2 Making Tools Travel: Pedagogy and the Transfer of Skills in Postwar Theoretical Physics

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Feynman diagrams revolutionized nearly every aspect of theoretical physics during the second half of the twentieth century. The young American theorist Richard Feynman introduced his diagrams in the late 1940s as a bookkeeping device for simplifying lengthy calculations in one area of physics—quantum electrodynamics, physicists' quantum-mechanical description of electromagnetic forces. Soon the diagrams gained adherents throughout the fields of nuclear and particle physics. Not long thereafter, other theorists adopted—and subtly adapted—Feynman diagrams for many-body applications in solid-state theory. By the end of the 1960s, a few physicists even wielded the simple line drawings for calculations in gravitational physics. With the diagrams' aid, entire new calculational vistas opened for physicists; theorists learned to calculate things that many had barely dreamed possible before World War II. With the list of diagrammatic applications growing ever longer, Feynman diagrams helped to transform the way physicists saw the world, and their place within it.

With few exceptions, historians, philosophers, and sociologists of science have overlooked the crafting and appropriation of theoretical tools such as Feynman diagrams. Instead, research in theoretical sciences has been analyzed as abstract thought, wholly separated from anything like labor, activity, or skill. Worldviews or paradigms seemed to be the appropriate unit of analysis, and the challenge became charting the birth and conceptual development of particular ideas. In short: more "night thoughts" than desk work; more *Weltbild* than *Fingerspitzengefühl*.[1] Yet since at least the middle of the twentieth century—and, arguably, during earlier periods as well—most theorists have not spent their days (nor, indeed, their nights) in some philosopher's dreamworld, weighing one cluster of disembodied concepts against another, picking and choosing among so many paradigms. Rather, their main task has been to *calculate*. They have tinkered with models and estimated effects, always trying to reduce the inchoate confusion of "out there"—an "out there" increasingly percolated through factory-sized apparatus and computer-triggered detectors—into tractable representations. They have accomplished such translations by fashioning theoretical tools and performing calculations. Theorists use calculational tools, in other words, to mediate between various kinds of representations of the world. These tools provide the currency of everyday work.

Since the late 1940s, generations of physicists have turned more and more often to Feynman diagrams as their tool of choice. For this reason, I follow Feynman diagrams around, focusing on how physicists fashioned—and constantly re-fashioned—the diagrams into a calculational tool, a theoretical practice. Once we begin to examine the tools of theory, we must also study the tools' users—a shift in emphasis from the isolated thoughts of Nobel laureates to the pedagogical work involved in training large numbers of researchers to approach physical questions in similar ways. After all, tools such as Feynman diagrams never apply themselves; physicists must be trained to use them, and to interpret and evaluate the results in certain ways. A link therefore always exists between research practices and the scientific practitioners who put them to work. This link, more often than not, involves some kind of pedagogical activity, such as advisors mentoring graduate students or postdocs working closely together. In this essay, I use the example of Feynman diagrams to disaggregate some of the different types of pedagogical activities involved in crafting theoretical tools and making them travel.[2]

My project is organized around three main questions: How did the diagrams spread so quickly? For what were they used during the late 1940s and throughout the 1950s and the 1960s? Given this variety of distinct applications, why did the diagrams "stick"? Resolving each of these questions clarifies the role of a specific pedagogical process for training young theorists during the decades after World War II. In pursuing these questions, it is helpful to consider two distinct meanings of the word "dispersion." One cluster of meanings is especially pertinent for the first question, regarding how the diagrams spread so quickly: "To distribute from a main source or centre . . . ; to put into circulation." A second meaning of "dispersion" is helpful for navigating through the ever-expanding, competing uses and interpretations given to the diagrams: "To cause to separate in different directions . . . ; to spread in scattered order."[3] "Dispersion" thus captures at once the work required to make theoretical tools travel and the plasticity of those tools when they travel.

An Introduction in the Poconos

Feynman introduced his diagrams in a private, by-invitation-only meeting in the spring of 1948. Twenty-eight theorists gathered in the Pocono Manor Inn, in rural Pennsylvania, for several days of intense discussions. Most of the young theorists were preoccupied with the problems of quantum electrodynamics (QED), the physicists' description of how electrons interact with light. Physicists had known since the early 1930s that QED produced unphysical infinities, rather than finite answers, when pushed beyond its lowest-order, simplest approximations. Feynman began doodling his diagrams in the context of working on the problems of QED. Even apart from the divergence difficulties, calculations within QED had long been infamously unwieldy—often single terms within a calculation could stretch over four or five lines of algebra, and it was all too easy to conflate or (worse) omit terms within the algebraic morass. As Feynman explained in his talk at the Pocono Manor Inn, his new diagrams could serve as convenient guides for marching through the thickets of QED calculations.

As one of his first examples, Feynman considered the problem of electron-electron scattering. He drew a simple diagram on the blackboard, similar to the one later reproduced in his first article on the new diagrammatic techniques.[4] (See figure 2.1.) Feynman explained how the diagram provided a shorthand for a uniquely associated mathematical description: an electron had a certain likelihood, which Feynman called $K(5,1)$, to move as a free particle from the spacetime point x_1 to x_5; the other incoming electron moved freely—with likelihood $K(6,2)$—from spacetime point x_2 to x_6. This second electron could then emit a virtual photon at x_6, which would move—with likelihood $\delta(s_{56}^2)$—to x_5, where the first electron would absorb it. (Here s_{56} represented the

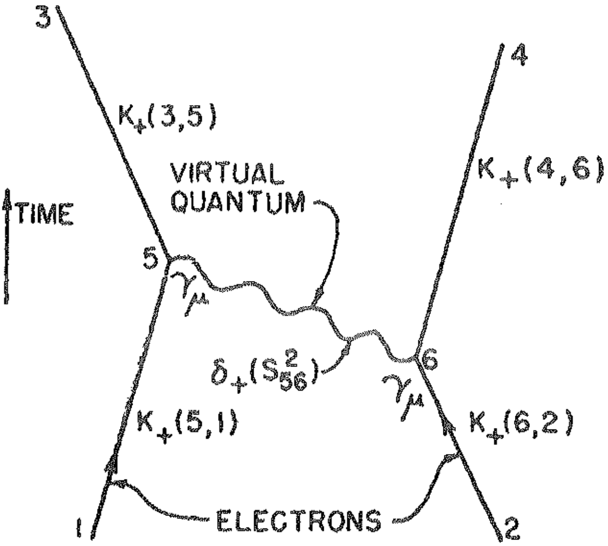


Figure 2.1
The simplest Feynman diagram for electron-electron scattering. Source: Richard Feynman, "Space-time approach to quantum electrodynamics," *Physical Review* 76 (1949): 769–789, on 772.

distance in space and time that the photon traveled.) The likelihood that an electron would emit or absorb a photon was $e\gamma_\mu$, where e was the electron's charge and γ_μ was a vector of Dirac matrices. Having given up some of its energy and momentum, the electron on the right would then move from x_6 to x_4. The electron on the left, upon absorbing the photon and hence gaining some additional energy and momentum, would scatter from x_5 to x_3. In Feynman's hands, then, this simple diagram stood in for the following mathematical expression (itself written in terms of the abbreviations K and δ)[5]:

$$-ie^2 \iint d^4x_5\, d^4x_6\, K(3,5)\, K(4,6)\, \gamma_\mu\, \gamma_\mu\, \delta(s_{56}{}^2)\, K(5,1)\, K(6,2).$$

In this simplest process, the two electrons traded just one photon between them; the straight electron lines intersected with the wavy photon line in two places, called "vertices." The associated mathematical term therefore contained two factors of the electron's charge, e—one for each vertex. When squared, this expression gave a fairly good estimate for the probability that two electrons would scatter. Yet both Feynman and his listeners knew that this was only the start of the calculation. In principle, the two electrons could trade any number of photons back and forth—two, seven, forty-five, one million; there was an infinite number of distinct ways the two electrons could interact, and each of these possibilities had to be included. These additional possibilities, involving more and more interactions and hence more factors of e, should have been small compared with the lowest-order approximation, since $e^2 \sim 1/137$. That is, the additional terms should have been mere "perturbations" to the basic, starting calculation involving the lone, single photon. Feynman used his new diagrams to delineate the various possibilities. For example, there were nine different ways that the electrons could trade two photons back and forth, each of which would involve four vertices (and hence their associated mathematical expressions would contain e^4 instead of e^2; see figure 2.2). As in the simplest case (involving only one photon), Feynman could walk through the associated mathematical contribution from each of these diagrams, plugging in Ks and δs for each electron and photon line and connecting them at the vertices with factors of $e\gamma_\mu$.

The main difference from the single-photon case was that most of the corresponding integrals for the diagrams in figure 2.2 blew up to infinity, rather than providing finite answers—just as physicists had found with their non-diagrammatic calculations since the late 1920s. Feynman next showed how one could remove some of the troublesome infinities by a combination of calculational tricks, some of his own design and others borrowed.[6] (The removal of infinities from QED calculations was dubbed "renormalization.") What is most important for our purposes is to consider Feynman's order of operations: *start* with the diagrams as a mnemonic aid in order to write down the relevant integrals, and only later alter these integrals, one at a time, to remove the infinities.

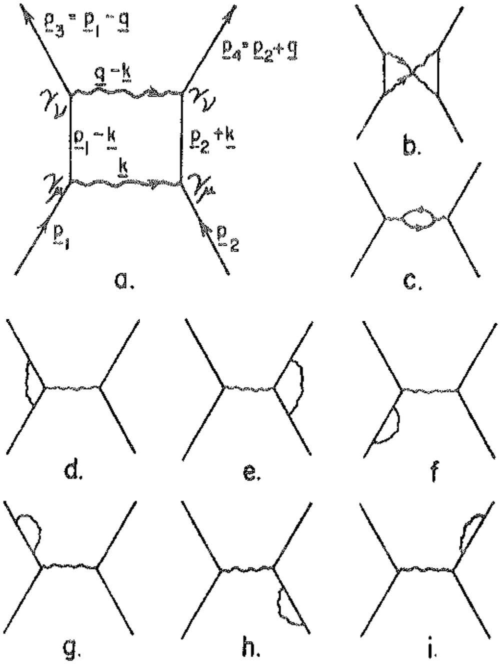


Figure 2.2
Feynman diagrams for electron-electron scattering correction terms; these diagrams play the role of "bookkeepers" for the perturbative analysis. Source: Richard Feynman, "Space-time approach to quantum electrodynamics," *Physical Review* 76 (1949): 769–789, on 787.

Diagrams in hand, Feynman had thus solved a puzzle that had stymied the world's best theoretical physicists for two decades. We might expect the reception from his colleagues at the Pocono Manor Inn to have been appreciative, to say the least. Yet things had not gone well at the Pocono meeting. For one thing, the odds were stacked against Feynman: his presentation followed a day-long lecture by Harvard's wunderkind, Julian Schwinger. Schwinger had arrived at a different method of removing the infinities from QED calculations, and the audience sat glued to their seats—pausing only briefly for lunch—as Schwinger unveiled his derivation. Coming late in the day, in contrast, Feynman's blackboard presentation was rushed and unfocused. No one seemed to be able to follow what Feynman was doing. He suffered frequent interruptions from Niels Bohr, Paul Dirac, and Edward Teller, each of whom pressed Feynman on how his new doodles fit in with the established principles of quantum physics. Others asked more generally, in exasperation, what *rules* governed the diagrams' use. By all accounts, Feynman left the meeting disappointed, even depressed.[7]

Feynman's frustration with the Pocono presentation has been noted often. Overlooked in these accounts, however, is the crucial fact that this confusion lingered long after the diagrams' inauspicious introduction. Even some of Feynman's closest friends and colleagues had difficulty following where his diagrams came from or how they were to be used. For example, Feynman had often discussed his new diagrammatic approach with Hans Bethe, both before and after the Pocono presentation. Bethe had been Feynman's boss at wartime Los Alamos and was at the time his senior colleague at Cornell; he was also a leading expert on QED and its problems. Yet Bethe wrote to Feynman while vacationing in England that summer that he tried to use Feynman's diagrams and kept getting stuck; Feynman had to coach his boss through the calculations through the mail.[8] Another theorist who had attended the Pocono meeting, Robert Marshak, remained flummoxed when trying to apply the new techniques. In a paper completed in December 1948, Marshak thanked Feynman in a footnote for completing a diagrammatic calculation at his request, since Marshak had been unable to undertake the calculation himself.[9]

Two years after the Pocono meeting, young physicists still struggled to make sense of the diagrams. "That great care must be taken" when calculating with "the different graphs," wrote a graduate student to two postdocs in February 1950, "is shown by the fact that we have obtained between us three different answers" for what was supposed to have been the same diagrammatic calculation. There followed a six-page enclosure that detailed everything from how to draw the various diagrams (which lines should be dashed or wavy, which lines should contain arrows); to which kinds of lines could and could not be inserted within a given diagram; to which kinds of diagrams contributed to which physical processes—all of which came before questions of how to translate the pages upon pages of tiny squiggles into mathematical expressions.[10]

Still the uncertainty lingered. During August 1952, the great architect of quantum theory, Wolfgang Pauli, admitted to a younger colleague that he was "not enough of an expert in 'graphs' to be able to check all the details" of a diagram-filled dissertation he had just received in the mail.[11] As late as May 1953—fully five years after Feynman had unveiled his new technique at the Pocono meeting—Stanford's Leonard Schiff wrote in a letter of recommendation for a recent graduate that his student *did* understand the diagrammatic techniques, and had used them in his dissertation.[12] The need to single out such hard-won skills for praise illustrates the larger point: as late as 1953, graduate students could not be assumed to understand or to be well practiced with Feynman's diagrams. The new techniques were neither automatic nor obvious for many physicists—the diagrams did not spread on their own.

Evidence of Dispersion

And yet the diagrams did spread, and spread quickly, starting just months after Feynman's private presentation at Pocono. Articles that made use of Feynman's diagrams began to stream into the *Physical Review*. Eight such articles were published in 1949 alone, followed by an exponential rise, doubling every 2.2 years. By 1952, the biweekly journal carried, on average, one article making use of Feynman diagrams per issue. Within a few years, the articles were coming in from physicists on the East Coast, on the West Coast, and in the Midwest. All these articles—submitted by as many as 114 different authors by 1954—had been written before any textbooks on the new techniques had been published.[13] Somehow, Feynman's new techniques, introduced to little fanfare (if not outright hostility) before a small, private gathering, had made their way onto the scratch pads and blackboards of more than a hundred geographically dispersed physicists within just a few short years.

It wasn't just any physicists who picked up the diagrams and used them in print. Some actively resisted the diagrams—the most famous was Julian Schwinger, who scoffed years later that Feynman diagrams had "brought computation to the masses" but were at best a matter of "pedagogy, not physics." Schwinger's students at Harvard never encountered the new techniques in their advisor's polished lectures, and they avoided using the diagrams in their dissertations.[14] Other physicists likewise continued to pursue their calculations within QED without the aid of Feynman diagrams, though their relative numbers began to dwindle during the early and mid 1950s.

Scrutinizing those physicists who did begin to use the diagrams provides important clues as to how the new techniques spread. The authors of these diagrammatic articles shared three traits: they were *theorists*, they were *young*, and they were *in personal contact* with one another. By the 1960s, Feynman diagrams had become as routine for experimentalists as for theorists. In the early years, however, all the diagrammatic articles were written by theoretical physicists. Moreover, more than 80 percent of the authors were still in the midst of their training when they submitted their first diagrammatic articles, either as graduate students or as postdocs. Most of the others began using Feynman diagrams in print while young instructors or assistant professors, not more than seven years past their doctorates.[15] Older physicists simply did not "re-tool." Clearly something pedagogical was going on.

For the earliest users of Feynman diagrams, personal contact with other users of the diagrams proved critical. The acknowledgments to the articles in the *Physical Review* that included Feynman diagrams reveal a remarkably closed set. The names that recur most frequently in the acknowledgments—Freeman Dyson, Hans Bethe, Richard Feynman, Norman Kroll, Abraham Klein, Abraham Pais, Fritz Rohrlich—were the same people who published the greatest number of diagrammatic articles during this period. In fact, the 17 authors thanked most often in the acknowledgments contributed more than half of the diagrammatic articles published in the *Physical Review* between 1949 and 1954. Often the members of this core set thanked each other. Still more often, however, it was *other* young physicists—especially those making use of the diagrams for the first time—who thanked these authors. By and large, diagram users who published in the *Physical Review* were in personal contact with other diagram users. Feynman diagrams did not spread by texts alone. Rather, a particular pedagogical pattern put the diagrams in circulation.

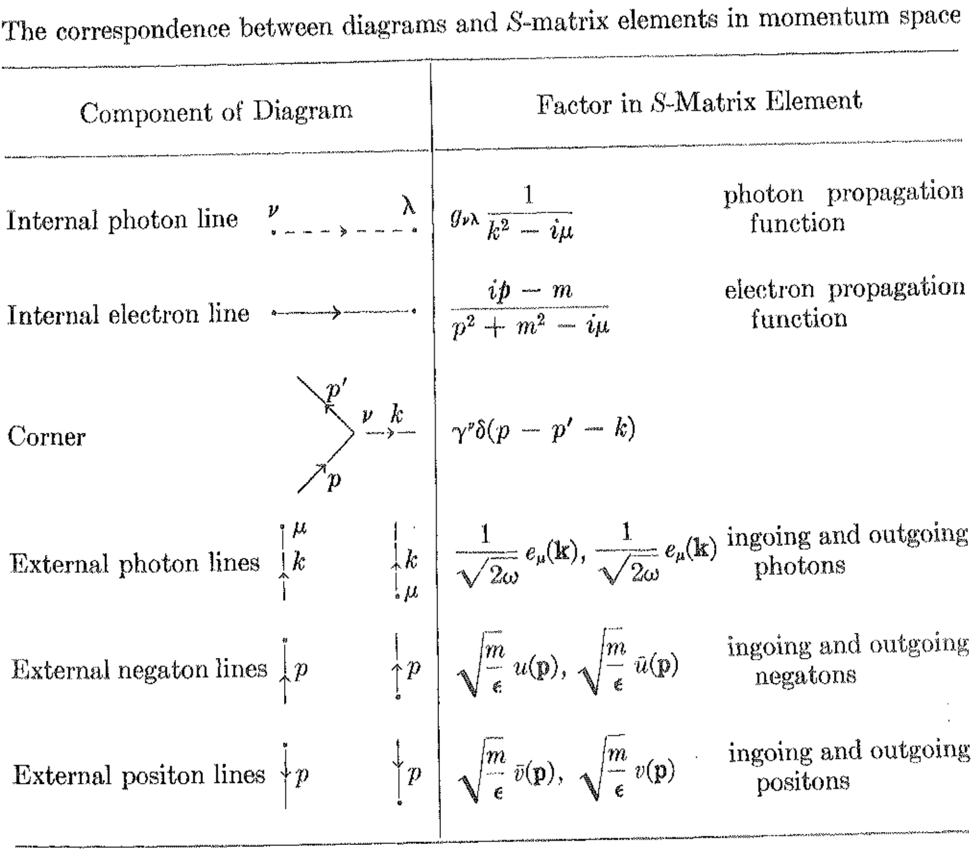
Dispersing the Diagrams: Dyson and the Postdoc Cascade

The spread of the diagrams was due mainly to the efforts of Feynman's younger associate Freeman Dyson. Dyson studied mathematics in Cambridge, England before traveling to the United States on a Commonwealth Fellowship to pursue graduate studies in theoretical physics. He arrived at Cornell in the fall of 1947, to study with Hans Bethe. Over the course of that year, he also began meeting with Feynman, just at the time that Feynman was working out his new approach to QED. Dyson and Feynman talked often during the spring of 1948 about Feynman's diagrams and how they could be used—conversations that continued in close quarters when the two drove from Ohio to Albuquerque together that summer, just a few months after Feynman's Pocono presentation.[16] Later that summer, Dyson attended the summer school on theoretical physics at the University of Michigan, which featured detailed lectures by Julian Schwinger on his non-diagrammatic approach to renormalization. The summer school offered Dyson the opportunity to talk informally and at length with Schwinger in much the same way that he had already been talking with Feynman. Thus, by September 1948, Dyson—and Dyson alone—had spent intense, concentrated time talking directly with both Feynman and Schwinger about their new techniques. At the end of the summer, Dyson took up residence at the Institute for Advanced Study in Princeton, where he spent the second and final year of his Commonwealth Fellowship.[17]

Dyson as Diagrammatic Ambassador

Soon after arriving in Princeton, Dyson submitted an article to the *Physical Review* that compared Feynman's and Schwinger's methods. (Dyson also analyzed the methods of the Japanese theorist Tomonaga Sin-itiro, who had worked on the problem during and after the war; soon after the war, Schwinger arrived independently at an approach very similar to Tomonaga's.) More than just compare, Dyson demonstrated the mathematical equivalence of all three approaches—all this before Feynman had written a single article on his new diagrams. Dyson's early article, and a lengthy follow-up article submitted that winter, were both published months in advance of Feynman's own papers. Even years after Feynman's now-famous articles appeared in print, Dyson's pair of articles were cited more often than Feynman's.[18]

In these early papers, Dyson derived *rules* for the diagrams' use—precisely what Feynman's frustrated auditors at the Pocono meeting had found lacking. Dyson's articles offered a "how-to" guide, including step-by-step instructions for how the diagrams should be drawn and how they were to be translated one-for-one into their associated mathematical expressions. These Dysonian rules were captured a few years later by some of his earliest recruits at the Institute, Josef Jauch and Fritz Rohrlich, in their 1955 textbook *The Theory of Photons and Electrons*. (See figure 2.3.)



The correspondence between diagrams and S-matrix elements in momentum space

Component of Diagram	Factor in S-Matrix Element	
Internal photon line	$g_{\nu\lambda}\dfrac{1}{k^2 - i\mu}$	photon propagation function
Internal electron line	$\dfrac{i\not{p} - m}{p^2 + m^2 - i\mu}$	electron propagation function
Corner	$\gamma^\nu\delta(p - p' - k)$	
External photon lines	$\dfrac{1}{\sqrt{2\omega}}\,e_\mu(\mathbf{k}),\ \dfrac{1}{\sqrt{2\omega}}\,e_\mu(\mathbf{k})$	ingoing and outgoing photons
External negaton lines	$\sqrt{\dfrac{m}{\epsilon}}\,u(\mathbf{p}),\ \sqrt{\dfrac{m}{\epsilon}}\,\bar{u}(\mathbf{p})$	ingoing and outgoing negatons
External positon lines	$\sqrt{\dfrac{m}{\epsilon}}\,\bar{v}(\mathbf{p}),\ \sqrt{\dfrac{m}{\epsilon}}\,v(\mathbf{p})$	ingoing and outgoing positons

Figure 2.3

The "Feynman rules" in momentum space, following Dyson's prescriptions. Source: Jauch and Rohrlich, *Theory of Photons and Electrons* (Addison-Wesley, 1955), 154.

In addition to systematizing Feynman's diagrams, Dyson *derived* the form and use of the diagrams from first principles, something that Feynman had not broached at all.[19] From the mathematics governing QED, for example, Dyson showed why each Feynman diagram had to have exactly two electron lines meet one photon line at each vertex. Dyson also included tiny arrows in the diagrams to distinguish particles (such as electrons) from antiparticles (such as positrons). With the aid of the arrows, Dyson could be sure to distinguish diagrams that differed only by the interchange of electrons and positrons—a distinction that had been lost on Feynman during some of his earliest diagrammatic calculations.[20] Beyond all these clarifications and derivations, Dyson—diagrams in hand—went on to demonstrate how the troubling infinities within QED could be removed systematically from any calculation, no matter how complicated. Until that time, Tomonaga, Schwinger, and Feynman had worked only with the first round of perturbative correction terms, and only in the context of a few specific problems. Building on the topology of the diagrams, Dyson generalized from these worked examples to offer a proof of renormalizability.

At first, Dyson's new boss at the Institute for Advanced Study, J. Robert Oppenheimer, remained underwhelmed by Feynman's diagrams and Dyson's use of them. Throughout the fall of 1948, Dyson gave a series of seminars on his diagrammatic work at the Institute. At every one, Oppenheimer behaved in his usual way, interrupting the talk with scathing criticisms or sarcastic dismissals. Unable to get a word in or to match Oppenheimer's renowned rhetorical skills face to face, Dyson sat down at his typewriter in exasperation one October evening to write a memorandum to Oppenheimer; this seemed to be the only way Dyson could say his peace. He argued that the diagrams were "considerably easier to *use, understand*, and *teach*" than the other approaches. Even the memorandum proved insufficient: only after his former advisor, Hans Bethe, intervened with Oppenheimer did Dyson get a fair hearing. In November 1948, after a new round of talks at the Institute, Oppenheimer left a simple note saying "Nolo contendere" in Dyson's mailbox.[21]

Creating a Factory of Feynman Diagrams

From that moment on, Dyson converted the Institute for Advanced Study into a factory of Feynman diagrams. To understand how, we must first step back and consider changes in physicists' postdoctoral training during this period. Before the war, not all physicists who completed PhDs in the United States went on for additional postdoctoral training; it was still common to take a job with either industry or academia directly from one's PhD. Theoretical physicists were still a small minority among physicists within the United States before the war, and those who did pursue postdoctoral training usually traveled to the established European centers for their postdoctoral study. Only in Cambridge, in Copenhagen, in Göttingen, or in Zurich could these young American theorists learn the music (in I. I. Rabi's famous words) and not just the libretto of research in physics. Upon their return, many of these same American physicists—including Edwin Kemble, John Van Vleck, and John Slater, as well as Rabi and Oppenheimer—endeavored to build up domestic postdoctoral training grounds for young theorists.[22]

Soon after the war, the Institute for Advanced Study, newly under Oppenheimer's direction, became one of the most important centers for young theorists completing postdoctoral work. Having achieved worldwide fame for his role as director of the wartime Los Alamos laboratory, Oppenheimer was in constant demand after the war. He left his Berkeley post in 1947 to become director of the Princeton Institute in part to have a closer perch to his newfound consulting duties in Washington, D.C. He made it a condition of his accepting the position that he be allowed to increase the numbers of young, temporary members within the physics staff—that is, to turn the Institute into a center for theoretical physicists' postdoctoral training. The Institute quickly became a common stopping-ground for young theorists, who circulated through what Oppenheimer called his "intellectual hotel" for two-year postdoctoral stays.[23] The Institute quickly became one of the most commonly visited sites for young theorists upon completion of their PhDs. Oppenheimer captured something of what life was like at the Institute in a letter to Wolfgang Pauli in February 1952. In the midst of trying to lure Pauli to return to the Institute as a permanent senior member, Oppenheimer explained that the Institute "is not a school in the sense that even the younger people are not listening to lectures or working for doctor's degrees; but it is a school in the sense that almost everyone who comes learns of parts of physics . . . which are new to him. It is a very fertile group."[24]

The focused yet informal nature of the Institute's postdoctoral "school" proved to be crucial for spreading Feynman diagrams around. When Dyson arrived at the Institute in the fall of 1948—just a year after Oppenheimer became director and began to implement his vision of the Institute as a center for theorists' postdoctoral study—he joined eleven other junior theorists. One of the new buildings at the Institute, which was supposed to contain offices for the new visitors, had not been completed on time, so the entire crew of theory postdocs spent much of that fall semester huddled around desks in a single office. The close quarters bred immediate collaborations among the postdocs.[25] Very quickly, Dyson emerged as a kind of ringleader, training his peers in the new diagrammatic techniques and coordinating a series of collaborative calculations involving the diagrams.

The most famous of the diagrammatic collaborations was a paper by Dyson's fellow postdocs Robert Karplus and Norman Kroll on the fourth-order corrections to an electron's magnetic moment. Their paper, submitted to the *Physical Review* during the fall of 1949, announced that their purpose was "to demonstrate in a complete calculation of a particular example the feasibility of Dyson's program," in the prosecution of which "Dyson's methods have been followed quite closely."[26] Similar acknowledgments of Dyson appear in the other diagrammatic papers submitted by Institute postdocs at the time. Dyson's efforts with his fellow postdocs had become so effective that the ever-observant (and ever-sarcastic) Pauli wrote to another of the Institute's young theorists, Abraham Pais, in May 1949, asking what Dyson and the rest of "the 'Feynman-school'" were working on.[27]

Next these postdocs, having been tutored in the niceties of diagrammatic calculations by Dyson, left the Institute to take teaching jobs elsewhere. More than 80 percent of the *Physical Review* articles that used Feynman diagrams between 1949 and 1954 were submitted by these Institute postdocs or by graduate students (and other colleagues) whom they trained upon arriving at their new jobs.[28] The great majority of the 114 authors who made use of the diagrams in the *Physical Review* during the period 1949–1954 did so because they had been trained in the new techniques by Dyson or by one of his newly minted Institute apprentices. (All but two of the remaining authors interacted directly with Feynman.) The acknowledgments in graduate students' dissertations from Berkeley, Rochester, Chicago, Iowa City, Bloomington, Madison, Urbana, and Ithaca—and places in between—confirm the role of the Institute postdocs in taking the new techniques with them and teaching their own recruits how to use them. In this way, Feynman diagrams spread throughout the United States by means of a postdoc cascade, emanating from the Institute for Advanced Study. Personal mentoring and the postdocs' circulation thus dispersed the diagrams, distributing them from a main center and putting them into circulation.[29]

Dispersion in Form, Use, and Meaning: Local Schools

Even as the diagrams began to disperse in this first sense, thanks to the postdoc cascade, they also underwent a second kind of dispersion, becoming more and more differentiated or "spread in scattered order." Physicists appropriated Feynman diagrams for many different kinds of calculations, often modifying the pictorial form of the diagrams they and their students drew to better suit their new purposes. Consider, for example, the diagrams from 1949–1954 reproduced here as figure 2.4, each of which was labeled a "Feynman diagram" by its author. Clearly Feynman's original diagram (figure 2.1) was not the only model on offer at the time.

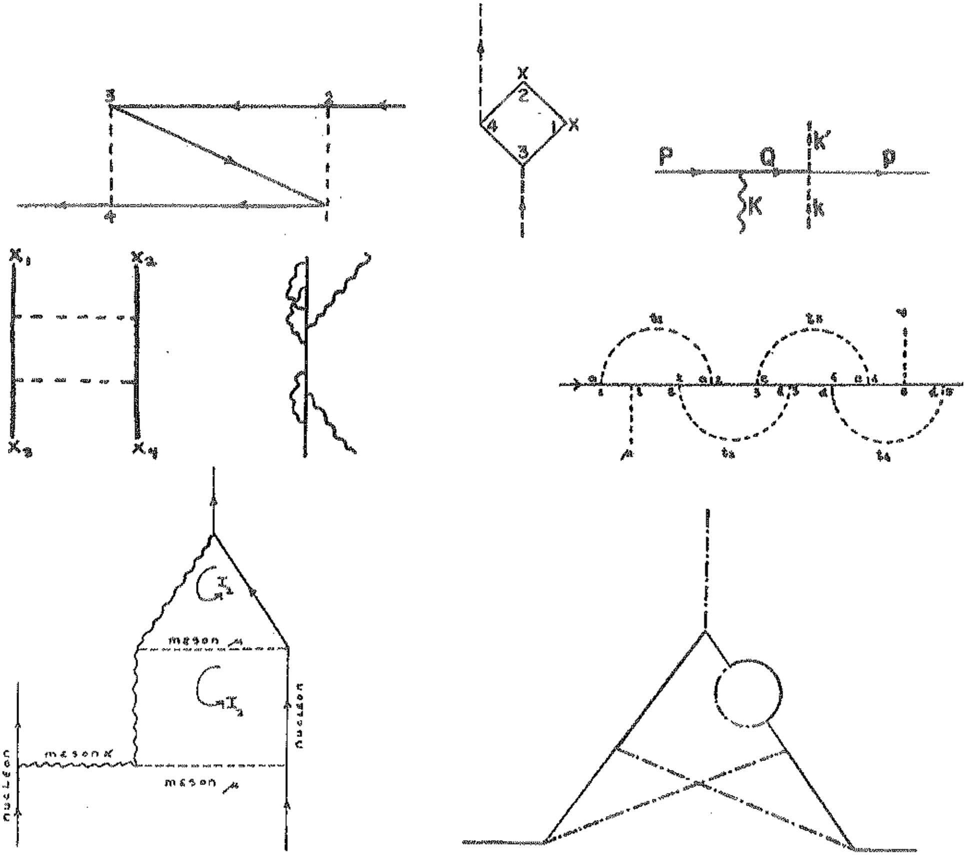


Figure 2.4

Feynman diagrams differentiate. Sources: Top row, left to right: Felix Villars, "Quantum electrodynamics," unpublished lecture notes from a July 1951 MIT course, 65; F. Rohrlich and R. Gluckstern "Forward scattering of light by a Coulomb field," *Physical Review* 86 (1952): 1–9, on 2; A. Lenard, "Inner Bremsstrahlung in μ-meson decay," *Physical Review* 90 (1953): 968–973, on 971. Middle row, left to right: M. Gell-Mann and F. Low, "Bound states in quantum field theory," *Physical Review* 84 (1951): 350–354, on 352; F. Low, "Natural line shape," *Physical Review* 88 (1952): 53–57, on 55; A. Salam, "Renormalized S-matrix for scalar electrodynamics," *Physical Review* 86 (1952): 731–744, on 735. Bottom row, left to right: J. Steinberger, "On the use of subtraction fields and the lifetimes of some types of meson decay," *Physical Review* 76 (1949): 1180–1186, on 1182; N. M. Kroll and M. A. Ruderman, "A theorem on photomeson production near threshold and the suppression of pairs in pseudoscalar meson theory," *Physical Review* 93 (1954): 233–238, on 235.

Some order may be brought to the random-looking display of figure 2.4 by considering more closely the *pedagogical* links between young instructors and their students. In each local setting, young physicists adapted the diagrams to better bring out aspects deemed most important for the new kinds of calculations. Thus the diagrams drawn by young Cornell physicists began to look different—and to be used in subtly distinct ways—from those drawn by their peers at Rochester, Columbia, Urbana, and so on. (With practice, one can actually "predict" where a physicist was trained based on the kinds of diagrams he or she drew and the kinds of calculations in which the diagrams were enrolled.) Consider the examples in figure 2.5, taken from *Physical Review* articles during the period 1949–1954: in each pair, the diagram on the left comes from an advisor in one of the major training centers, and the example on the right from someone

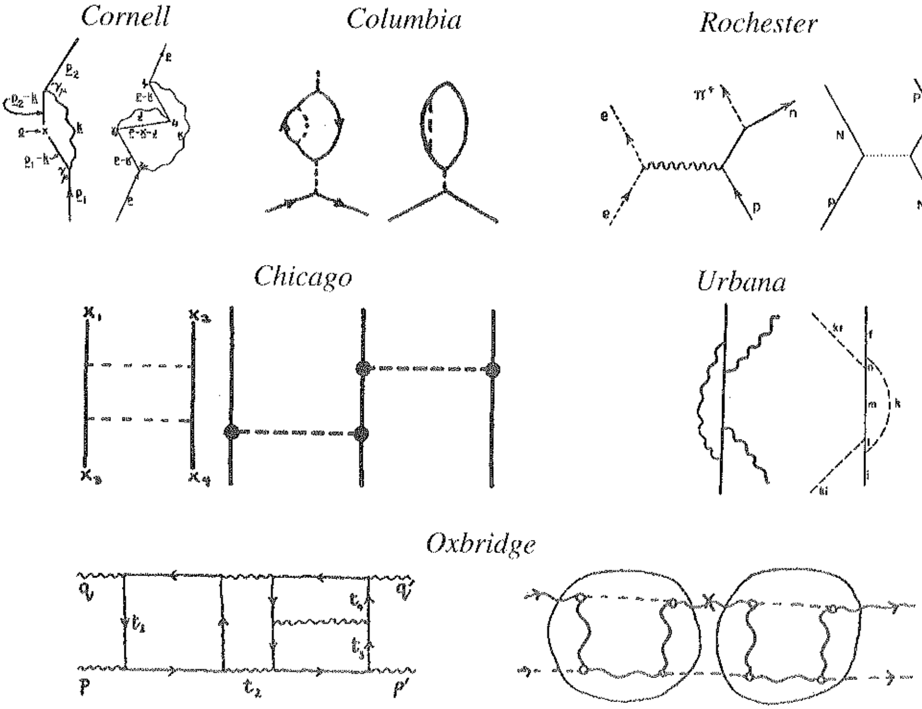


Figure 2.5
"Family Resemblances": mentors and students crafted diagrams for new purposes. Note the differences in which items were labeled, which lines received arrows, which lines were inclined at an angle, and so on. These pictorial differences were intimately tied to the different calculational roles the diagrams were meant to perform, and to more subtle differences in how the diagrams were interpreted. (Sources listed in note 30.)

he trained.[30] The pictorial similarity between advisors' and students' diagrams provides a second hint that something pedagogical was going on: students were clearly learning something from their supervisors above and beyond an abstract notion of what a Feynman diagram is or for what it should be used. They were practicing how to *apply* the semi-standard techniques to specific research questions—research questions that varied from place to place. Among the many distinct schools that emerged, consider two examples: young physicists at Columbia University and at Rochester University.

Columbia: Kroll's Perturbative Bookkeepers

The Columbia students learned of the new diagrams from Norman Kroll, who took up a teaching position in the department in 1950 directly upon completing his postdoctoral training at the Institute. Kroll had been one of the earliest "converts" to Dyson's diagrammatic program; his famous article with Robert Karplus on fourth-order corrections to an electron's magnetic moment, submitted to the *Physical Review* from the Institute in October 1949, had been heralded immediately as a triumph both for Feynman diagrams and for QED. Karplus and Kroll followed Dyson's prescriptions to the letter, introducing the five distinct classes of Feynman diagrams involved in their unprecedented calculation. (See figure 2.6.) Note, for example, their careful application of Dyson's antiparticle arrows on the electron and positron lines. As the postdocs made explicit, these arrows carried mathematical bite: precisely because the arrows in the triangles of the two diagrams of class V ran in opposite directions, the contributions from these distinct diagrams cancelled exactly. What Feynman had at first overlooked in his energetic doodling, these two disciples of Dyson could discern with tiny arrows and clarify with a single sentence. Next came the laborious job of evaluating the integrals associated with each of the remaining diagrams—a painstaking calculation rendered feasible by using Feynman's diagrams in the strict manner that Dyson had specified.[31]

In Karplus and Kroll's hands, then, Feynman diagrams had been disciplined into trusty bookkeepers for perturbative calculations. After "much helpful discussion with F. J. Dyson," they had drawn the diagrams carefully to Dyson's specifications, and applied his methodical rules to translate each dashed and solid line uniquely into its corresponding mathematical term.[32] When the dust settled and each of these integrals had been evaluated and added together, Karplus and Kroll had demonstrated that an electron in an external electromagnetic field will behave as if it had a magnetic moment of 1.001147 (in appropriate units) instead of 1, as Dirac's equation would have it. More than this, they had shown how the second-order result, 1.001162, first calculated without diagrams by Schwinger and re-calculated diagrammatically by both Feynman and Dyson, was further modified by the fourth-order correction terms to yield their answer.

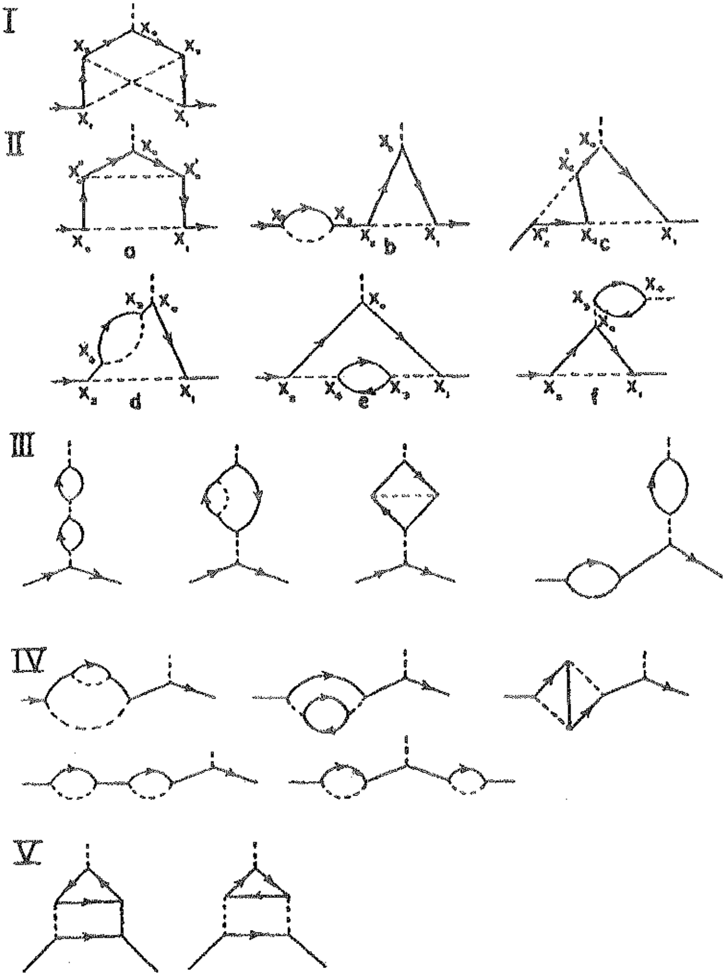


Figure 2.6
Fourth-order diagrams for the scattering of an electron in an external field. Source: R. Karplus and N. M. Kroll, "Fourth-order contributions in quantum electrodynamics and the magnetic moment of the electron," *Physical Review* 77 (1950): 536–549, on 537.

They had demonstrated, with this concrete example, how to put the diagrams to work for tracking the ever-tinier wisps of QED's correction terms.

Upon arriving at Columbia to begin teaching in the autumn of 1949, Kroll carried these hard-won skills with him, and soon a series of his own graduate students and postdocs submitted diagrammatic articles to the *Physical Review*. Their calculations followed Kroll's example closely, employing Dyson's rules to set out and methodically evaluate the fourth-order corrections to other important quantities within QED.[33] No one had been able to produce, let alone evaluate, each of the distinct, competing contributions that enter into a fourth-order perturbative QED calculation before Karplus and Kroll. Yet by treating the diagrams as handy mnemonic devices, as Dyson had taught them, the two postdocs—and soon Kroll's own students—could draw the five distinct classes of diagrams without confusing or conflating the various terms. Moreover, with the distinct diagrams written down, it became almost trivial to translate each of these, step by step, into its own integral expression.

The efficiency of calculating with Feynman diagrams in this manner was undeniable. Surely the rapidity and ease with which such labyrinthine correction terms could be clarified and evaluated would have convinced great numbers of theorists to pick up the diagrams and march along their own perturbative calculations. And surely this, in turn, would explain the diagrams' rapid dispersion to theorists throughout the world. And yet this simply wasn't so. Only a handful of other physicists followed Karplus and Kroll's famous perturbative calculation with similar ones, trotting out the diagrams as bookkeepers for the ever-smaller correction terms within Dyson's power-series expansion. In fact, fewer than twenty percent of the diagrammatic articles within the *Physical Review* between 1949 and 1954 made use of Feynman diagrams in this manner. Nearly all of these papers, in turn, were contributed by graduate students at Cornell (working with Feynman, Dyson, and/or Bethe) and by Kroll's students at Columbia.

Rochester: Marshak's Meson Markers

Young physicists elsewhere rarely used the diagrams for perturbative calculations within QED. What captured most theorists' attention soon after the war was not electron physics, but rather the embarrassment of riches suddenly pouring forth from the new accelerators. A flood of new particles, similar to but in many ways distinct from the familiar electrons and photons, surprised physicists when they began to probe high-energy interactions with the aid of accelerators, rather than relying only upon cosmic rays. As quickly became clear, the new particles—dubbed "mesotrons" or "mesons," since the masses of many of them were intermediate between electrons and protons—interacted with each other differently than electrons and photons did. Most important, most of these new particles interacted *strongly*, with coupling constants g^2 between 7 and 57, unlike the weak electrodynamic interaction, governed by the electron's charge of $e^2 \sim 1/137$. If theorists tried to treat interactions among, for example, pions and protons in the same way as they treated electron-photon scattering, with a long series of more and more complicated Feynman diagrams, each containing more and more vertices, then each higher-order diagram would include extra factors of the large number g^2. In contrast with the situation in QED, then, these complicated diagrams, with many vertices and hence many factors of g^2, would overwhelm the lowest-order, more basic contributions. Perturbative approaches seemed impossible within meson physics.

If there could be no reliable perturbative expansions for mesonic calculations, then what place could there be for Feynman's diagrams, which had been introduced for the sole purpose of simplifying perturbative calculations? As it turned out, there would be plenty of room for them. In fact, more than half of the diagrammatic articles published in the *Physical Review* between 1949 and 1954 applied Feynman diagrams in one way or another to problems in meson physics, including the first four diagram-filled papers published after Dyson's and Feynman's own. One of the early groups to bring Feynman diagrams to bear on meson physics was Robert Marshak's group at the University of Rochester. Rochester was in the process of building its own cyclotron, and Marshak set his team to work preparing to be useful once their colleagues had the new machine up and running.[34]

Marshak had attended Feynman's original introduction of the diagrams at the Pocono meeting, and his group benefited from frequent visits by Feynman over the next two years. In addition, one of Marshak's younger collaborators, Julius Ashkin, was a close associate of Feynman's whom Feynman thanked several times in his articles about the diagrams.[35] Whereas Kroll and his Columbia students worked with the diagrams in Dysonian fashion, Marshak's group at Rochester learned to use the diagrams much more as Feynman himself did. They learned directly from Feynman that his line drawings could provide "intuitive" help far beyond the narrow dictates of perturbative QED.

In the spring of 1950, Marshak explained some of the differences between QED and meson physics to his students. Whereas theorists had long ago narrowed the range of options for the basic interaction between electrons and photons down to one in the case of QED, the options remained frustratingly open in the mesonic realm. For one thing, the pions' characteristics were still unclear: Were they spin-0, or spin-1? Under parity transformations were they even, or odd? The way in which they interacted with protons and neutrons remained equally unclear: Did they couple to the nucleons via scalar, pseudoscalar, vector, pseudovector, tensor, or pseudotensor interactions? The various possibilities, Marshak lectured, led to eight distinct choices for the basic interaction, each of which was still in the running for describing the nuclear domain.[36]

Awash in this sea of open-ended possibilities, and unable to calculate anything beyond the lowest-order in meson models because of the large size of g^2, Marshak and his students thus had different goals in mind when they picked up Feynman diagrams and began to calculate. They wanted to fashion a useful means of distinguishing the eight different possibilities for the meson-nucleon interaction. Marshak and his young team pursued this goal by comparing the lowest-order predictions of the eight contenders, scanning for qualitative differences between the various models' phenomenological predictions. Beginning in the spring of 1950, they fastened onto each model's theoretical predictions for the angular distribution of decay products—that is, how likely it would be for pions to be detected at various angles or directions as they careened away from the interaction region. The predicted angular distributions, Marshak's team soon realized, depended sharply upon the symmetry properties of the mesons and their interactions—symmetry properties that were encoded in the various interaction terms, and hence reflected in calculations that included only the simplest Feynman diagrams for a given process. In his first diagrammatic article, Marshak, together with one of his graduate students and Feynman's friend Ashkin, explained that working only with the lowest-order Feynman diagrams "is extremely crude, but it is thought that the qualitative features will persist in a more correct theory." Never dreaming of pushing any given calculation beyond g^2, they would nonetheless try to bring order to the nuclear realm.[37]

In the process, Marshak and his young collaborators refashioned the diagrams, designing them for better use toward their own goal—all within a year of Feynman's and Dyson's original articles on the diagrams. The Rochester group used the diagrams as illustrations of basic processes; then they could run down their list of eight contenders for the mesonic interaction and calculate each model's lowest-order contribution to the same physical process. Only five pages into his 1952 textbook on the new techniques (culled primarily from his 1950 lectures), Marshak's students found a full page of examples of these newly refurbished Feynman diagrams. (See figure 2.7.) Calculating a particular lowest-order contribution—for instance, the prediction for photon-proton scattering coming from the pseudoscalar interaction term—in itself was no trouble. Theorists had written down the analogous terms for QED for years without the aid of Feynman diagrams. The complicated task in Marshak's meson physics was navigating the maze of competing versions of each lowest-order calculation; Marshak and his students had to distinguish the different theoretical predictions for a given process. By doodling the lowest-order Feynman diagrams and labeling carefully the

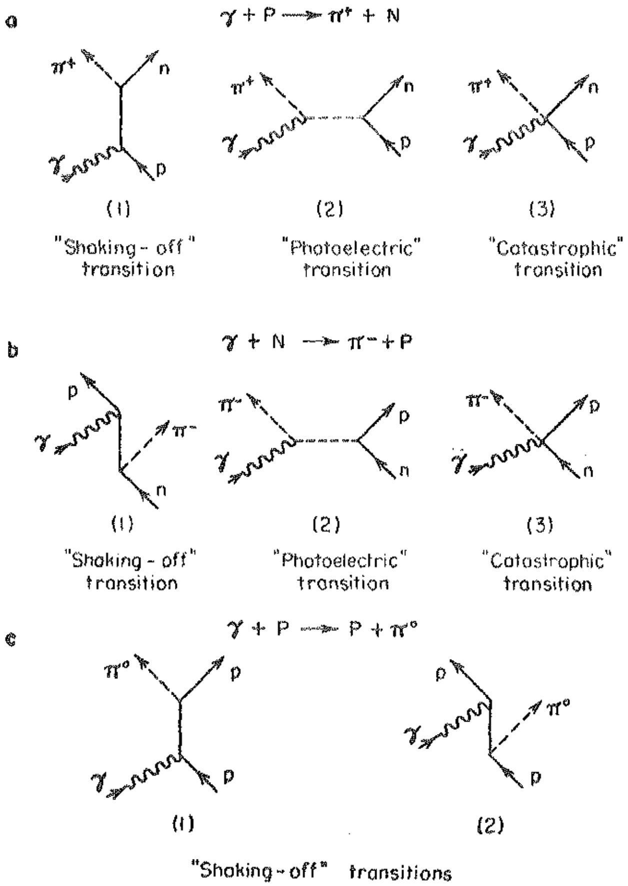


Figure 2.7

Feynman diagrams for the photoproduction of pions. Source: Robert Marshak, *Meson Physics* (McGraw-Hill, 1952), 6.

specific particles involved in each specific process, they could march through the contributions in turn, calculating and re-calculating each model's predictions.[38]

As Marshak and his students unglued Feynman diagrams from their stipulated Dysonian rules, the diagrams they drew thus changed as well. Features of the diagrams that had carried specific meanings when considered under their QED rubric could now be discarded without being missed. Particle labels replaced the coordinate-space or momentum-space labels that had been so important for cranking through perturbative calculations within QED. In Marshak's hands, moreover, upward-directed arrows appeared on all external lines—in other words, he did not make use of Dyson's antiparticle arrow convention for distinguishing particles from antiparticles. In the context of Kroll's perturbative-bookkeeping calculations, these antiparticle arrows had proven essential for correctly distinguishing between distinct closed-loop diagrams; to Kroll and his students, the tiny arrows carried specific meaning. In the Rochester meson work, by contrast, no one was calculating any closed-loop contributions, so there was hardly any need to distinguish between different kinds of closed loops; the antiparticle arrows meant nothing. In only a few months, the antiparticle arrow convention had become a difference that no longer made any difference. The diagrams' pictorial forms, calculational roles, and interpretations thus became intertwined. All three shifted as young theorists deployed the diagrams toward different ends.

The Rochester and Columbia approaches were clearly different from each other. How easy was it for members of one group—say, graduate students working with either Kroll or Marshak—to understand what the other group was doing? Some physicists at the time spoke as if it were impossible for members of such different "schools" to understand one another—thereby reaching a similar conclusion to that of sociologists such as Harry Collins, in his work on tacit knowledge and skills-transfer. Consider, for example, Wolfgang Pauli's amusing formulation, in a letter written in 1954, of how one could define various schools within theoretical physics:

> What is the definition of the 'Wigner School'? The question can be answered in the practical American way by an *operational* definition. In order to decide, whether or not a person is a member of the Wignerschool [*sic*], you give him a paper of Schwinger. . . . If he then says, that this is very obscure, and that he can't understand it, he is a member of the Wigner school. But he, who says that it is quite well understandable and clear will be excluded from the Wigner school. . . . Similar tests are certainly very popular at Harvard where the test object for membership of the *Schwinger* school is a paper of *Wigner*.[39]

Thus Pauli tied the social question of young physicists' training under senior colleagues directly to the epistemic question of understanding various calculational approaches.

But such ties were not always so tight in practice. In 1950, Ashkin, Marshak, and Marshak's graduate student Albert Simon published a long article in the Japanese journal *Progress of Theoretical Physics* in which they went through the motions of a fourth-order perturbative calculation within a specific meson model. With much help and personal coaching from Dyson, whom they thanked for "a lucid presentation of his method," Marshak and his group could perform calculations more like those of Kroll and the Columbia contingent.[40] They worked in terms of the parameter g^2, neglecting the fact that $g^2 >> 1$, in order to demonstrate that Dyson's systematic renormalization program for QED could also fix certain types of calculations among mesons and nucleons (even if the swollen size of g^2 prevented the Rochester physicists from comparing their calculations with quantitative experimental data). This appeared to be a one-time exercise: neither Marshak nor his students pursued mesonic calculations beyond the lowest order after this early paper. Students other than Simon appear not to have sweated such details; at least, no traces of such types of calculations were left in their dissertations or in their published articles. In Marshak's textbook, published in 1952, he made it clear that students were expected to have a background in non-relativistic quantum mechanics but need not ever have studied QED, let alone Dyson's diagrammatic renormalization program.[41]

The differences between Marshak's and Kroll's pedagogical programs thus point to local choices about what to work on and what to drill one's students in, rather than wholly incommensurable or incommunicable epistemic regimes. Both groups (and many others in between) made Feynman diagrams central to their calculations. Students in Rochester, just as much as those at Columbia, spent hours practicing how to draw the diagrams and put them to work in certain kinds of calculations, even though the diagrams they drew and the calculations for which they became central were clearly different from each other. Different research programs called for different pedagogical patterns. Students' Feynman diagrams bore the marks of their distinct training.

Why Did They Stick? Global Aspects

Marshak's energetic doodling, in which he and his students loosened Feynman diagrams from Dyson's systematic rules for their use, was among the earliest examples of the diagrams' dispersion. Over the course of the 1950s, many more theorists, especially in the United States, grew ever more frustrated with their inability to use quantum field theory to get any phenomenological handle on the "zoo" of new strongly interacting particles. By the late 1950s and on into the 1960s, a few groups, most notably Geoffrey Chew's group in Berkeley, declared that quantum field theory itself was dead, at least for nuclear and high-energy physics. Chew announced, with increasing gusto, that Dyson's careful QED apparatus—Lagrangians, interaction Hamiltonians, perturbative series, even the exchange of virtual particles as carriers of force—was less than useless for studying what had come to dominate the attention of most high-energy theorists (not to mention experimentalists).[42]

One might expect, given that Feynman diagrams had been designed for perturbative calculations within weakly coupled theories like electrodynamics, that the alleged fall of that entire theoretical program would bring Feynman diagrams down with it. Instead, just the opposite happened: Chew and his students and postdocs extracted the diagrams from their original field-theory embedding and used them as a heuristic scaffolding for building what they hoped would become a rival theory. A decade after Marshak began tinkering with the diagrams for making meson calculations, Chew and company announced a clean break between the diagrams and their so-called derivation.

Why did the diagrams stick, even as many theorists tugged so doggedly at (and eventually discarded) their original theoretical embedding? The diagrams' curious tenacity points to a different pedagogical feature in the making of young theorists in the middle of the twentieth century: associations shared by Feynman diagrams and another, more widely adopted, diagrammatic practice.[43] In short, physicists during the late 1940s and throughout the 1950s often associated Feynman's diagrams with Minkowski's spacetime diagrams used for special relativity, which most physicists had practiced drawing and analyzing from their earliest studies in physics. Clearly the two types of diagrams were distinct—physicists at the time rarely proclaimed them to be conceptually equivalent, and some even argued out loud that the two types of diagrams should not be confused. All the same, most physicists continued to draw their Feynman diagrams according to certain specific learned pictorial conventions. Their occasional verbal denials to the contrary, the visual features of their Feynman diagrams betray a distinct pedagogical legacy.

As physics students around the world routinely learned by the late 1940s, Minkowski diagrams could be used for charting objects' propagation through space and time. They learned about Minkowski diagrams in the context of special relativity.[44] In the already-standardized diagrams, students were taught to place the time axis vertically and one spatial axis horizontally. (See figure 2.8.) In addition, students learned to scale the speed of light to 1, so that light would travel along 45° diagonals within their Minkowski diagrams.

Feynman consistently introduced his own diagrams only after including explicit Minkowski diagrams—in his 1949 articles introducing the diagrams, in his unpublished lecture notes from Cornell and Caltech, and in his later popular book on QED.[45] In Feynman's many presentations, Feynman diagrams came laden with talk of "world-lines," the order of "events" along particles' "trajectories," and "spacetime pictures"—all clear markers of "Minkowski-talk." This language—and, more important, these visual features—continued even after Feynman moved from coordinate space to momentum space. Still his diagrams displayed particles moving along tilted trajectories, with photon lines often moving at or near 45°—even though in momentum space, 45° diagonals bore no relation whatsoever with light-like travel. (Compare figure 2.1 with figure 2.9.)

The tacit, visual assimilability between Feynman's doodles and Minkowski diagrams had stirred Niels Bohr, that elder statesman of quantum physics, to object vigorously at

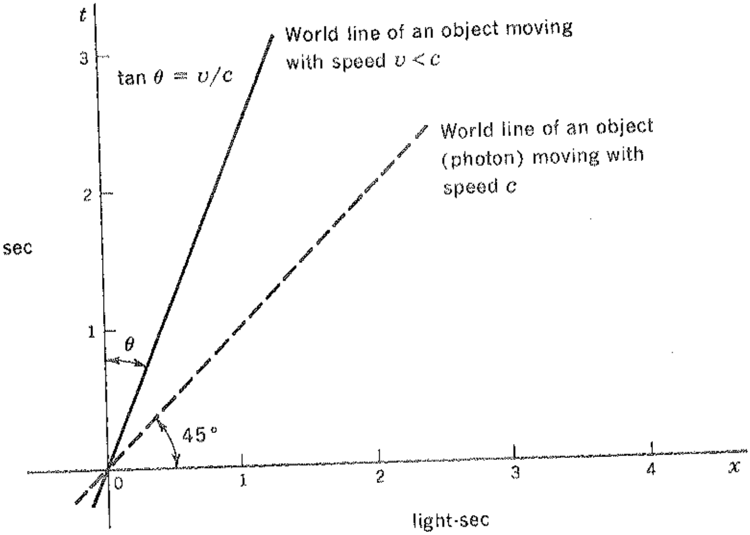


Figure 2.8
A Minkowski diagram. Source: N. David Mermin, *Space and Time in Special Relativity* (McGraw-Hill, 1968), 160.

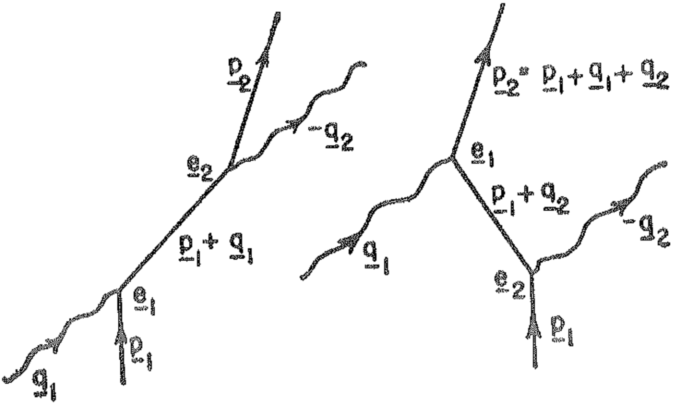


Figure 2.9
Feynman diagrams in momentum space, drawn with Minkowski-diagram conventions. Source: Richard Feynman, "Space-time approach to quantum electrodynamics," *Physical Review* 76 (1949): 769–789, on 775.

Feynman's original presentation at the 1948 Pocono meeting. Spacetime trajectories were one thing for macroscopic objects, Bohr reminded Feynman, but were ruled out of court for quantum mechanics: Heisenberg's uncertainty principle denied the possibility of knowing a quantum object's simultaneous position and momentum, the two ingredients needed to construct something like a spacetime trajectory. Feynman's flustered reply—that his diagrams were not meant to be read literally as spacetime trajectories, but rather as a convenient shorthand notation—had convinced few in the room.[46]

Despite Bohr's objections, Feynman's pictorial scheme caught on. As a small sampling of the enormous repetition of these pictorial conventions, consider the diagrams shown here in figure 2.10, all of which were published in the *Physical Review* between 1949 and 1954. The earliest diagrammatic textbooks, published in the mid 1950s, likewise used Minkowski-diagram imagery when introducing Feynman diagrams, sometimes even including explicit space and time axes as Feynman had done. (See figure 2.11.) These associations did not always remain tacit. Sometimes textbook authors actually tried to explain away (in lengthy footnotes) any conceptual links between the two types of diagrams, even as they continued to draw Feynman diagrams in this "stylized"

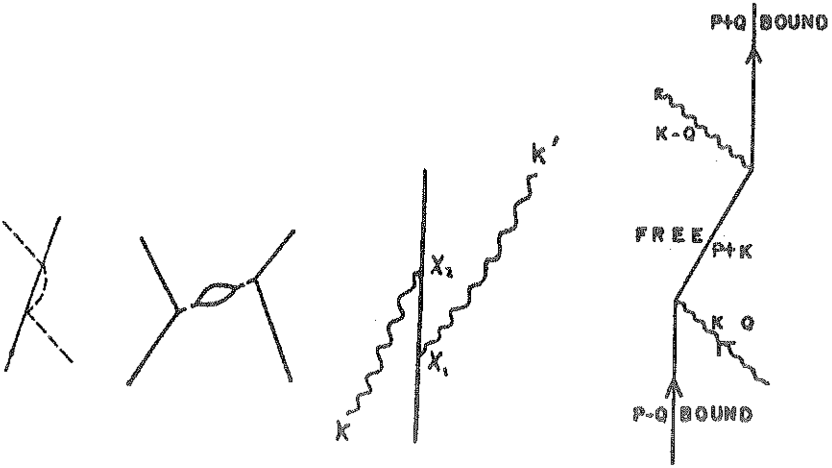


Figure 2.10
Repetition of Minkowski conventions in Feynman diagrams. Sources, left to right: F. Rohrlich, "Quantum electrodynamics of charged particles without spin," *Physical Review* 80 (1950): 666–687, on 671; J. L. Anderson, "Green's functions in quantum electrodynamics," *Physical Review* 94 (1954): 703–711, on 706; Francis Low, "Natural line shape," *Physical Review* 88 (1952): 53–57, on 54; and J. S. Levinger, "Small angle coherent scattering of gammas by bound electrons," *Physical Review* 87 (1952): 656–662, on 661.

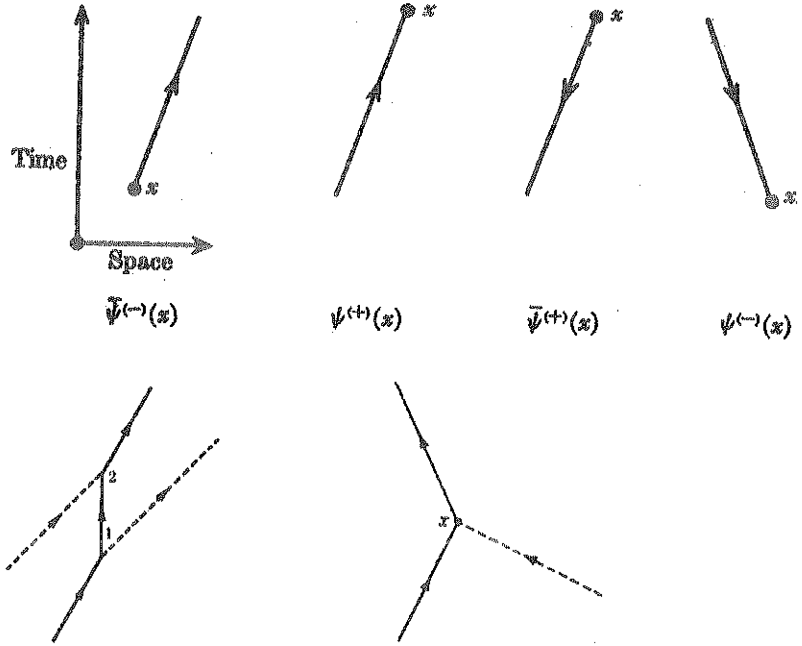


Figure 2.11
Feynman diagrams in early textbooks. Sources: (top) S. Schweber, H. Bethe, and F. de Hoffmann, *Mesons and Fields* (Row and Peterson, 1955), volume 1, 219; (bottom, left to right) J. Jauch and F. Rohrlich, *Theory of Photons and Electrons* (Addison-Wesley, 1955), 150; F. Mandl, *Introduction to Quantum Field Theory* (Interscience, 1959), 73.

way. Still other physicists trumpeted the associations as an especially good method for introducing Feynman diagrams to beginning physics students or to audiences of nonphysicists.[47]

Thus, despite Feynman's denials to Niels Bohr that his diagrams were intended literally to picture particles' physical paths, they were consistently drawn *and taught* as being of a piece with the reigning pictorial standards for studying particle trajectories through space and time. The mnemonic device simply was not "innocent" of physicists' prior inculcation in the visual practice of depicting particle paths, regardless of the distinct meanings attributed in different contexts to the stick figures. This long, geographically dispersed pedagogical tradition carried tacit, visual "baggage" far more general than the specific functions Feynman diagrams had been designed to play in the context of perturbative QED calculations. Minkowski diagrams had already become second nature to young physicists in many parts of the world. In a broad, general way, they helped to pattern how theorists and their students set up problems and carved out solutions. Within this broader pedagogical space, physicists could improvise and tinker with Feynman diagrams without being unduly constrained by Feynman's, Dyson's, or anyone else's diagrammatic rules of the game.

Conclusions: Three Pedagogical Functions amid the Dispersion

Robert Kohler's recent work on the history of genetics during the early decades of the twentieth century provides a telling example of how intertwined crafting research practices and creating scientific communities can become. As Kohler demonstrates, *Drosophila melanogaster* was never a research tool outside of a specific community of "drosophilists," and a specific set of social, political, and economic ties that these researchers forged and shared. In the case of fruit-fly genetics, it took a lot of work to domesticate a particular variation of the fly into a useful and interpretable tool. At the same time, it took a lot of work by these same drosophilists to domesticate the nascent community of fruit-fly investigators to share their stocks of mutant-fly varieties, communicate their findings, and regulate intellectual-property claims.[48] In the case of Feynman diagrams in postwar physics, the need to craft both the tool and its user was even more extreme, since the diagrams were never anything more than paper-and-pencil representations—representations that could therefore do absolutely nothing without an interpreter. In the drosophilists' case, at least the bugs were in some sense "out there," even if the artificially stabilized basis of study, *Drosophila melanogaster,* was not an independently existing tool simply found in nature. We are much harder pressed to point to any particular example that would be able to stand in, on its own, for the myriad ways in which theorists drew, calculated with, and interpreted Feynman diagrams.

Three lessons about pedagogy emerge from studying Feynman diagrams' dispersion during the middle decades of the twentieth century. The great majority of physicists who used the diagrams did so only after working closely with a member of the diagrammatic network: the diagrams were put into circulation largely by means of the postdoc cascade. Postdocs in theoretical physics circulated through the Institute for Advanced Study, participating in intense study sessions and collaborative calculations while there. Then they took jobs throughout the United States (and elsewhere) and drilled their own students in how to use the diagrams. Something like tacit knowledge seemed to be crucially important: for the most part, physicists outside of this rapidly expanding network did not pick up the diagrams for their research. It is therefore no accident that more than 80 percent of the physicists who first used Feynman diagrams in print (in the *Physical Review*) between 1949 and 1954 did so as either graduate students or postdocs. Personal contact and individual mentoring remained the diagrams' predominant means of circulation even years after explicit instructions for their use had been in print. Face-to-face mentoring, rather than the circulation of texts, provided the most robust means of inculcating skill with the new diagrams in expanding groups of users. In fact, the homework assignments these postdocs assigned to their students often stipulated little more than to draw the correct Feynman diagram for a given problem, not even to translate the diagrams into mathematical expressions.[49] These students learned early, in ways rarely shared by students outside this early network of dispersed postdocs, that calculations would now begin with Feynman diagrams.

When it comes to physicists' actual uses of the diagrams, however, the simple cascade model must be augmented. Here we see evidence of the second meaning of "dispersion": pictorially and calculationally, physicists' appropriations of the diagrams showed greater and greater differentiation. Local traditions emerged, within which "family resemblances" can be found. Young physicists at Cornell, Columbia, Rochester, Berkeley, and elsewhere practiced using the diagrams in distinct ways, toward distinct ends. These diagrammatic appropriations bore less and less resemblance to Dyson's original packaging for the diagrams. His first-principles derivation and his set of one-to-one translation rules guided students at Columbia, for example, but were deemed less salient for students at Rochester, and were all but dismissed by Geoffrey Chew's students at Berkeley. Students' mentors made choices about what to work on and what to train their students to do. Thus, as with any tool, we can understand physicists' uses of Feynman diagrams only by considering their local contexts of use.

Why did physicists continue to use the diagrams, often basing entire calculational and pedagogical programs on them, even as the problem for which they were invented faded from view? In part because of visual links with other learned practices, such as Minkowski diagrams. These links were largely shared across local groups and national borders; thanks to decades of systematic training, they had become second nature to generations of physicists long before Feynman began doodling his new diagrams. Thus, whereas much of the diagrams' pedagogical dispersion highlighted local groups and face-to-face communication, at least some of the diagrams' dispersion and staying power must be understood in terms of more broadly shared pedagogical resources.

Thus, in considering the diagrams' dispersion, it remains impossible to separate the research practices from how various scientific practitioners were trained. Within a generation, Feynman diagrams became the "theoretical technology" that undergirded calculations in everything from electrodynamics to nuclear and particle physics to solid-state physics. This was accomplished through much pedagogical work, postdoc-to-postdoc, mentor-to-disciples, and department-by-department. Feynman diagrams do not occur in nature, and theoretical physicists are not born, they are made. During the decades after World War II, the practices and practitioners were forged as part of the same pedagogical process.

Notes

1. Research during the past decade has begun to challenge these older assumptions. See esp. Kathryn Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics* (Cornell University Press, 1991); Martin Krieger, *Doing Physics: How Physicists Take Hold of the World* (Indiana University Press, 1992); Andrew Pickering, *The Mangle of Practice: Time, Agency, and Science* (University of Chicago Press, 1995), chapter 4; *The Cultures of Theory*, ed. Peter Galison and Andrew Warwick, published as *Studies in History and Philosophy of Modern Physics* 29 (1998): 287–434; Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (University of Chicago Press, 2003). See also Ursula Klein, "Techniques of modelling and paper-tools in classical chemistry," in *Models as Mediators*, ed. Mary Morgan and Margaret Morrison (Cambridge University Press, 1999), 146–167; idem, "Paper tools in experimental cultures," *Studies in History and Philosophy of Science* 32 (2001): 265–302. Cf. Eric Livingston, *The Ethnomethodological Foundations of Mathematics* (Routledge & Kegan Paul, 1986).

2. This essay is based on my book *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (University of Chicago Press, 2005).

3. *Oxford English Dictionary*, ed. J. A. Simpson and E. S. C. Weiner, second edition (Oxford University Press, 1989), s.v. "disperse." For historiographical, rather than etymological, clarity I have combined the *OED*'s definitions 4a and 4b for the first meaning of "disperse" quoted here, and combined definitions 1a and 2b for the second meaning of "disperse." See also Kathleen Jordan and Michael Lynch, "The sociology of a genetic engineering technique: Ritual and rationality in the performance of the 'plasmid prep,'" in *The Right Tools for the Job: At Work in Twentieth-Century Life Sciences*, ed. Adele Clarke and Joan Fujimura (Princeton University Press, 1992), 77–114.

4. Richard Feynman, "Space-time approach to quantum electrodynamics," *Physical Review* 76 (1949): 769–789. For more on Feynman's own route to his diagrams, see Feynman, "The development of the space-time picture of quantum field theory," *Science* 153 (1966): 699–708; Silvan Schweber, "Feynman and the visualization of space-time processes," *Reviews of Modern Physics* 58 (1986): 449–508; Jagdish Mehra, *The Beat of a Different Drum: The Life and Science of Richard Feynman* (Oxford University Press, 1994), chapters 5, 6, 10–14; Silvan Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton University Press, 1994), chapter 8; Peter Galison, "Feynman's war: Modelling weapons, modelling nature," *Studies in History and Philosophy of Modern Physics* 29 (1998): 391–434.

5. Feynman, "Space-time approach," 771–773. The absolute square of this integral yielded an approximation, to lowest-order in e, for the probability that two incoming electrons will scatter into two outgoing electrons. Actually, this is one-half of the lowest-order contribution: because electrons are indistinguishable, Feynman next explained that one must include a similar amplitude which would describe the case in which the incoming electron on the left ended up, after

scattering, as the outgoing electron on the right, and vice versa. Feynman had already fixed upon these features of his approach before his presentation at the Pocono meeting, though other elements of this 1949 article included developments which Feynman only worked out after the meeting. See Schweber *QED,* chapter 8; Mehra, *Beat of a Different Drum,* chapters 12 and 13, for further details on the evolution of Feynman's work between 1947 and 1949.

6. Feynman, "Space-time approach"; Schweber, *QED,* chapter 8.

7. Schweber, *QED,* 318–334, 436–445; Abraham Pais, *Inward Bound: Of Matter and Forces in the Physical World* (Oxford University Press, 1986), 458–459; Robert Crease and Charles Mann, *The Second Creation: Makers of the Revolution in Twentieth-Century Physics* (Macmillan, 1986), 137–138; Gleick, *Genius,* 255–261. For more on Schwinger's approach, see esp. Schweber, *QED,* chapter 7.

8. Mehra, *Beat of a Different Drum,* 262–263.

9. L. L. Foldy and R. E. Marshak, "Production of π mesons in nucleon-nucleon collisions," *Physical Review* 75 (1949): 1493–1499, on 1493.

10. Paul Matthews to Wolfgang Pauli, February 25, 1950, in Wolfgang Pauli, *Wissenschaftlicher Briefwechsel,* ed. Karl von Meyenn, volume 4, part I (Springer, 1996), 27–33, on 27. Matthews, a graduate student at Cambridge University, had been corresponding with Fritz Rohrlich and David Feldman, both postdocs at the time, as well as with Pauli. See also the other coresopndence between Matthews, Pauli, and Rohrlich from spring 1950, in ibid., 4–5, 46–47, 72–73, and 97–99.

11. Wolfgang Pauli to Gunnar Källén, August 19, 1952, in Pauli, *Wissenschaftlicher Briefwechsel,* volume 4, part I, 708; my translation. The dissertation in question was by C. A. Hurst, from Cambridge University.

12. Leonard Schiff to Edward Teller, May 26, 1953, in folder "Schiff, Leonard Isaac, 1915–1971," Raymond Thayer Birge, *Correspondence and Papers,* call number 73/79c, Bancroft Library, Berkeley, California.

13. Kaiser, *Drawing Theories Apart,* chapter 2. Two diagrammatic textbooks appeared in 1953 in Japanese and Russian, but these were not translated into English until 1956 and 1957, respectively, and had no impact on American physicists ca. 1949–1954.

14. Julian Schwinger, "Renormalization theory of quantum electrodynamics: An individual view," in *The Birth of Particle Physics,* ed. Laurie Brown and Lillian Hoddeson (Cambridge University Press, 1983), 329–353, on 343, 347. See also *Julian Schwinger: The Physicist, The Teacher, and the Man,* ed. Y. Jack Ng (Singapore: World Scientific, 1996). Although Schwinger's graduate students did not study Feynman diagrams in their formal coursework, some did learn about the diagrams by other means: see Kaiser, *Drawing Theories Apart,* chapter 3.

15. Details may be found in chapter 2 of Kaiser, *Drawing Theories Apart.*

16. In typical fashion, Dyson's letters home to his family in England during and after the trip speak of a Homeric affair, as he and Feynman climbed intellectual heights even as they battled floods and closed roads. Feynman's later reminiscences of the trip, in contrast, centered around bawdy stories of staying overnight in a brothel because there was no vacancy in the local hotels.

Compare Dyson to his parents, June 5, 1948, reprinted on pp. 327–330 of Freeman Dyson, *From Eros to Gaia* (Pantheon, 1992), with pp. 65–66 of Richard Feynman with Ralph Leighton, *"What Do You Care What Other People Think?"* (Norton, 1988).

17. Freeman Dyson, correspondence with his family, 1947–1960, in Professor Dyson's possession, Institute for Advanced Study, Princeton, New Jersey. See also Freeman Dyson, *Disturbing the Universe* (Basic Books, 1979), 47–68; Schweber, *QED*, chapter 9.

18. F. J. Dyson, "The radiation theories of Tomonaga, Schwinger, and Feynman," *Physical Review* 75 (1949): 486–502; idem, "The *S* matrix in quantum electrodynamics," *Physical Review* 75 (1949): 1736–1755; *Science Citation Index* (Institute for Scientific Information, 1961–), s.v. "Feynman" and "Dyson."

19. In his published article, Feynman avowed unapologetically that "Since the result was easier to understand than the derivation, it was thought best to publish the results first in this paper," noting that "in the interest of keeping simple things simple the derivation will appear in a separate paper"—a "separate paper" that was only submitted a full thirteen months later (Feynman, "Space-time approach," 770). The later paper was published as Feynman, "Mathematical formulation of the quantum theory of electromagnetic interaction," *Physical Review* 80 (1950): 440–457. Years later, Dyson recalled: "Nobody but Dick [Feynman] could use his theory, because he was always invoking his intuition to make up the rules of the game as he went along. Until the rules were codified and made mathematically precise, I could not call it a theory." (Dyson, *Disturbing the Universe*, 62)

20. During Dyson's and Cécile Morette's visit with Feynman at Cornell in October 1948, Feynman had calculated the probability for the scattering of light by an external electromagnetic potential, a result that had eluded physicists for nearly a decade. Yet Feynman wrote to Dyson the following week, explaining that in fact the effect vanished (at least at lowest-order in the electron's charge), because the contributions from two different diagrams exactly cancelled out: in one diagram, the virtual electrons circled around the closed loop in one direction, while in the second diagram, they circled in the opposite direction. Only if one clearly distinguished between these two separate diagrams could one thereby reproduce Furry's theorem. See Schweber, *QED*, 450. On Furry's theorem, see W. Furry, "A symmetry theorem in the positron theory," *Physical Review* 51 (1937): 125–129; Pais, *Inward Bound*, 381.

21. Dyson to J. Robert Oppenheimer, October 17, 1948, in Dyson papers; emphasis added. See also Dyson, *Disturbing the Universe*, 72–74; Schweber, *QED*, 520–527.

22. Rabi's famous quotation is reprinted in John Rigden, *Rabi: Scientist and Citizen* (Basic Books, 1987), 46. Theorists' need to travel to Europe had begun to change by the mid to late 1930s, by which time theorists could pursue postdocs at a handful of American institutions with the aid of National Research Council fellowships, most notably with Oppenheimer at Berkeley, John Van Vleck at Wisconsin, or Eugene Wigner at Princeton. See Alexi Assmus, "The creation of postdoctoral fellowships and the siting of American scientific research," *Minerva* 31 (1993): 151–183.

23. Beatrice M. Stern, *A History of the Institute for Advanced Study, 1930–1950* (unpublished typescript, 1961), chapter 11; Ed Regis, *Who Got Einstein's Office? Eccentricity and Genius at the Institute for Advanced Study* (Perseus, 1987), 137–140.

24. J. Robert Oppenheimer to Wolfgang Pauli, February 20, 1952, in Pauli, *Wissenschaftlicher Briefwechsel*, volume 4, part I, 553–554.

25. Freeman Dyson, interview with David Kaiser, January 8, 2001, Princeton.

26. Robert Karplus and Norman Kroll, "Fourth-order corrections to quantum electrodynamics and the magnetic moment of the electron," *Physical Review* 77 (1950): 536–549, on 536–537.

27. Wolfgang Pauli to Abraham Pais, May 26, 1949, in Pauli, *Wissenschaftlicher Briefwechsel*, volume 4, part I, 655.

28. Kaiser, *Drawing Theories Apart*, chapter 3.

29. The diagrams spread to physicists working in other countries largely by similar means. See Kaiser, *Drawing Theories Apart*, chapter 4.

30. The examples from Cornell, Columbia, and Rochester involve advisors and their graduate students. The cases of Chicago and Urbana involve older theorists who learned about the diagrams from their colleagues. In the final example, from Oxford/Cambridge, the putative "advisee," John Ward, had already made use of Feynman diagrams for different types of analyses, and talked extensively with his colleague, Abdus Salam, about how to use the diagrams for the types of calculations Salam had been working on; the diagrams Ward began to draw shifted accordingly. Cornell: Feynman, "Space-time approach," 775; R. M. Frank, "The fourth-order contribution to the self-energy of the electron," *Physical Review* 83 (1951): 1189–1193, on 1190. Columbia: R. Karplus and N. M. Kroll, "Fourth-order corrections in quantum electrodynamics and the magnetic moment of the electron," *Physical Review* 77 (1950): 536–549, on 537; J. Weneser, R. Bersohn, and N. M. Kroll, "Fourth-order radiative corrections to atomic energy levels," *Physical Review* 91 (1953): 1257–1262, on 1258. Rochester: Robert Marshak, *Meson Physics* (McGraw-Hill, 1952), 39; A. Simon, "Bremsstrahlung in high energy nucleon-nucleon collisions," *Physical Review* 79 (1950): 573–576, on 574. Chicago: Gell-Mann and Low, "Bound states," 351; G. Wentzel, "Three-nucleon interactions in Yukawa theory," *Physical Review* 89 (1953): 684–688, on 684. Urbana: Low, "Natural line shape," 55; G. F. Chew, "Renormalization of meson theory with a fixed extended source," *Physical Review* 94 (1954): 1748–1754, on 1749. Oxford and Cambridge: A. Salam, "Overlapping divergences and the *S*-matrix," *Physical Review* 82 (1951): 217–227, on 223; J. C. Ward, "Renormalization theory of the interactions of nucleons, mesons, and photons," *Physical Review* 84 (1951): 897–901, on 899.

31. Actually, the postdocs next clarified that for their stated problem—calculating corrections to an electron's magnetic moment in the presence of an external field—only the diagrams in Classes I and II would contribute. The other diagrams contributed instead to different types of radiative corrections, such as the photon and electron self-energies.

32. One of these terms, in fact, proved simply too lengthy to write out in full. After setting up the integral with the aid of Dyson's version of Feynman's diagrams, the two postdocs quoted their result, explaining in a footnote, "The details of two independent calculations which were performed so as to provide some check of the final result are available from the authors." Karplus and Kroll, "Fourth-order," 548, n. 23. They were describing the integral associated with diagram I in figure 2.6.

33. See in particular Norman Kroll and Franklin Pollock, "Second-order radiative corrections to hyperfine structure," *Physical Review* 86 (1952): 876–888; Weneser, Bersohn, and Kroll, "Fourth-order radiative corrections."

34. See, e.g., J. Ashkin, T. Auerbach, and R. Marshak, "Note on a possible annihilation process for negative protons," *Physical Review* 79 (1950): 266–271, on 266. See also Robert Marshak, interview with Charles Weiner (1970), part II, 63, and part IV, 1–11. Call number OH308, Niels Bohr Library, American Institute of Physics, College Park, Maryland.

35. Ashkin was an assistant professor in Rochester. Feynman thanked him in the acknowledgements to both "Space-time approach" and "Mathematical formulation."

36. Marshak, *Meson Physics*, chapter 1. As explained on x, this textbook was based largely on lectures Marshak had given at Rochester during spring 1950.

37. Ashkin, Auerbach, and Marshak, "Possible annihilation process," 266–267.

38. This approach was adopted in Ashkin, Auerback, and Marshak, "Possible annihilation process," as well as in papers by Marshak's graduate students: Albert Simon, "Bremsstrahlung in high energy nucleon-nucleon collisions," *Physical Review* 79 (1950): 573–576; Morton Kaplon, "The contribution of the Pauli moment to π-meson production by photons," *Physical Review* 83 (1951): 712–715.

39. This addendum was apparently intended to be included in a letter to Léon Rosenfeld; it continued themes that Pauli and Rosenfeld had been discussing in recent correspondence. The quotation is reprinted in Pauli, *Wissenschaftlicher Briefwechsel*, ed. Karl von Meyenn, volume 4, part II (Springer, 1999), 956.

40. J. Ashkin, A. Simon, and R. Marshak, "On the scattering of π-mesons by nucleons," *Progress of Theoretical Physics* 5 (1950): 634–668; quotation on 635.

41. Marshak, *Meson Physics*, preface.

42. See David Kaiser, "Nuclear democracy: Political engagement, pedagogical reform, and particle physics in postwar America," *Isis* 93 (2002), June: 229–268.

43. The argument in this section is condensed from David Kaiser, "Stick-figure realism: Conventions, reification, and the persistence of Feynman Diagrams, 1948–64," *Representations* 70 (2000): 49–86.

44. Peter Galison, "Minkowski's space-time: From visual thinking to the absolute world," *Historical Studies in the Physical Sciences* 10 (1979): 85–121; Stanley Goldberg, *Understanding Relativity: Origin and Impact of a Scientific Revolution* (Birkhäuser, 1984), esp. part III.

45. Feynman, "Space-time approach"; idem, Quantum Electrodynamics (unpublished lecture notes, Cornell, fall 1949); idem, Quantum Electrodynamics and Meson Theories (unpublished lecture notes, Caltech, February-March 1950); idem, Quantum Mechanics III (unpublished lecture notes, Caltech, fall 1953); idem, *QED: The Strange Theory of Light and Matter* (Princeton University Press, 1985). Copies of the 1949 and 1950 lecture notes are in the possession of Sam Schweber; a

copy of the 1953 lecture notes are in the possession of Elisha Huggins. My thanks to both for sharing the notes with me.

46. Pais, *Inward Bound*, 458–459; Schweber, *QED*, 444.

47. Jauch and Rohrlich, *Theory of Photons and Electrons*, 149; Ernest Henley and Walter Thirring, *Elementary Quantum Field Theory* (McGraw-Hill, 1962), 146. Cf. Richard Mattuck, *A Guide to Feynman Diagrams in the Many-Body Problem* (McGraw-Hill, 1967), chapters 2–3; Kenneth Ford, *The World of Elementary Particles* (Blaisdell, 1963), 191–201; M. Stanley Livingston, *Particle Physics: The High-Energy Frontier* (McGraw-Hill, 1968), 204–205; Feynman, *QED*, chapter 3.

48. Robert Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (University of Chicago, 1994).

49. See chapter 7 of Kaiser, *Drawing Theories Apart*.