Drawing Theories Apart

The Dispersion of FEYNMAN DIAGRAMS in Postwar Physics

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Introduction: Pedagogy and the Institutions of Theory

> I would wish, therefore, that the man who claims to be scientific
> first tell me the method he employs for his scientific demonstrations
> and then show me how he has been trained in it.
>
> GALEN, CA. AD 150[1]

RICHARD FEYNMAN AND HIS DIAGRAMS

Few scientists, living or dead, surpass Richard Feynman (1918–88) as a widely recognized scientific icon. His star rose early. Following undergraduate work at the Massachusetts Institute of Technology and graduate study at Princeton, Feynman served as the youngest subgroup leader in wartime Los Alamos—he was only twenty-five—leading a band of fellow physicists in the sprawling effort to build atomic bombs. As the war ground closer to a conclusion, physics departments throughout the United States jockeyed to hire Feynman, as word of his creativity and sheer calculating power began to spread. His boss at Los Alamos, Hans Bethe, managed to lure Feynman to Cornell, where he taught for five years before the California Institute of Technology enticed him away from Ithaca's winters to the golden groves of Southern California. At Caltech, Feynman's renown as an animated teacher grew to match that of his physics prowess. An invitation to teach Caltech's large introductory physics class during the late 1950s led to the famous *Feynman Lectures on Physics*—a three-volume set still known simply as "the red books" by admiring physicists the world over.[2] In later years, Feynman made a habit of giving informal lectures on

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[1] Galen, *On the Doctrines of Hippocrates and Plato* (1978), 113 (bk. 2, 3:17).

[2] Feynman, Leighton, and Sands, *Feynman Lectures on Physics* (1965). The phrase "red books" refers to the original cover design, which featured a bold, red background. Never mind that the "experiment" of teaching the class was a flop: even Feynman came to admit within a few years that his approach was too difficult for the entering undergraduates, though it resonated with the grateful graduate students, postdoctoral students, and faculty who made a habit of sneaking into Feynman's lectures. Ibid., 3–5.

physics at neighboring industries and of teaching his "Physics X" class, a freewheeling class open to anyone with questions about science. All the while he made lasting contributions to physicists' understanding of electrodynamics, nuclear forces, solid-state physics, and gravitation, many of which still bear his name.

The Nobel Prize that Feynman shared with two fellow physicists in 1965 piqued the wider public's interest; by the mid-1980s, he had became a folk hero well beyond the world's clique of theoretical physicists. A visible role in the 1986 investigation of the explosion of the space shuttle *Challenger*—dunking a piece of O-ring rubber in a glass of ice water in the midst of a televised press conference—capped a decades-long process of becoming a household name. One year after his death, the American Physical Society, American Association of Physics Teachers, and American Association for the Advancement of Science jointly organized a daylong memorial. Since then, four biographical portraits and at least five collections of friends' and colleagues' reminiscences have been published, supplementing the two best-selling compilations of his own anecdotes and aphorisms that were published near the end of his life.[3] A book of his artwork and a compact disk of his celebrated bongo drumming were each on sale during the 1990s.[4] Television specials have supplemented his popular accounts of modern physics and of his own life story. During the late 1990s, the Apple Computer Company selected various portraits of Feynman, along with portraits of other iconic figures such as Albert Einstein, Pablo Picasso, and Charlie Chaplin, for their "Think different" advertising campaign. (See fig. 1.1.) More recently, no less an actor than Alan Alda depicted Feynman in a solo performance at the Vivian Beaumont Theater in New York City's Lincoln Center. The show sold out so quickly upon its opening in November 2001 that the producers brought it back for an even longer run the following spring. As

[3] On the jointly sponsored memorial event on 18 Jan 1989 in San Francisco, see Lubkin, "Special issue: Richard Feynman" (1989), 23. Biographies include Gleick, *Genius* (1992); Schweber, *QED* (1994), esp. chap. 8; Mehra, *Beat of a Different Drum* (1994); and Gribbin and Gribbin, *Richard Feynman* (1997). Interviews and reminiscences include the Feb 1989 issue of *PT;* Ralph Leighton, *Tuva or Bust!* (1991); Brown and Rigden, *Most of the Good Stuff* (1993); Sykes, *No Ordinary Genius* (1994); and Mlodinow, *Feynman's Rainbow* (2003). Feynman's own anecdotes were published as Feynman with Leighton, *Surely You're Joking* (1985); and Feynman with Leighton, *What Do You Care* (1988). Both of these books were soon reissued, with wider circulation, in the New Age Books imprint series from Bantam. On the Feynman publishing industry, see also Goodstein, "Feynmaniacs should read this review" (1998).

[4] M. Feynman, *Art of Richard P. Feynman* (1995); examples of his artwork appear in "Richard Feynman, Artist," *PT* 42 (Feb 1989): 86–87. Information on purchasing CDs of Feynman's bongo drumming is available in the back of Feynman with Leighton, *What Do You Care* (1988); and in Gribbin and Gribbin, *Richard Feynman* (1997), 287.

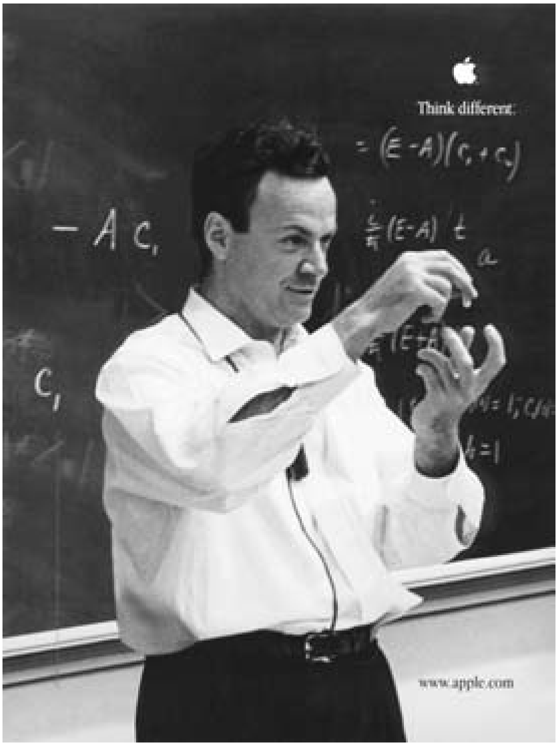

Figure 1.1. Richard Feynman featured in an Apple computer advertisement. (Source: *Scientific American* 280 [June 1999]: back cover.)

Alda explained, with more than a little Feynmanesque hyperbole, "Feynman's personality is so strong that if he was played by a three-foot-high dwarf of the opposite sex, you would still think it was Feynman up there."[5] Few academics answer to such depictions.

When the journal *Physics Today* dedicated a special memorial issue to Feynman in February 1989, the editor faced the difficult task of summing up Feynman's many contributions. She selected a simple graphic theme to unify the volume. "The diagram you see scattered throughout this issue is a reminder of the legacy Richard Feynman left us," the editor explained.[6] A simple line drawing, known to physicists as a "Feynman diagram," appeared atop each article in the issue, its white lines set against a dark field of mourning. (See fig. 1.2.) It was a fitting gesture. For all of Feynman's many contributions to modern physics, his

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[5] Alda as quoted in Overbye, "On stage" (2001), D5; *Playbill* 118 (Mar 2002): 15–23. The play, entitled *QED*, premiered in Los Angeles, with Alda playing Feynman, in March 2001.

[6] Lubkin, "Special issue: Richard Feynman" (1989), 23.

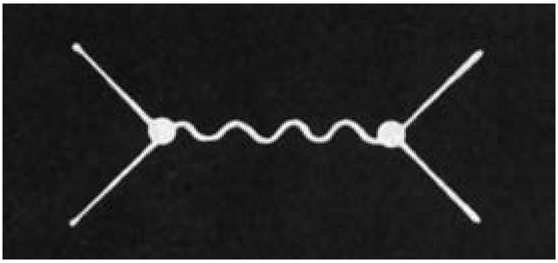

Figure 1.2. Feynman diagram from the special Feynman memorial issue of *Physics Today.* (Source: Lubkin, “Special issue: Richard Feynman,” 22.)

diagrams have had the widest and longest-lasting influence. Feynman diagrams have revolutionized nearly every aspect of theoretical physics since the middle of the twentieth century. Feynman first 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, some physicists even wielded the 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.

Today physicists all over the world scribble down the diagrams when studying everything from the mundane to the bizarre. Whether calculating the properties of materials that will form electronic computer chips, the levitation of magnets above superconductors, the behavior of particles near black holes, or the origin of matter itself just fractions of a second after the big bang, physicists begin by drawing Feynman diagrams. Everything about using the diagrams has become routine. Particle physicists can now download computer programs that will both draw the standardized diagrams and evaluate the associated mathematical terms: the diagrams’ lines and their mathematical content have become both algorithmic and automatic. As one physicist wrote in a recent preprint detailing the new automated diagram tools, “Explaining the necessity of [diagrammatic] one-loop calculations in the light of modern-day colliders is like carrying owls to Athens”—the centrality of Feynman diagrams to everyday practice, and the need to evaluate large numbers of them quickly, simply goes without saying.[7] The latest programs can generate and evaluate one thousand simple diagrams in about five minutes on an ordinary desktop computer.

Yet it hasn't always been this way. A generation of theoretical physicists earned doctorates for performing far less grandiose tasks just a few short decades ago. Evaluating a few Feynman diagrams by hand was a publishable feat in the early 1950s, and the stuff of which scores of dissertations were made. More important, both aspects of the diagrams' use and interpretation—how to draw them and how to calculate with them, that is, what they meant—became contested and shifting during the two decades after their introduction in the late 1940s. Today's automated computational utopia hides a rich history of competing appropriations and theoretical foundations for these bare line drawings. During the 1950s and 1960s, Feynman diagrams did not compel, by themselves, a unique meaning or interpretation. They were drawn differently and mustered in different fashions to varying calculational, and ultimately ontological, ends. Despite the diagrams' centrality today, and despite all the attention lavished on Feynman himself—his quirky genius, his lasting contributions to physics, his now-famous eccentricities—no attention has been paid to how his simple-looking diagrams actually came to be embraced by so many physicists for so many distinct applications. Indeed, more has been written about the hunt for Feynman's celebrated passenger van, bedecked in larger-than-life Feynman diagrams, than about how scores of physicists—and soon hundreds and thousands—came to traffic in the diagrams in the first place.[8]

Several physicists and historians have scrutinized various roots within Feynman's thinking for what became Feynman diagrams.[9] My project concerns instead what happened to the diagrams once they made the leap *out* of Feynman's head. How did the diagrams spread so quickly? What kinds of applications did physicists forge for the line drawings? Why did the diagrams remain central to physicists' work even as related methods of calculation came and

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[7] Hahn, "*FeynArts, FormCalc,* and *LoopTools*" (1999), 1. See also Tentyukov and Fleischer, "DIANA, a program for Feynman diagram evaluation" (1999); and Tentyukov and Fleischer, "Feynman diagram analyzer" (2000).

[8] On the search for Feynman's van, see Gribbin and Gribbin, *Richard Feynman* (1997), 281–84; and Sykes, *No Ordinary Genius* (1994), 85–86.

[9] Schweber, "Feynman" (1986); Mehra, *Beat of a Different Drum* (1994), chaps. 5, 6, 10–14; Schweber, *QED* (1994), chap. 8; and Galison, "Feynman's war" (1998). Feynman recounted his route to the diagrams in his Nobel Prize acceptance speech: Feynman, "Space-time view" (1966).

went? Feynman's protracted and largely private struggles to work out a consistent calculational scheme for quantum electrodynamics paled in comparison with the efforts required to equip other physicists with the new diagrammatic tool. Long after Feynman himself had grown accustomed to thinking and working with his diagrams, they remained neither obvious nor automatic for others. In fact, Feynman's particular ideas about the diagrams provided only one—and by no means the most important one—of several contrasting factors in determining how other physicists would treat them.

Rather than dwell on the isolated thoughts of a few Nobel laureates, therefore, I focus in this book on the *pedagogical* work involved in training large numbers of researchers to approach physical questions in similar ways. Unlike previous historical treatments of modern physics, this study follows neither the grand march of particular theories nor the lumbering progress of ever-growing experiments. Instead, I follow Feynman diagrams around, focusing on how physicists fashioned—and constantly refashioned—the diagrams into a calculational *tool,* a theoretical *practice.* My goal is to unpack the history of postwar theoretical physics from the ground up, as a story ultimately about crafting, deploying, and stabilizing the tools that undergird everyday calculations. Research tools such as Feynman diagrams never apply themselves; physicists have to be trained to use them, and to interpret and evaluate the results in certain ways. Stabilizing the new tool went hand in hand with training a new generation of theoretical physicists after World War II. The story of Feynman diagrams' spread thus illuminates larger transformations in what "theoretical physics" would be and how young physicists would become "theorists" after the war.

By following how physicists learned about and used Feynman diagrams from the late 1940s through the late 1960s, we bring broader changes in the infrastructure and intellectual development of postwar physics into focus. Everything about physicists' patterns of work came in for reevaluation after the war, from the methods of training young theorists, to the means of communicating new results and techniques, to decisions about what topics merited study and by what means. The diagrams likewise reveal the fissures and politics of becoming a young theorist after the war—from the international politics of the cold war, which shaped how physicists could communicate with colleagues in other countries; to the cold war's domestic doppelgänger, McCarthyism, and its effects on physicists' civil liberties and patterns of thought; to the generational politics between mentors and students, and the microsocial politics between competing research groups. In all these ways, following Feynman diagrams around helps us make sense of theoretical physicists' changing world.

This account of the dispersion of Feynman diagrams draws on several general themes, which I discuss in this introductory chapter and elaborate upon in the chapters that follow. Before turning to these issues in more detail, I should pause and discuss the book's title, *Drawing Theories Apart.* Two senses of the phrase are likely to be recognizable already: first, an emphasis upon drawing and similar pencil-and-paper work within theoretical physics; and second, the need to rethink the roles of theories versus tools in our accounts of modern physics. A third reference might not be as obvious for all readers: an inverted analogy with Bruno Latour's 1986 article "Drawing Things Together." As scholars in science studies will no doubt recognize in the chapters that follow, this book draws on several quintessentially Latourian themes: the building of networks, the importance of inscriptions, the work of translation and enrollment, and so on. Indeed, the very idea of following a nonhuman scientific object around as an organizing principle bears a certain Latourian signature.[10] Yet I follow a different line when it comes to the question of "immutable mobiles," a notion that Latour introduced in his 1986 article. Whereas Latour emphasizes "optical consistency" (even "immutability") as an essential feature of why diagrams and other scientific inscriptions carry so much force among scientists, I focus instead on unfolding variations within their work—on the production and magnification of local differences, and the work required to transcend these differences when comparing results from different places.[11] Hence the "apart" of my title, in place of Latour's "together."

PAPER TOOLS AND THE PRACTICE OF THEORY

Despite their centrality, the crafting and use of theoretical tools such as Feynman diagrams has not found an easy place within historians' and philosophers' traditional accounts of modern physics. Most studies have followed in the spirit of a joke that the wisecracking theorist George Gamow was fond of making. Gamow used to explain to his students what he liked most about

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[10] Latour, "Drawing things together" (1990 [1986]). See also Latour and Woolgar, *Laboratory Life* (1986 [1979]); Latour, *Pasteurization* (1988 [1984]); Latour, *Science in Action* (1987); and Latour, *Aramis* (1996).

[11] This move parallels recent work by historians of early modern printing: rather than focus on the purported "fixity" of printed (rather than manuscript) texts, recent historians have highlighted the mutability of printed texts themselves and of the varied readings they could inspire. Cf. Eisenstein, *Printing Press* (1979); with Johns, *Nature of the Book* (1998); Eisenstein, "Unacknowledged revolution revisited" (2002); and Johns, "How to acknowledge a revolution" (2002). See also Darnton, *Great Cat Massacre* (1984); Chartier, *Order of Books* (1992); de Grazia and Stallybrass, "Materiality of the Shakespearean text" (1993); and Secord, *Victorian Sensation* (2000).

being a theoretical physicist: he could lie down on a couch, close his eyes, and no one would be able to tell whether or not he was working.[12] For too long, historians and philosophers have adopted Gamow's central (not to say sleepy) metaphor: research in theory, we have been told, concerns abstract thought, wholly separated from anything like labor, activity, or skill. Theories, worldviews, or paradigms seemed to be the appropriate units of analysis, and the challenge became charting the birth and conceptual development of particular ideas. In these traditional accounts, the skilled manipulation of tools played little role: theorists were assumed to write papers whose content other theorists could understand, at least in principle, anywhere in the world. Ideas, embodied in texts, traveled easily from theorist to theorist in these accounts, shorn of the material constraints that might make bubble chambers or electron microscopes (along with the skills required for their use) difficult to carry from place to place. The age-old trope of minds versus hands has been at play: a purely cognitive realm of ideas has been pitted against a manual realm of action. In short, more "night thoughts" than desk work, more *Weltbild* than *Fingerspitzengefühl.*

During the past decade, a rival vision of how to analyze work in theoretical sciences has begun to take shape.[13] Building upon these studies, this book begins with a simple premise: since at least the middle of the twentieth century—and, arguably, during earlier periods as well—most theorists have not spent their days (or, indeed, their nights) in some philosopher's dreamworld, weighing one cluster of disembodied concepts against another, picking and choosing among so many theories or paradigms. Rather, their main task has been to *calculate.* Theorists 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 these translations by fashioning theoretical tools and performing calculations. Theorists have used calculational tools, in other words, to mediate between various

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[12] Geoffrey Chew, who studied under Gamow as an undergraduate at George Washington University during the early 1940s, repeated Gamow's joke in a May 1997 interview with Stephen Gordon, as quoted in Gordon, *Strong Interactions* (1998), 15n10.

[13] See esp. Olesko, *Physics as a Calling* (1991); Warwick, "Cambridge mathematics" (1992); Warwick, "Cambridge mathematics" (1993); Warwick, *Masters of Theory* (2003); Krieger, *Doing Physics* (1992); Pickering and Stephanides, "Constructing quaternions" (1992); Buchwald, *Scientific Practice* (1995); Galison and Warwick, *Cultures of Theory* (1998); Morgan and Morrison, *Models as Mediators* (1999); Kennefick, "Star crushing" (2000); and MacKenzie, "Equation and its worlds" (2003).

kinds of representations of the natural world. These tools have provided the currency of everyday work.

Focusing on theorists' tools cuts orthogonally across conceptual histories of theoretical physics, since physicists often improvise with calculational techniques across a wide range of distinct topics or fields of inquiry. Feynman himself explained near the end of his life that he chose various topics to work on—from the scattering of electrons to the superfluid behavior of liquid helium, from vortex rings and polarization waves in crystals to superconductivity, from the scattering of constituents inside protons to the scattering of gravity waves off of planets—because such problems all fell "in the range of my tools."[14] Concentrating on the separate theory domains with which such problems are usually associated—electrodynamics, solid-state physics, nuclear and particle physics, or gravitation—obscures deeper continuities in daily practice and calculational approach. Historians Andrew Warwick and Ursula Klein have given useful names to such calculational techniques, each drawing on an analogy to instruments: Warwick introduced the term "theoretical technology," and Klein the phrase "paper tools."[15] Since the late 1940s, generations of physicists have turned more and more often to Feynman diagrams as their paper tool of choice.

In order to build an account of theorists' uses of paper tools, we must understand how tools have functioned in historians' and sociologists' studies of the laboratory and field sciences. Central to many of these studies is the notion of skill. Students rarely look through a microscope the first time and see what they are supposed to see. The point becomes all the more obvious with the gargantuan apparatus of postwar "big science": there is nothing obvious or natural (at first) about pumping high-pressure liquid hydrogen into a bubble chamber, or searching for identifiable patterns in the meandering tracks of the millions of resulting photographs. With practice and over time, however, using scientific instruments eventually becomes obvious or even second nature for accomplished practitioners. As the eighteenth-century chemist Joseph

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[14] Richard Feynman, Jan 1988 interview with Jagdish Mehra, as quoted in Mehra, *Beat of a Different Drum* (1994), 429. The condensed-matter theorist David Pines drew a similar conclusion about Feynman's research trajectory: "Feynman became interested because he saw the polarons as an opportunity to test the power of his path integral approach." Pines, "Richard Feynman" (1989), 66.

[15] Warwick, "Cambridge mathematics" (1992); Warwick, "Cambridge mathematics" (1993); Warwick, *Masters of Theory* (2003); U. Klein, "Techniques of modelling" (1999); U. Klein, "Paper tools" (2001); and U. Klein, *Experiments, Models, Paper Tools* (2003). For examples drawn from the life sciences, see Clarke and Fujimura, *Right Tools for the Job* (1992).

Priestley argued, scientists build up these skills, not (or not only) by reading elaborate written instructions for the instruments' use, but by practicing with the tools day in and day out:

> I would not have any person, who is altogether without experience, to imagine that he shall be able to select any of the following experiments, and immediately perform it, without difficulty or blundering. It is known to all persons who are conversant in experimental philosophy, that there are many little attentions and precautions necessary to be observed in the conducting of experiments, which cannot well be described in words, but which it is needless to describe, since practice will necessarily suggest them; though, like all other arts in which the hands and fingers are made use of, it is only *much practice* that can enable a person to go through complex experiments, of this or any other kind, with ease and readiness.[16]

As Priestley admonished, and as a number of historians and sociologists have concurred more recently, experimentalists must work hard to hone something like artisanal knowledge or craft skill in addition to an understanding of general principles. No amount of formal written instructions will suffice for producing this feel for the instrument—no student jumps from reading a manual to using an instrument correctly the first time. Michael Polanyi gave a name to those skills that Priestley argued "cannot well be described in words": *tacit knowledge*.[17]

Historians and sociologists have argued that tacit knowledge plays a central role when it comes to replicating someone else's instruments, even when the would-be replicator is already an expert experimentalist or instrument maker. In cases ranging from the design of modern-day lasers, to the use of early modern air pumps and glass prisms, to the establishment of electrical standards during the height of Britain's imperial rule, no amount of written instructions, supplied at a distance from the original site, proved sufficient for successful replication. Certain features of the instruments' design and use, according to these studies, remained literally ineffable—the rules required for actual use remained impossible to specify fully, even in principle, via textual instructions alone. The key to successful replication in each of these cases—the one way to transmit the tacit knowledge required to build and use these tools—was through extended personal contact. Only those scientists who worked face to

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[16] Priestley, *Experiments and Observations* (1775), 2:6–7, as quoted in Levere, "Measuring gases" (2000), 111; emphasis in original.

[17] Polanyi, *Personal Knowledge* (1958); Polanyi, *Tacit Dimension* (1967). See also Lave, *Cognition in Practice* (1988).

face with those already "in the know" could develop the skills and master the practices necessary to build and use these instruments.[18]

Craftlike skill, local practices, and material culture might now seem appropriate categories for analyzing laboratory and field sciences, but what about theoretical sciences? Getting the "feel" in one's fingers for how to turn a dial or solder an electrical connection is one thing, but isn't theoretical work all about manipulating representations on paper—that is, isn't it *all* about texts? Once we shift from a view of theoretical work as selecting between preformed theories, however, to theoretical work as the crafting and use of paper tools, tacit knowledge and craft skill need not seem so foreign. Thomas Kuhn raised a similar point with his discussion of "exemplars." Kuhn wrote that science students must work to master exemplars, or model problems, before they can tackle research problems on their own. The rules for solving such model problems and generalizing their application are almost never adequately conveyed via appeals to overarching general principles, and rarely appear in sufficient form within published textbooks.[19] To put the matter in a more mundane way: theoretical physicists do not enter the field on the basis of correspondence courses, sending and receiving written instructions in the absence of any face-to-face training interactions.

If the point has been underappreciated by scholars in science studies, it was by no means lost on theoretical physicists themselves. Hans Bethe, for example, lobbied hard in 1950 with the American consul general in Genoa to gain a visitor's visa for an Italian physicist, Antonio Borsellino. "Professor Borsellino has made theoretical studies about matters which are directly connected with the experimental investigations" then underway at Cornell, Bethe explained. Despite Borsellino's publications, Bethe continued, he and his colleagues had been "unable . . . to obtain enough information on Professor Borsellino's theory to enable us to use it, and his personal presence here would therefore be of great value."[20] Nor was the matter restricted to theorists who worked an ocean

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[18] H. Collins, "TEA set" (1974); H. Collins, *Changing Order* (1992 [1985]); H. Collins, *Artificial Experts* (1990); Shapin and Schaffer, *Leviathan and the Air-Pump* (1985); Schaffer, "Glass works" (1989); and Schaffer, "Manufactory of ohms" (1992). See also Ravetz, *Scientific Knowledge* (1971); MacKenzie and Spinardi, "Tacit knowledge" (1995); Fujimura, *Crafting Science* (1996); Pinch, Collins, and Carbone, "Inside knowledge" (1996); Collins, de Vries, and Bijker, "Ways of going on" (1997); M. Jackson, *Spectrum of Belief* (2000); and Delamont and Atkinson, "Doctoring uncertainty" (2001).

[19] Kuhn, *Structure* (1996 [1962]), 187–98.

[20] Hans Bethe to the American consul general, Genoa, Italy, 28 Nov 1950, in *HAB,* Folder 9:60. See also the one-page memorandum by Guiseppe Coccioni, 19 Dec 1950, in the same folder. Bethe and Coccioni had difficulty gaining the visitor's visa for Borsellino because of the recently enacted McCarran Act, which blocked visas to former members of such groups as the Fascist Party

apart and who were not well acquainted personally. A few years earlier, in the summer of 1947, Enrico Fermi had complained that he was unable to make sense of one of Bethe's own recent papers, and hence could not reproduce and extend Bethe's calculation. Fermi and Bethe were both experts in the field in question, and they had worked closely together throughout the war years; they knew the territory and they knew each other quite well. All the same, however, only after Fermi and Bethe met together again that summer, where they could talk face to face about Bethe's work, could Fermi follow all the ins and outs of the calculation and carry it forward.[21] In both of these examples, senior theorists had difficulty making adequate sense of each other's work to build on it without informal personal communication. As we will see throughout part 1 (chaps. 2–4), the issue was magnified greatly when it came to younger theorists, still in the midst of their training. Something like tacit knowledge—or at least extended personal contact over and above the exchange of formal written instructions—proved crucial for spreading Feynman diagrams around.

Note that I wrote "something like tacit knowledge": Priestley and Polanyi notwithstanding, the category of tacit knowledge has not been without its detractors, and important cautions must be acknowledged before we import the category into studies of theoretical sciences. Myles Jackson, for example, raises the important point that items of scientific practice may remain "tacit" for many different reasons: trade secrecy and designs to protect priority are hardly the same as an in-principle inability to express all the nuances of experimental protocols. Stephen Turner asks how, if tacit knowledge is truly tacit, historians, philosophers, and sociologists can ever analyze it, let alone prove that it was the causal agent behind a scientist's work. Kathryn Olesko notes that much of what has formerly been classified as tacit knowledge—such as a scientist's judgments about how to analyze and reduce data—was in fact subject to explicit codification. And Peter Galison cautions against reifying skill: many aspects of laboratory life, Galison finds, were easily replicable at a distance, while others remained more stubbornly rooted to time and place.[22]

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in Italy. Bethe explained that Borsellino had never been a party member and so was eager to head off potential State Department opposition from the start. We will examine similar problems that other physicists encountered regarding visas and passports during this period in chap. 9.

[21] Enrico Fermi to Edwin Uehling, 26 Sep 1947, as quoted in Schweber, *QED* (1994), 232. The paper in question was Bethe's nonrelativistic treatment of the Lamb shift, which we will examine in chap. 2.

[22] M. Jackson, *Spectrum of Belief* (2000), 10–13; Turner, *Social Theory of Practices* (1994), esp. chaps. 4, 6; Olesko, "Tacit knowledge" (1993); and Galison, *Image and Logic* (1997), 52–54.

Even with these well-placed cautions and caveats, I find tacit knowledge helpful as a phenomenological description of how Feynman diagrams spread. Exceedingly few physicists picked up the diagrams and applied them in research based only on reading articles about the new techniques. In almost every case, some form of informal personal communication—usually of an explicitly pedagogical kind, such as an adviser mentoring graduate students, or postdocs working closely together—can be traced behind the scenes of physicists' uses of Feynman diagrams. Looking at the work that physicists performed before and after they interacted with members of the growing diagram-using network quickly reveals a stark pattern: even years after the diagrams had been described at length in print, individuals—and even whole departments—that were not in personal contact with members of the diagrammatic network did not make any use of the diagrams, whereas those people who interacted regularly with other diagram users made frequent use of the diagrams. Without reifying "skill" or trafficking in inscrutable or mystical know-how, the concept of tacit knowledge remains useful because of its emphasis upon these nontextual means of transmission.[23]

Yet in one important example that we will consider in chapter 4—a small group of physicists working in the Soviet Union during the early years of the cold war—some theorists *did* pick up the diagrams and make use of them without any discernible personal contact with other diagram users. Clearly *something* about the diagrams could travel via texts alone. The question thus becomes not *whether* they could travel in principle, but rather *what exactly* traveled via these published sources? The handful of physicists who learned about the diagrams only from texts developed an adequate facility with one way to calculate with the diagrams. What seems not to have been transmitted via texts was a sense for how else the diagrams could be applied as useful tools. That is, some form of understanding could be packaged and transmitted via texts, but not the more improvisational uses developed by those groups of physicists who shared informal contacts. Infinite epistemic barriers—the "all or nothing" lurking behind most analyses of tacit knowledge—did not separate those who learned about the diagrams in person from the small minority who learned about them only from publications. Nor will we be aided by Kuhn's notion of "incommensurability": the gap between diagram users and the rest was not a hopeless conceptual mismatch so much as differences in physicists' preferences for one type of tool over another. A spectrum emerged, as physicists in various

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[23] A similar point is made in Trevor Pinch's review of Turner's *Social Theory of Practices:* Pinch, "Old habits die hard" (1997).

local settings made choices about what to work on, how best to deploy their hard-won tools, and what kinds of calculations their students should practice and master. Few aspects of these choices are captured by talk of "knowledge," "information," or "conceptual understanding." Rather, the idiom is one of skills and tools.

PEDAGOGY AND POSTWAR PHYSICS

Central to the argument of this book is that once we begin to examine the tools of theory, we must also consider the tool users—and thereby we enter the realm of pedagogy and training. Rather than construe "pedagogy" in the narrow sense of classroom teaching techniques—though these are certainly important —I interpret "pedagogy" more broadly. I am interested in the institutions of training by means of which young physicists became working theorists during the decades after World War II. Much of this training and apprenticeship took place outside the classroom. Thomas Kuhn raised similar themes with his discussion of exemplars in the theoretical sciences, though his prescient analysis left many questions open. In particular, he did not explore in any historical detail how such exemplars emerged, how students in various generations actually learned to solve exemplars and build upon them in their research, or whether students at different training centers learned about and leaned upon exemplars in distinct ways.[24] Kathryn Olesko and Andrew Warwick have begun to fill in Kuhn's picture. In their studies of nineteenth-century Königsberg and Cambridge (respectively), they have demonstrated that the transfer of theoretical skills was tied directly to new pedagogical instruments, such as new types of seminars, problem sets, paper-based examinations, and private coaching. As these new teaching techniques emerged over the course of the nineteenth century, so too did a distinct specialty called "theoretical physics," along with a critical mass of young physicists who now considered it their own.[25]

These detailed historical studies point to institutional and pedagogical reforms and their bearing on the form and content of knowledge. We learn from their accounts how new skills were inculcated in a particular setting and how these techniques became second nature to students in training there.[26] To understand physicists' incorporation of Feynman diagrams during the middle

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[24] Kuhn, *Structure* (1996 [1962]).

[25] Olesko, *Physics as a Calling* (1991); Warwick, *Masters of Theory* (2003). Cf. Jungnickel and McCormmach, *Intellectual Mastery of Nature* (1986).

[26] Cf. Traweek, *Beamtimes and Lifetimes* (1988); Smith and Wise, *Energy and Empire* (1989), esp. chaps. 1–5; Gooday, "Precision measurement" (1990); Gooday, "Teaching telegraphy" (1991);

decades of the twentieth century, we must remain similarly sensitive to changes in physicists' institutions and infrastructure. In particular, the diagrams' rapid dispersion raises the question of how theoretical skills—honed in local settings, subject to all the contingencies of time and place—became shared across so many distinct locations and research groups. That is, how did local practices take on the trappings of universal methods? Sustained pedagogical work proved crucial, connecting physicists at Cornell, Princeton, Berkeley, Cambridge, Tokyo, and a half dozen other sites in between. To understand these mechanisms of transfer, we must throw our historical net wide, tracing developments in each of these far-flung places; no single site will do.

During the 1940s and 1950s, the diagrams were adopted quickest and to the widest extent by physicists working in the United States—and thus it is crucial to understand the broader American setting within which Feynman diagrams began to disperse. The early postwar years presented unprecedented challenges to American physicists: following the dramatic (and well-publicized) wartime efforts of physicists on radar and the atomic bomb, and with the aid of such measures as the G.I. Bill, enrollments in physics graduate programs grew at nearly twice the rate of all other fields combined. Just four years after fighting had ended, the nation's physics departments were granting three times as many doctorates per year as the prewar highs—a number that would soon climb by another factor of three after the surprise launch of Sputnik. From the late 1940s through the end of the 1960s, the tremendous and rapid growth in American physics departments was felt viscerally in small, medium, and large departments alike. No aspect of daily life remained untouched, from the lack of office space and overcrowded laboratory conditions, to the proliferation of formal bureaucratic procedures where informal precedents had once held sway, to widespread feelings of "facelessness" and a perilous loss of "intimacy" amid the overflowing lecture halls. Even routine social events, such as the Berkeley physics department's annual picnics, became logistically exhausting affairs.[27] At the same time, the U.S. Atomic Energy Commission began to construct a coast-to-coast series of national laboratories, in part to keep the nation's physicists "on tap" and at the ready in case of the next outbreak of fighting.[28] The ever-growing cyclotrons at the new laboratories quickly flooded

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Geisen and Holmes, *Research Schools* (1993); Kohler, *Lords of the Fly* (1994); Rudolph, *Scientists in the Classroom* (2002); and Kaiser, *Pedagogy and the Practice of Science* (2005).

[27] Kaiser, "Cold war requisitions" (2002); Kaiser, "Suburbanization" (2004).

[28] Seidel, "Home for big science" (1986); Forman, "Behind quantum electronics" (1987); and Westwick, *National Labs* (2003).

high-energy physicists with more data on particle scatterings and interactions than ever possible before.[29]

In the midst of skyrocketing enrollments, overflowing lecture halls, and unprecedented reams of experimental data, American physicists suddenly were faced with the twin questions: how do you train roomfuls of students instead of handfuls of disciples, and what do you train them to do? Institutionally, their responses centered on revamped postdoctoral training, refurbished summer schools, and new series of informal and inexpensive textbooks. Intellectually, most theorists interested in nuclear and high-energy phenomena redoubled their efforts to develop phenomenological models of use to their experimentalist colleagues. To many of these theorists, Feynman diagrams seemed to offer the ideal pedagogical resource for their newfound challenges: tailor made (or, rather, tailorable) for efficient, repeatable—and hence trainable—calculations, they gave theorists and their students a way to "get the numbers out" with greater efficiency than ever before. The oft-discussed pragmatic character of American science, and of postwar American theoretical physics in particular, must be understood in terms of these changes in physicists' infrastructure.[30] As we will see in chapters 4, 6, and 8, physicists working in other parts of the world faced different challenges after the war: not fables of abundance but rigors of reconstruction. Working within their own institutional constraints, when physicists outside the United States picked up Feynman diagrams and put them to work, they often did so in ways distinct from those of their American colleagues. Neither of the large-number pressures—student enrollments or new experimental data—affected physicists outside the United States in the same ways. These differences were inscribed in the calculations they undertook and the diagrams they drew.

OVERVIEW: THE TWO MEANINGS OF DISPERSION

Physicists' uses of Feynman diagrams from the late 1940s through the late 1960s present a series of puzzles. Practically no physicists could understand

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[29] Pickering, *Constructing Quarks* (1984), chap. 3; Galison, *How Experiments End* (1987), chap. 4; Galison, *Image and Logic* (1997), chap. 5–7; Brown, Dresden, and Hoddeson, *Pions to Quarks* (1989); and Polkinghorne, *Rochester Roundabout* (1989).

[30] The phrase "get the numbers out" is borrowed from Schweber, *QED* (1994), chap. 3. See also Cini, "Dispersion relations" (1980); Schweber, "Empiricist temper regnant" (1986); Pickering, "From field theory to phenomenology" (1989); Assmus, "Americanization of molecular physics" (1992); and Galison, *Image and Logic* (1997), chap. 4. Cf. Kohler, "Ph.D. machine" (1990); and Oreskes, *Rejection of Continental Drift* (1999), chap. 5.

what Feynman was doing with his unusual doodles during his inaugural presentation in the spring of 1948, yet other physicists picked up the diagrams, often without comment, only a few months later. Seemingly standard rules for drawing and calculating with the diagrams were in place by early 1949, yet differences in the diagrams' pictorial form, calculational use, and theoretical interpretation quickly multiplied. In fact, the main purpose for which the diagrams had been fashioned seemed entirely hopeless for tackling the pressing problems of the early and mid-1950s, yet physicists clung to their diagrams, tweaking them here and there, even as they discarded other elements of their calculational instrumentarium. These puzzles point to three main questions, around which this book is organized. Chapters 2–4 address the question, *how* did the diagrams spread so quickly? Chapters 5–9 ask, for *what* did physicists actually use the diagrams during the two decades after their introduction? Given the great variety of uses and interpretations, chapter 10 examines the question, *why* did the diagrams "stick," even as related techniques came and went?

In order to make sense of the themes of transmission and differentiation, I draw upon the term "dispersion." One cluster of meanings is especially pertinent to the book's first question, regarding how the diagrams spread: "To distribute from a main source or centre . . . ; to put into circulation." A second meaning of "dispersion" points to the ever-expanding circle of competing uses forged for the diagrams: "To cause to separate in different directions . . . ; to spread in scattered order."[31] "Dispersion" captures at once the work required to make paper tools travel and the plasticity of those tools once they do travel; both meanings are essential. Here, in outline, are some of the main contours of the diagrams' dispersion:

How did the diagrams spread? If the tools of theory were not (or not only) conveyed as disembodied textual information, but rather involved something closer to craft skill and artisanal knowledge, then what mechanisms did physicists draw upon to put the tools into circulation? As we will see in chapters 2–4, Feynman diagrams spread by means of several new institutional arrangements, each taking form in the years after World War II. Most important within the United States was the rise of postdoctoral education for theoretical physicists. Although postdoctoral training had been introduced in the sciences during the early decades of the twentieth century, it became a standard element of

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[31] Simpson and Weiner, *Oxford English Dictionary* (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." Cf. Jordan and Lynch, "Plasmid prep" (1992).

American theoretical physicists' training only after the war. Its champions custom-designed postdoctoral training for the cultivation of craftlike skill. Equally important, postdocs were seen as the best way to circulate new tools: after completing their graduate work, physicists almost always moved to a new site for their postdoctoral training before moving to a third site to begin teaching. Leading science policy makers engineered postdoctoral training to feature precisely these design specs: skill acquisition and circulation. Similar, though by no means identical, pedagogical mechanisms of circulation took form within other countries, such as Japan, just at the time that Feynman diagrams were introduced. Ancillary pedagogical materials rose up at the same time, further aiding the diagrams' spread: preprints (another prewar invention, put on a firmer foundation after the war), widely circulating (though unpublished) lecture notes, and soon new series of textbooks and reprint volumes. The new traffic in pedagogical texts reinforced, though rarely replaced, the primary circulation of students and postdocs.

What did physicists use the diagrams to do? As anthropologists such as Claude Lévi-Strauss have long emphasized, tools almost always prove malleable and multivalent. "Primitive peoples," explained Lévi-Strauss, constantly engage in bricolage—making do with the tools at hand. As he emphasized, a bricoleur's set of tools is not tied to a specific task or program; rather, the tools are accumulated over time and deployed on a case-by-case basis. The tool user's first move, in Lévi-Strauss's account, is thus retrospective: What tools do I already have? How have I used them in the past? What rearrangement might work now?[32] Lévi-Strauss clearly intended his account to apply to modern scientists and engineers as well; over the past twenty years, historians and sociologists have obliged, producing dozens of detailed descriptions of how experimental scientists tinker and improvise with tools, extending them piecewise for new investigations.[33] Theoretical physicists proved no less prolific at refashioning the tools at hand. Consider the examples in figure 1.3, each of which appeared with the label "Feynman diagram" between 1949 and 1954. As we will see throughout chapters 5–9, theorists tinkered with the new

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[32] Lévi-Strauss, *Savage Mind* (1966 [1962]), 16–22. See also Lynch, *Art and Artifact* (1985), chap. 1.

[33] See esp. Gooding, Pinch, and Schaffer, *Uses of Experiment* (1989); Galison, *How Experiments End* (1987); Galison, *Image and Logic* (1997); Van Helden and Hankins, *Instruments* (1994); Pickering, *Mangle of Practice* (1995); Rasmussen, *Picture Control* (1997); and Holmes and Levere, *Instruments and Experimentation* (2000). Historians of life sciences have explored similar themes in terms of model organisms: Kohler, *Lords of the Fly* (1994); Rader, "Mouse people" (1998); Rader, "Of mice, medicine, and genetics" (1999); and Creager, *Life of a Virus* (2002).

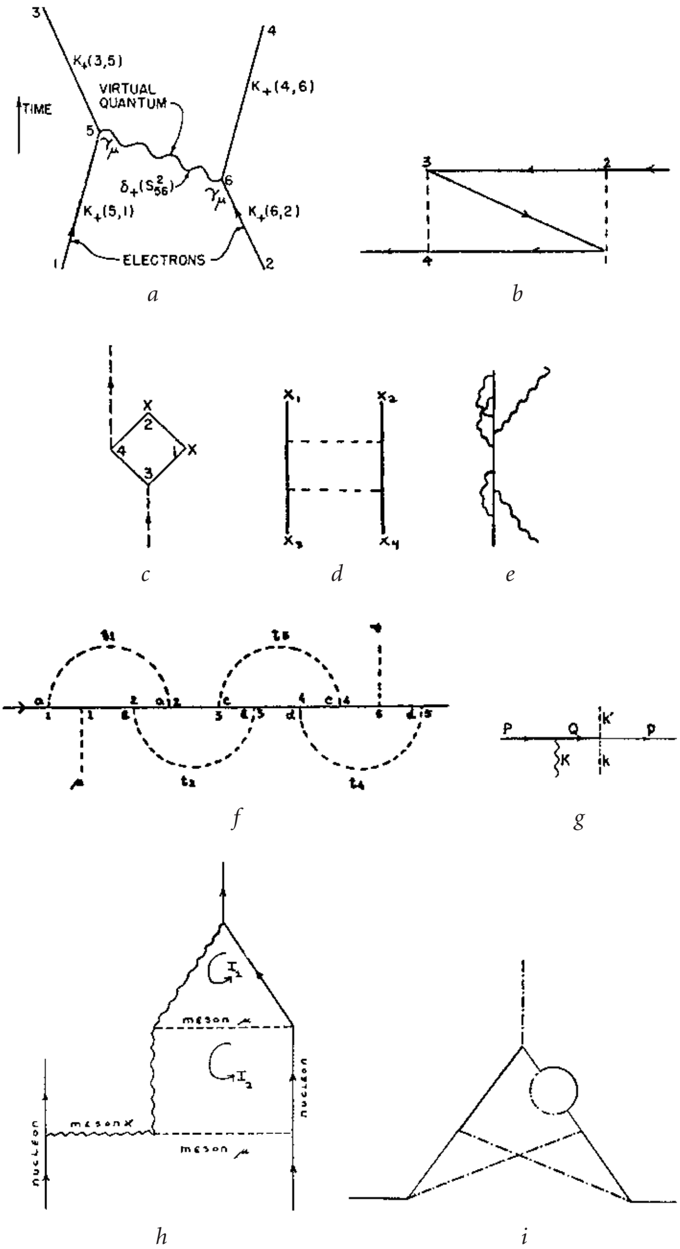

Figure 1.3. Feynman diagrams, 1949–54. (Sources: *a*, Feynman [A.4], 772; *b*, Villars, “Quantum electrodynamics” [1951], 65; *c*, Rohrlich and Gluckstern [A.54], 2; *d*, Gell-Mann and Low [A.43], 352; *e*, Low [A.65], 55; *f*, Salam [A.56], 735; *g*, Lenard [A.81], 971; *h*, Steinberger [A.6], 1182; *i*, Kroll and Ruderman [A.102], 235.)

diagrams, adapting them for use in the problems they deemed most relevant or pressing.[34]

These adaptations were neither random nor produced in vacuo. Instead, order may be brought to this scattershot display by focusing on theorists' training. Art historians have long been accustomed to classifying painters according to their training, using descriptive phrases such as "follower of van Eyk," or "from the school of Titian." Historians of science stand to make similar gains by considering the pedagogical links between young instructors and their students as these played out in various departments. In each local setting, physicists adapted features of the diagrams to better bring out aspects deemed most important for new kinds of calculations. More often than not, theorists trained their students to practice using the diagrams the way they used them. The diagrams drawn by young physicists at Cornell thus began to look different—and to be used in ways subtly distinct—from those drawn by their peers at Rochester, the University of Illinois, or Cambridge. (With practice, one can actually "predict" where a physicist was trained based on the diagrams he or she drew and the kinds of calculations in which the diagrams were enrolled.) Consider the examples in figure 1.4: in each pair, the diagram on the left comes from an adviser in one of the major training centers, and the example on the right from someone the adviser trained.[35] As we will see in chapters 5 and 6, mentors and students crafted the diagrams in locally varying ways, in pursuit of distinct types of calculations.

More than this, physicists in various places differed on what their diagrams purported to show. To some, the diagrams functioned as pictures of

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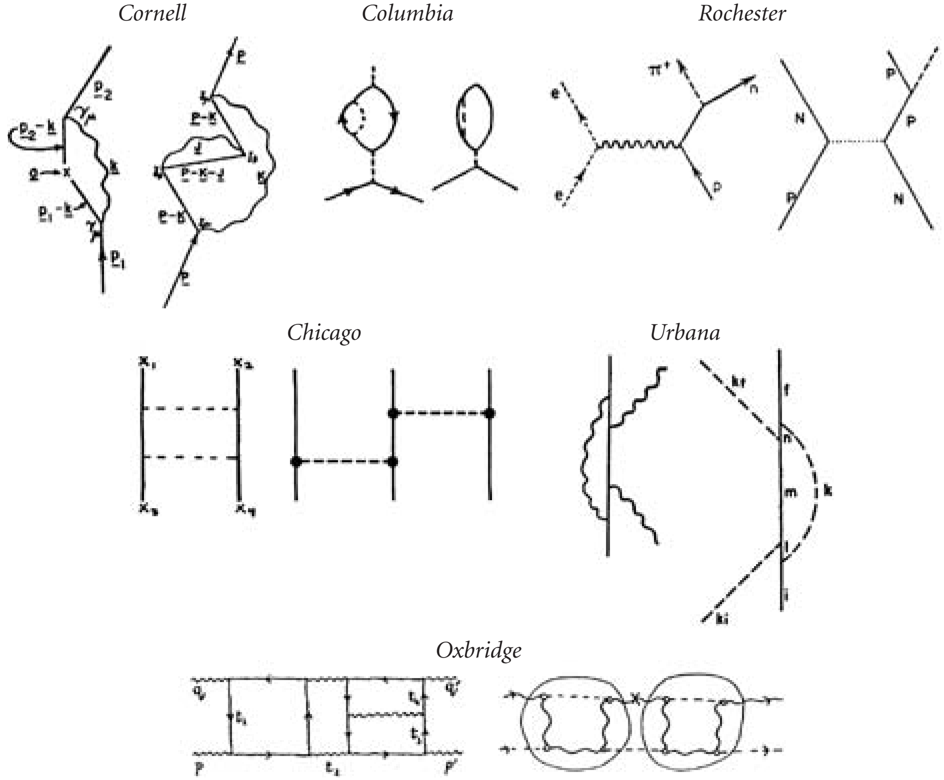
[34] Physicists developed several closely associated (and genealogically related) diagrams during the 1950s and 1960s, such as Lévy diagrams, diagrams for use with the Tamm-Dancoff approximation, Goldstone, Landau, and Cutkosky diagrams, and so on. Most of these stood in for classes of Feynman diagrams, such as all those with two particles in the intermediate state. These "Feynman-like" diagrams are also included in this study: where some physicists spoke of "Lévy diagrams," for example, others continued to label them simply "Feynman diagrams." Moreover, physicists routinely talked about relations between these types of diagrams. The label "Feynman diagram" did not extend infinitely, however: as we will see in chap. 10, there were other types of diagrams in play at the time, such as "dual diagrams," that physicists treated quite differently from Feynman diagrams and were often at pains to distinguish; they had little trouble telling them apart.

[35] The examples from Cornell, Columbia, and Rochester all involve advisers and their graduate students. The cases of Chicago and the University of Illinois involve older theorists who learned about the diagrams from their colleagues. In the final example, from Oxford and 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 *Columbia* *Rochester*

Chicago *Urbana*

Oxbridge

Figure 1.4. “Family Resemblances.” Mentors and students crafted diagrams for different purposes. (Sources: *Cornell,* Feynman [A.4], 775; Frank [A.41], 1190; *Columbia,* Karplus and Kroll [A.11], 537; Weneser, Bersohn, and Kroll [A.90], 1258; *Rochester,* Marshak, *Meson Physics* [1952], 39; Simon [A.15], 574; *Chicago,* Gell-Mann and Low [A.43], 351; Wentzel [A.73], 684; *University of Illinois at Urbana,* Low [A.65], 55; Chew [A.117], 1749; *Oxford and Cambridge,* Salam [A.32], 223; Ward [A.46], 899.)

physical processes—they seemed to capture something essential about the mechanisms of the microworld. To others, the diagrams were no more than helpful mnemonic aids for wading through long strings of complicated mathematical expressions—they were not to be confused with the stuff of the real world. Still others developed the diagrams as tools for a new kind of diagrammatic reasoning—the diagrams’ structural or topological features prompted and enabled the investigation of various symmetries that their associated mathematical expressions should obey. Most often, these distinct roles blurred together in practice. Feynman diagrams thus functioned as many other influential scientific diagrams have done—ranging from stratigraphic columns, to chemical formulas and atomic orbitals, to pictures of immunological antibodies—subject to the same slippage between realistic depiction, convenient calculational device, and heuristic guide to further research.[36]

Chapters 7–9 push the theme of the diagrams' many functions further, following physicists' efforts from the mid-1950s through the 1960s to pull together a new framework for studying nuclear particles' interactions. The Berkeley theorist Geoffrey Chew's "*S*-matrix program" grew out of the piecewise adaptation of Feynman diagrams, together with new rules (or, most often, rules of thumb) for their use. Chew and his students and collaborators came to read content in the diagrams' unadorned lines that ran counter to much of the diagrammatic machinery that had come before. Indeed, they argued that their new program, borne on the backs of reinterpreted Feynman diagrams—and which always seemed to be just around the corner, never quite at hand in final form—augured the death of quantum field theory itself, from which the diagrams' original rules had been derived. Chew's creative appropriation of the diagrams, which grew to dominate studies of the strong nuclear force during the 1960s, drew upon a mixed bag of conceptual resources. As he struggled to make sense of the strong force, Chew engaged in several other struggles as well, from political strife in the age of McCarthyism to unusual ideas about how to train large cadres of graduate students at a time when physics enrollments were booming. In pulling his particular package of diagrammatic techniques together—centered on what he called "nuclear democracy"—Chew drew metaphorically upon his earlier, intense efforts to forge "democracy" in other corners of his life. Physicists always appropriated Feynman's diagrams against a backdrop of local motivations and resources, forged by prior training and experience; Chew's fascinating work highlights the range of interpretations that physicists fashioned for Feynman diagrams.

Why did the diagrams stick? The challenge that Chew and his Berkeley group mounted to the prevailing framework for studying particles' interactions highlights a final question: why did physicists cling to Feynman diagrams, even as so many other elements of their toolkit came and went? Why, moreover, did physicists cling stubbornly to their Feynman diagrams when working on problems far beyond the domain for which the diagrams' effectiveness and efficiency had been demonstrated? In answering this final question, we must look beyond the myriad local differences and variations charted in earlier chapters and ask about more broadly shared features of physicists' training.

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[36] See esp. Rudwick, "Visual language" (1976); U. Klein, *Experiments, Models, Paper Tools* (2003); Park, "Quantum chemistry" (2005); and Cambrosio, Jacobi, and Keating, "Ehrlich's 'beautiful pictures'" (1993).

Art historians have long pondered such questions as why certain pictorial styles persist. Surveying the grand sweep of representational styles that have dominated at one time or another throughout world history, the resolution of this question can hardly be, “because that’s just how the world looks.” Likewise with Feynman diagrams, physicists always had choices about what tools to use and what problems to study. Many times during the postwar decades, physicists chose to work with Feynman diagrams even for those problems that—on the face of it—were *least* well-suited to diagrammatic treatment. No “natural fit” argument will do.

Feynman diagrams owed their persistence to broadly shared elements of physicists’ training, which had become standard across the many local groups scattered throughout the world. Most physicists after the war came to draw and teach their Feynman diagrams according to much older pictorial conventions for depicting objects’ paths through space and time. The visual connections with these older techniques, which had become standard fare for physics undergraduates by the early postwar years, help explain why Feynman diagrams stuck: they could be taught in ways that borrowed from more elementary skills that had already become second nature for most young physicists. On top of this, physicists often worked with their Feynman diagrams much the way they worked with other visual representations of particles’ scattering, including the millions of bubble-chamber photographs that flooded the work of the postwar generation of nuclear and particle physicists. Issues of realism and reification, overlaid upon questions of pedagogical inculcation, help explain why physicists chose Feynman diagrams so consistently as their tool of choice.

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Feynman diagrams are clearly artifactual. In this sense, we may speak of them as “constructions,” or even as “social constructions.” Yet to halt the investigation here would mean stopping short: it would do no more to clarify theorists’ treatment of the diagrams than would equating all forms of written expression, from Romantic poetry to the federal tax code, as “social constructions.” Our next task within postconstructivist science studies is to distinguish between kinds of constructions; the means by which constructions make sense for certain scientists in particular times and places; how they spread among large and heterogeneous communities; and how scientists put these constructions to work, generating new ideas or providing a heuristic scaffolding for others. Paper tools and pedagogy offer powerful means with which to tackle these questions.