stanford physics department faculty, 27 Apr 1956, in *FB* 10:3; see also Felix Bloch, memo to physics department faculty, 10 July 1956, in *FB* 10:4.

[57] W. E. Meyerhof memo to Stanford's physics graduate students, 29 Sep 1972, in *FB* 12:10; on the Harvard course, see Olival Freire, "Philosophy enters the optics laboratory: Bell's theorem and its first experimental tests, 1965-1982," unpublished manuscript (2005), on p. 29.

[58] Evelyn Fox Keller, "The anomaly of a woman in physics," in *Working it Out*, ed. Sara Ruddick and Pamela Daniels (New York: Pantheon, 1977), 78-91, on 83. Remarkably similar recollections appear in Daniel F. Styer, "The new quantum universe" [book review of Tony Hey and Patrick Walters, *The New Quantum Universe* (New York: Cambridge University Press, 2003)], *Physics in Perspective* 7 (2005): 381.