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ABSTRACT Historians and sociologists have highlighted the importance of skills, local practices and material culture in their studies of experimental sciences. This paper argues that the acquisition and transfer of skills in theoretical sciences should be understood in similar terms. Using the example of Feynman diagrams – first introduced by the US theoretical physicist Richard Feynman in 1948 as an aid for making certain kinds of calculations – we study how physicists in the USA, Japan, and the Soviet Union learned how to use the new tools and put them to work. Something about the diagrammatic tools could be learned from written instructions alone, at a distance from those physicists already 'in the know', although this type of transfer proved to be very difficult, slow, and rare. The rate at which new physicists began to use the diagrams in various settings, and the types of uses to which the diagrams were put, reveal the interplay between geopolitics, personal communication, and pedagogical infrastructures in shaping how paper tools spread.

Keywords paper tools, pedagogy, theoretical physics

Spreading the Tools of Theory:

Feynman Diagrams in the USA, Japan, and the Soviet Union

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When historians and sociologists in recent decades taught us to take skills seriously in the experimental sciences, they most often did so by identifying them as forms of 'tacit knowledge' that belong exclusively to the local practices and material culture of the laboratory. A central contention of these studies has been that experimentalists must work to hone something like artisanal or craft knowledge, in addition to their understanding of general principles. No amount of formal, written instructions will suffice for producing the proper feel for the often recalcitrant instruments that populate the laboratory – this much is surely a familiar refrain by now. Taking our cue from laboratory studies, we want to seize the other end of the ostensive stick, so to speak, and study theoretical calculations as locally acquired tools, rather than found objects. Our calculational tool of choice is the so-called Feynman diagram, introduced by the US theoretical physicist Richard Feynman soon after World War II. Our task is to describe how this seeming epitome of free-floating representational knowledge achieved its initial successes more as a species of learned practice than as

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iterated proof demonstration. We argue that a focus on 'technology transfer' provides a much more plausible account of the historical evolution of theoretical work among diverse groups of physicists, while also helpfully deemphasizing the problematic dyads 'local/experimental/embodied skill' versus 'global/theoretical/disembodied knowledge'.

Recent science studies have accumulated many examples of the need to learn and practice specific skills within the experimental sciences – no student jumps from reading a manual to using an instrument correctly the first time. Several historians and sociologists have argued further that tacit knowledge plays a similar role when it comes to replicating someone else's instruments. 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 stubbornly ineffable – the rules required for actual use remained impossible to specify fully via textual instructions alone. The key to successful replication in each of these cases was through extended personal contact. Only those scientists who worked face-to-face with those already 'in the know' could develop the skills and master the practices necessary to build and use these instruments.[1]

Explication of skills and practices originally served to move laboratory and field studies beyond traditional theory-centered accounts in the history and philosophy of science.[2] Yet these notions can also be extended profitably to the theoretical sciences, as recent work by Andrew Warwick and Ursula Klein demonstrates. In standard conceptual histories, theoretical papers are portrayed as embodying ideas whose content travels easily from theorist to theorist, shorn of the material constraints that might make bubble chambers or electron microscopes (and their associated skills) difficult to carry from place to place. In contrast, Warwick and Klein have introduced terms such as 'theoretical technology' and 'paper tools' to emphasize continuities between work on paper and work at the laboratory bench. They and others have begun to analyze theoretical work in terms of tool-use: theorists must work hard to master locally honed skills with specific theoretical techniques. Theoretical skills are rendered visible not by virtue of any immanent logic in their exercise, but because 'off-the-shelf' sets of mathematical symbols, graphical representations, and even natural language have been put to work in rule-like routines. In other words, work in theory always involves more than cerebral trafficking in globally shared ideas (Olesko, 1991; Krieger, 1992; Pickering & Stephanides, 1992; Warwick, 1992, 1993, 2003; Galison & Warwick, 1998; Klein, 1999, 2001, 2003; Kennefick, 2000).

For our purposes it is not enough to acknowledge evidence of friction in that global traffic, and then trace it back to imperfectly shared worldviews, thereby remaining in the idiom of theoretical contemplation and conceptual frameworks. The metaphor of tool-use points to more explicitly social processes involved in the transfer and replication of theoretical work, and to the piecewise nature of any communal consensus about the effectiveness of a given technique. Yet if we are to speak of local theoretical practices – the mundane business of calculating, drawing diagrams, operating computers, and otherwise manipulating symbols on paper (or computer displays) – we must then ask how they circulate. As in the case of experimental sciences, the skills to operate paper tools do not simply spread of their own accord. Much like the introduction of new scientific instruments, we would expect the circulation of new paper tools to depend upon informal networks of personal communication, networks structured by particular institutional and pedagogical arrangements (cf. Merz, 1998; Ito, 2002). For that reason, our study exhibits much the same concern for the transfer of theoretical skills across geographical distances that we have come to expect for instrument-related skills.

The need for detailed geographic comparison becomes readily apparent in our choice of Feynman diagrams, arguably physicists' most successful paper tool of the postwar era. As we shall see later, Feynman introduced these diagrams in the late 1940s for tackling specific types of calculations in quantum electrodynamics (QED), physicists' quantum-mechanical description of electromagnetic forces. QED had risen to the top of physicists' agendas throughout the world during the 1930s, and attention had returned squarely to the topic soon after World War II. The tasks for which Feynman designed his diagrams were widely acknowledged at the time to be crucial ones. Our task is to reveal the mechanisms behind the diagrams' diaspora in the USA, Japan, and the Soviet Union in the 7 years that followed their introduction. What distinguishes the three cases is the degree of personal contact in the dissemination of Feynman diagrams. In the USA, as discussed later, personal contact and informal mentoring proved crucial to putting the new tool into circulation. Japanese physicists re-established contact with their US colleagues several years into the postwar US occupation of their country, just at the moment that Feynman introduced the diagrams within the USA. Meanwhile, Soviet physicists fell out of contact with their peers in the USA with the hardening of the Cold War, which snapped off all informal communication just months before the diagrams were introduced.

If new techniques could only be replicated with the aid of personal contact and informal, face-to-face training, then we would expect to find very few Feynman diagrams in Japan and no Feynman diagrams whatsoever among the Soviet physicists' publications.[3] On the other hand, if theoretical skills and paper tools could travel with ease by means of written instructions alone, then we would expect to find a roughly equal proportional distribution of diagrammatic papers in every physics community. Instead, we find a set of subtle but instructive contrasts. In the broadest sense Feynman diagrams thrived in Japan, in spite of the limited number of personal contacts that Japanese physicists had with US practitioners. At the same time, the structure of the personal contacts among Japanese theorists led to patterns of use that by no means mapped easily onto US patterns, and do not invite the simple label ‘replication’. In the Soviet Union, no theoretical physicists had extended personal contact with their US counterparts during the early Cold War. The Soviet publications that made use of Feynman diagrams numbered less than 10% of those published in either the US or Japanese journals, and only began to appear fully 3 years after the diagrams in the USA and Japan. The few Soviet papers that made use of the tool demonstrate at once the possibility, in principle, of learning new techniques from written instructions alone, as well as the extraordinary difficulty of doing so in practice. The Soviet case, much like the US and Japanese examples, likewise reveals how important informal training and pedagogical institutions were to distributing the newfound skills throughout the country, once the early working knowledge had been cultivated by a small cohort of theorists.

The Japanese and Soviet uses of Feynman diagrams thus point to a series of related questions: not only whether the new techniques could be learned at a distance, but whether the diagrams and their use could be transmitted by written texts alone. Who picked up the original papers on the diagrammatic tools in the first place, under what conditions, and toward what ends? What struck physicists at the time as the most salient, interesting, or useful features of the diagrams? And how were these appropriations conditioned by the pedagogical institutions in which they were embedded (cf. Warwick, 1992, 1993, 2003)? The contrast between the US, Japanese, and Soviet cases throws into relief the interplay between geopolitics, personal communication, and pedagogical infrastructures in shaping how paper tools spread.

Feynman Diagrams in the USA

Richard Feynman first introduced his diagrams at a private meeting at the Pocono Manor Inn, in rural PA, USA, in the spring of 1948. Twenty-eight theorists had gathered there to talk about outstanding challenges in their field, and QED topped the list. How, for example, might physicists explain at the microscopic level the common phenomenon that opposite electric charges attract each other, while like charges repel? Feynman began with the simplest case: the interactions between two electrons. (An electron is a sub-atomic particle carrying one unit of negative electric charge.) At the quantum-mechanical level, Feynman and his colleagues knew that the electrons would repel each other by shooting force-carrying particles – photons, or quanta of light – back and forth at each other. The challenge physicists had faced since the late 1920s was to find some reliable means with which to keep track of all of the terms in such a calculation. In principle, the electrons could shoot any number of photons back and forth: they could exchange only one photon, two or three photons, sixty-seven thousand photons, three billion photons, and so on. The more photons in the fray, the more complicated the corresponding equations, and yet the full quantum-mechanical calculation depended on tracking each possible scenario and adding up all of the contributions.

For several decades, physicists had known that they could approximate this infinitely complicated calculation, because the charge of the electron is so small: $e^2 \sim 1/137$, in appropriate units. The electrons' charge governed how strong their interactions would be with the force-carrying photons: every time the electrons traded one more photon back and forth, the equations describing the exchange picked up one more factor of this small number, e^2. Thus a scenario in which the electrons traded only one photon would 'weigh in' to the full calculation with the factor e^2, whereas electrons trading two photons would carry the much smaller number e^4 – the latter term would contribute to the full calculation more than 100 times less than the former. The term corresponding to an exchange of three photons would carry the factor e^6 – 10,000 times smaller than the simplest, one-photon-exchange term. Four photons exchanged and the corresponding term would be a mere one-millionth the size of the original term. Although the full calculation extended in principle to include an infinite number of separate contributions – one for each way that the two electrons could exchange any number of photons – in practice any given calculation could be truncated after only a few terms. This was known as a perturbative calculation: physicists could approximate the full answer by keeping only those few terms that made the largest contribution, since all of the additional terms were expected to contribute numerically-insignificant corrections.[4]

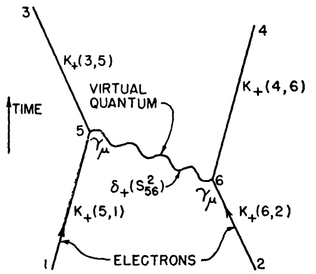
This perturbative scheme appeared simple in the abstract, but was notoriously difficult to implement in practice, for two reasons. First, as physicists bemoaned throughout the 1930s, it was all too common to confuse, or worse to omit, individual terms that contributed to any given order of approximation. For example, there were nine distinct ways in which two electrons could trade two photons back and forth – the photons could cross each other mid-flight; one photon could be reabsorbed before the second was fired off, and so on – and the algebra describing each of these separate cases spilled over two or three lines. Thus there were nine distinct, messy terms that needed to be added together just to describe the two-photon-exchange; the e^4-contribution to the full calculation was itself a morass of equations. The complications grew quickly with each new photon added to the mix: the e^8, or four-photon term, included more than 800 distinct contributions, each of which needed to be written out and added up. Even worse, physicists had faced a second problem when trying to make calculations in QED throughout the interwar period. Many of the terms within these expressions actually diverged to infinity, rather than yielding finite numbers. Most physicists considered the presence of these infinities to be a sign that their underlying theory harbored some fatal flaw: rather than returning finite numbers that could be compared with physical quantities, such as the mass or charge of an electron, calculations within QED, when pushed past the simplest level of approximation, consistently yielded infinities. Whereas the one-photon-term might equal some number such as 0.692, for example, try as they might, theorists could only find infinity for the two- and three-photon terms. Here the problem stood when young theorists such as Feynman returned from wartime service (see especially Schweber, 1994; see also Brown, 1993).

At the Pocono Manor Inn that spring day in 1948, Feynman introduced his diagrams to serve as a bookkeeping device when wading through these complicated calculations. His ultimate goal was to find some way to tame the infinities that kept cropping up. As a step along the way, he wanted first to find a reliable way of making perturbative calculations – to write down the algebraic form for these terms without confusing or omitting elements, before worrying about how to coax the infinities into finite numbers. He designed his diagrams to stand in a one-to-one relation with the mathematical terms he aimed to calculate. He asked his listeners to consider again the simplest way that two electrons could scatter. In this simplest case, one electron could shoot out a force-carrying photon, which would then be absorbed by the other electron. Feynman illustrated this process with the diagram in Figure 1.

This line-drawing, Feynman explained, provided a shorthand way to help calculate the one-photon contribution to the full calculation. The electron on the left had a certain likelihood to move from the point x_1 to x_5, which Feynman abbreviated $K_+(5,1)$; the other electron similarly had a certain likelihood to move from the point x_2 to x_6, hence a factor of $K_+(6,2)$. This second electron could then emit a photon at x_6. The photon itself had a certain likelihood to move from the point x_6 to x_5, which Feynman labeled $\delta_+(s_{56}{}^2)$. Upon reaching the point x_5, the first electron could absorb the photon. The likelihood that an electron would emit or absorb a photon also had a unique mathematical expression, derived from the interwar research, which could be written as $e\gamma_\mu$, where e was the electron's charge and γ_μ 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, much the way a hunter recoils after firing a rifle. 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 diagram stood in for the mathematical expression (itself written in terms of the abbreviations, K_+ and δ_+ [Feynman, 1949b: 771–73]):[5]

FIGURE 1

The simplest Feynman diagram for electron–electron scattering. From Feynman (1949b: 772). Reproduced with kind permission of the American Physical Society.



$$e^2 \iint d^4x_5\, d^4x_6\, K_+(3,5)\, K_+(4,6)\, \gamma_\mu\, \delta_+(s_{56}{}^2)\, \gamma_\mu K_+(5,1)\, K_+(6,2).$$

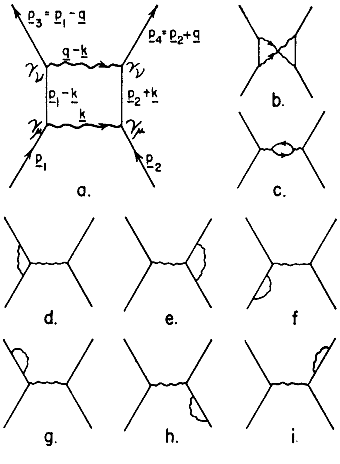
So much for calculating the simplest, one-photon term. As everyone knew at the time, the process in Figure 1 was only the start of the calculation; the two electrons could scatter in all kinds of other ways, trading more and more photons in more and more complicated fashions, and both Feynman and his audience knew that these terms needed to be included as well. Feynman thus used the diagrams as a shorthand way to keep track of these correction terms. The next-simplest processes by which two electrons could scatter – that is, by trading two photons back and forth – corresponded to the diagrams in Figure 2.

For each of these distinct diagrams, in turn, Feynman could write down the associated mathematical contribution – a K_+ for each leg of an electron's motion, a δ_+ for each exchange of a photon, factors of $e\gamma_\mu$ at each vertex where electron and photon lines met, and so on. The key to the diagrams' use, Feynman emphasized, was the unique one-to-one relation between each element of a diagram and each mathematical term in the accompanying equations.

With the aid of these diagrams – to keep the overall calculation on track, each integral built up piecewise from its corresponding diagram – Feynman found a few tricks that could be used to remove the infinities from the two-photon-exchange equations. Simple as the scheme might have appeared to Feynman himself, however, his listeners at the 1948 Pocono meeting had great difficulty following his energetic presentation. Not only did Feynman suffer frequent interruptions from the likes of Niels Bohr, Wolfgang Pauli, Paul Dirac, and Edward Teller, he also eschewed formal rules for manipulating his diagrams in favor of more casual rules of thumb, which he hoped to flesh out via worked examples. The interruptions prevented him from doing so, and Feynman managed only to

FIGURE 2

Feynman diagrams for electron–electron scattering correction terms. From Feynman (1949b: 787). Reproduced with kind permission of the American Physical Society.



further confuse his listeners. By all indications, Feynman's initial presentation of his diagrams was a flop.[6]

A few months later, Freeman Dyson, a graduate student at Cornell (where Feynman was teaching and working out his new diagrammatic scheme) supplied what many people had found missing in Feynman's original presentation. After working closely with Feynman throughout the spring of 1948, the two drove cross-country together that summer, on a trip that afforded Dyson the opportunity to do some sightseeing as well as to plumb more deeply into how Feynman's new techniques were meant to work. After their long drive, the two parted company: Feynman stayed in New Mexico for a few weeks to do some work at Los Alamos, while Dyson made his way by bus to Ann Arbor, MI, for the start of the famous summer school on theoretical physics. The main speaker that summer was Julian Schwinger – like Feynman, one of the young guns of US theoretical physics – who was also then working on QED. Schwinger had worked out his own, non-diagrammatic methods to rid QED of its troublesome infinities, at least in the two-photon term – in fact, before Feynman had said a word at the Pocono meeting, Schwinger had delivered a virtuoso, all-day lecture on his new techniques. His arcane mathematical approach likewise occupied his lectures that summer in Ann Arbor. During the summer school session, Dyson managed to talk several times with Schwinger outside of the lecture hall, learning in more detail about the ins and outs of Schwinger's methods (Dyson, 1979: 53–68; Dyson, in Sykes, 1994: 73–74; Schweber, 1994: chapter 9). Thus by the middle of the summer of 1948, Dyson – and Dyson alone – had spent intense time working side-by-side with both Feynman and Schwinger, learning informally how each of them went about making calculations in QED.

On the bus-ride back to the east coast after the summer school session, Dyson worked out two key results: first, that all of Feynman's relations between diagram elements and mathematical expressions – Feynman's sometimes vague rules of thumb – could be derived rigorously from the foundations of quantum field theory; and second, that Feynman's and Schwinger's very different-looking approaches were in fact mathematically equivalent. By September 1948, Dyson had thus derived and codified the rules that would allow one systematically to translate Feynman's doodles into the mathematical terms of a perturbative calculation within QED.[7] Dyson wrote up his results during the autumn of 1948, as soon as he arrived at the Institute for Advanced Study in Princeton, NJ, where he was to spend the academic year 1948–49. Over the fall term, Dyson worked on a second paper, pushing Feynman's diagrams still further. In his second long and difficult paper, Dyson demonstrated, with the diagrams' aid, that the infinities that had spoiled QED for so long could be removed systematically from any perturbative order. Whereas Feynman and Schwinger had each independently found schemes for removing the infinities from the two-photon term only, Dyson now showed that the infinities could effectively be cancelled out from any arbitrarily complex term involving any number of exchanged photons. Dyson's two papers were the first papers ever published on Feynman diagrams – Feynman's own papers did not appear for several more months. Written as a kind of 'how-to' guide for putting Feynman diagrams to work, Dyson's papers were cited more often during the next decade than Feynman's own pair of papers on the new techniques. Dyson further contributed to putting the diagrams into circulation with an influential set of unpublished, but widely circulating, lecture notes from 1951 (Dyson, 1949a, 1949b, 1951).[8]

Yet even with all of these textual interventions – the pair of papers from 1949, the mimeographed notes from 1951, and several later papers – Dyson's most important contributions to the diagrams' spread came not from his writings, but from his other activities at the Institute for Advanced Study. Dyson arrived at the Institute at a propitious moment: J. Robert Oppenheimer had just become director of the Institute the year before, and immediately upon arrival had decided to re-make the sleepy, tree-lined Institute into a major center for postdoctoral training, especially for young theoretical physicists. Oppenheimer's vision, which he quickly established in practice, was to set up what he called an 'intellectual hotel', inviting young theorists to spend 1 or 2 years in residence, where they could work intensely and talk informally with their peers, outside the demands of teaching or administrative duties. The year that Dyson arrived, Oppenheimer had boosted the enrollments of postdoctoral theorists by 60% – an expansion that Oppenheimer continued to push through in following years, despite strenuous objections from many of the Institute's otherwise-powerful senior faculty. Oppenheimer explained his rationale to a reporter from *Time* magazine soon after he arrived at the Institute: 'the best way to send information is to wrap it up in a person' (Anonymous, 1948: 81).[9]

Thus, when Dyson arrived at the Institute for Advanced Study in September 1948, Feynman diagrams in hand, he immediately joined ten fellow 'postdocs' in theoretical physics, the largest cohort yet. In fact, the new building that had been planned to hold offices for the newly expanded ranks had not been completed on time, so all of the postdocs wound up sharing desks in one large room for the first 6 weeks of their stay – an architectural arrangement perfectly suited for the sharing of tacit knowledge. They huddled around large tables swapping ideas, pressing Dyson to explain the details of how to use Feynman diagrams. He delivered several sets of long lectures to the group, supplemented by constant, informal conversations. By the winter of 1948–49, Dyson had organized them into several groups, each working on distinct diagrammatic calculations. One pair, Kenneth Watson and Joseph Lepore, worked with Dyson to calculate high-order terms in a certain model of nuclear forces, while another duo, Norman Kroll and Robert Karplus, took up Dyson's lead to undertake similarly complex diagrammatic calculations in QED. So effective were Dyson's tutorials that Wolfgang Pauli wrote to another of the young Institute theorists that May, asking what Dyson and the rest of the 'Feynman-school' were working on.[10]

Quickly a pattern became established: postdocs circulated into the Institute for Advanced Study for 1- or 2-year stays, learned the details of how to use the diagrams either from Dyson himself or from other members of the diagrammatic circle in residence there, and then took up teaching jobs elsewhere. Wherever the former postdocs went, they drilled their graduate students in how to use the new techniques, assigning them on problem sets and aiding in their inclusion within dissertations. Surviving lecture notes from the period give some flavor of the new recruits' efforts in their training mission. Often the problems they assigned involved little more than drawing the appropriate Feynman diagrams for a given physical situation – not even translating them piece-by-piece into their accompanying equations. Which types of lines to use (solid, dashed, wavy), how they should meet at vertices, which lines should be inclined at an angle, which lines should carry arrows and which remain unadorned – all these pictorial conventions took center stage. As the Institute postdocs had learned from Dyson, so too would their students learn from them: from now on, calculations would begin with the diagrams.[11]

A steady stream of diagrammatic papers followed as the postdoc cascade unfolded: Norman Kroll to Columbia; Fritz Rohrlich to Cornell, Princeton, and later the State University of Iowa; Kenneth Watson to Berkeley, Indiana and Madison; Robert Karplus to Harvard and Berkeley; Donald Yennie to Stanford, and so on. Where no member of the expanding diagrammatic circle landed, virtually no physicists picked up the diagrams.[12] The Institute postdoc cascade proved remarkably efficient: the number of papers in the *Physical Review* that made use of Feynman diagrams grew exponentially, doubling every 2.2 years. Between 1949 and 54, the journal published 139 diagrammatic papers, submitted by 114 authors; by 1954, each biweekly issue included two diagrammatic installments, on average. More than four-fifths of all the papers in the *Physical Review* that made use of Feynman diagrams during this period came either from these Institute postdocs or from their students; nearly all the rest came from physicists who worked directly with Feynman. (Only two of the 114 physicists who published on Feynman diagrams in the *Physical Review* during this period did so with no discernible contact with other diagram users.) Nearly the only people who picked up the diagrams were young theorists, still in the midst of their training: 37% as graduate students and 45% as postdocs. Another 10% began using the diagrams while still instructors or assistant professors, less than 7 years past their doctorates – and nearly all the members of this 'older' group likewise picked up the new techniques only after working closely with one of the dispersed Institute postdocs. Physicists needed to practice using Feynman diagrams; older physicists simply did not re-tool.

Personal contact and informal mentoring thus proved crucial for spreading the diagrams to physicists working in the USA. One of the earliest converts to the new methods, Fritz Rohrlich – who had been among the original group of postdocs clustered around Dyson at the Institute for Advanced Study during the 1948–49 academic year – counseled the physics department chair at the University of Pennsylvania that without any of the diagrammatic cognoscenti in town, the department's graduate students would either have to select different dissertation topics, or simply drop out of graduate school and wait until the department had hired someone who could teach them how to use the new diagrams in person.[13] Graduate students elsewhere experienced similar difficulties when they tried to learn about Feynman diagrams from texts alone, without the aid of one of the dispersed Institute postdocs. Henry Stapp, for example, read Feynman's and Dyson's papers as carefully as he could when he began his graduate studies at Berkeley, and quickly got his hands on Dyson's mimeographed lecture notes from 1951 as well. Yet try as he might, he simply could not pick up the new techniques from these textual sources, and he wound up selecting a different research topic for his dissertation.[14]

Feynman Diagrams in Japan

Tomonaga's Tokyo Group

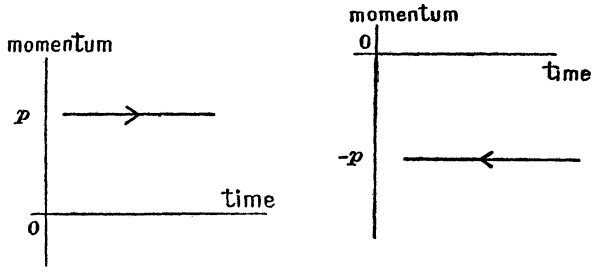
When word of the new Feynman diagram techniques reached Japan early in 1949, an active community surrounding Tomonaga Sin-itiro was already well primed to receive the news and act on it. In the spring of 1946, Tomonaga began to rally an active group of young theorists. Breaking with the long-standing tradition of mutual isolation between imperial universities and all other institutions, the group drew members from several schools in the Tokyo area – students and researchers from Tokyo University began to converse freely with members from the Tokyo Bunrika Daigaku (Tokyo Education University, where Tomonaga worked) and elsewhere. During the war, Tomonaga had worked out many elements of his own formalism for QED. Now his rag-tag group of young followers set to work extending his formalism and applying it to problems in both QED and in studies of nuclear interactions. Some weeks, the group gathered in his Quonset hut – which doubled as Tomonaga's makeshift office and residence – to talk about some member's recent calculations or to arrange further collaborations on new projects; at other times, they entered into intense discussions of a recently published paper. Other weeks, the students worked together to make sense of sections of Walter Heitler's pre-war textbook, *Quantum Theory of Radiation* (1936), with Tomonaga's aid, to keep calculation-skills sharp. By all accounts, the Tokyo group was informal yet spirited: they threw themselves into their physics, despite – or because of – the lasting material deprivations all around them (Brown & Nambu, 1998; Hayakawa, 1988; Kinoshita, 1988; Nambu, 1988; Schweber, 1994: chapter 6).

Tomonaga's circle logged many hours in the Tokyo Civil Information and Education (CIE) library, established in November 1945 by the US occupation authorities. The CIE libraries – branch locations were opened throughout Japan beginning in August 1947 – stocked copies of the *Physical Review* alongside more mainstream US periodicals, such as *Time* and *Newsweek* (Nakayama, 2001b: 238–39). During the summer of 1947, Tomonaga's students noticed a brief announcement in *Newsweek* of Willis

Lamb's high-precision measurement of an energy-level splitting within hydrogen atoms, and they realized – just as surely as the US physicists who heard Lamb's news directly – that the effect might be explainable in terms of perturbative corrections within QED. Their suspicions were confirmed 2 months later when they read (again in the CIE library) Hans Bethe's *Physical Review* paper, in which he examined the Lamb shift within a non-relativistic approximation (Bethe 1947). The news pushed Tomonaga's circle even more fervently to seek ways of using Tomonaga's formal approach to QED to tackle the meddlesome infinities. Although neither side knew it yet, Tomonaga's entire approach, right down to his notation and formalism, showed remarkable resemblance with Julian Schwinger's emerging work.[15] In one of the earliest packages to be sent overseas after the ban on international mail had been lifted – no international mail could be sent by Japanese citizens under the US occupation until the spring of 1948 – Tomonaga sent a series of papers and manuscripts to Oppenheimer, who received them at the Institute for Advanced Study as soon as he returned from the Pocono conference (at which Schwinger had dazzled his listeners with his day-long lecture, after which Feynman had haltingly introduced his diagrams). Oppenheimer immediately arranged to have a brief report of Tomonaga's group's work published in the *Physical Review*.[16]

In the course of these intense and fast-moving developments, the Tokyo group focused on how to make better, more efficient and more reliable perturbative calculations. These tasks took on a certain immediacy thanks to competition from other groups, both in Japan and in the USA.[17] Given the importance of identifying and including all possible contributions to a given perturbative order (and having received no news as yet about the brand-new Feynman diagrams, at the time still being developed by Feynman and Dyson), Tomonaga's group began tinkering with their own graphical means of keeping track of the necessary terms. In the spring of 1948, Tomonaga and his student Koba Zirô initiated a text-based notation, using arrows to denote various possible processes (Koba & Tomonaga, 1948a, 1948b). Yet soon even these arrows proved insufficient. Six months later, Koba and Takeda Gyô, another young member of Tomonaga's circle, submitted a long paper, published in two installments. The perturbative terms they now sought to calculate had become so intricate that they turned to a new form of graphical means – they called them 'transition diagrams' – to try to keep them all straight (Koba & Takeda, 1949a: 61). Whereas Feynman's and Dyson's diagrams plotted particles' positions in space and time, Koba and Takeda mapped particles' momentum over time. An electron with momentum p would therefore be represented by a horizontal line at a height p with an arrow moving to the right. A positron (the anti-matter cousin to the electron, having the same mass and opposite electric charge) with momentum p would be represented by a horizontal line with a height $-p$ with an arrow moving to the left (Koba & Takeda, 1949a: 61)[18] (see Figure 3).

FIGURE 3
Transition diagrams. Reproduced from Koba & Takeda (1949a: 62), with kind permission of the Japanese Physical Society.

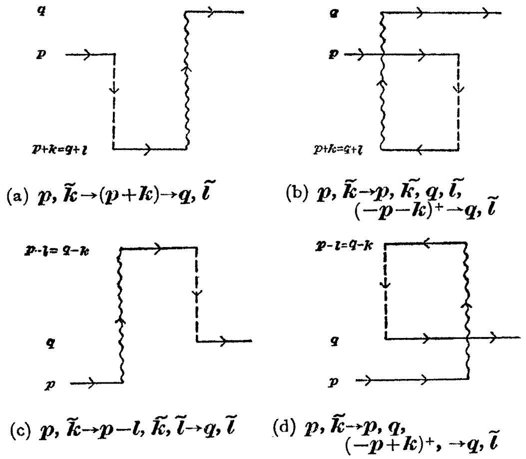


(a) electron p (b) positron p^+

The emission of photons would be represented by wavy vertical lines connecting the two momentum states of an electron; the absorption of photons would appear as dashed vertical lines between the electron's two momentum states. They illustrated their technique by considering the four distinct ways in which Compton scattering – the scattering between an electron and a photon – could occur in the lowest approximation (see Figure 4).

Many young members of Tomonaga's group quickly recognized the usefulness of such simple line-drawings. Nambu Yoichiro, who shared an office (and hence makeshift living quarters) with Koba at the time, submitted a paper just 3 weeks after Koba and Takeda wrote up their transition diagrams work, making use of a similar graphical scheme for his own perturbative calculations (Nambu, 1949).[19] At last, by October 1948,

FIGURE 4
Compton scattering, as illustrated by transition diagrams. Reproduced from Koba and Takeda (1949a: 64), with kind permission of the Japanese Physical Society.



(a) $p, \tilde{k} \to (p+k) \to q, \tilde{l}$

(b) $p, \tilde{k} \to p, \tilde{k}, q, \tilde{l}, (-p-k)^+ \to q, \tilde{l}$

(c) $p, \tilde{k} \to p-l, \tilde{k}, \tilde{l} \to q, \tilde{l}$

(d) $p, \tilde{k} \to p, q, (-p+k)^+, \to q, \tilde{l}$

members of the Tokyo group seemed to have a reliable method for pursuing perturbative calculations.

Enter Feynman Diagrams

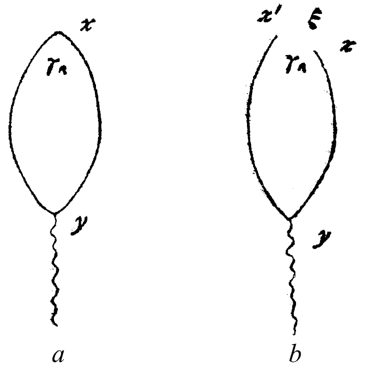
Yet Koba and Takeda's transition diagrams were not to last. In a note added in proof soon before their final installment appeared in the April/June 1949 issue of the new Japanese physics journal, *Progress of Theoretical Physics*, they announced that they had just read a paper in the *Physical Review* by F.J. Dyson (Dyson, 1949a). Dyson's paper introduced, as they noted, 'diagrams in ordinary space-time, and we hope our momentum-diagram will act as an intermediary between the formalism' of Dyson's paper 'and the conventional perturbation calculation. Judging from Dyson's work, Feynman's radiation theory, of which we know very little, seems also to employ some diagram method' (Koba & Takeda, 1949a: 141). The paper by Dyson to which they referred was the first paper ever to appear on the diagrams; it was submitted to the *Physical Review* in October 1948 and published in February 1949. (In fact, Dyson's paper was received at the *Physical Review* on 6 October 1948, while Koba and Takeda's paper on transition diagrams was received at *Progress of Theoretical Physics* on 4 October 1948.) The similarity between Koba and Takeda's transition diagrams and Dyson's reports of Feynman diagrams had not been lost on the Tokyo theorists; nor was it lost on Dyson himself. Dyson added a brief footnote to his own paper just before it went to press, after he read a short Letter to the Editor in the Japanese journal by Koba and Takeda that had described (in words only) some of the rudimentary elements of their transition-diagram scheme (Dyson, 1949a: 486–87, referring to Koba & Takeda, 1948).[20]

Only a little news of Dyson's work had trickled into Japan before his first paper was published. The first Japanese scientist allowed to travel outside the country after the end of the war was the senior physicist Yukawa Hideki; Oppenheimer interceded directly with General Douglas MacArthur to allow Yukawa to visit the Institute for Advanced Study, beginning in September 1948. Yukawa thus arrived at the Institute at the same time as Dyson. He immediately began sending reports to his colleagues in Japan, many of which were quickly published in a new informal newsletter, *Soryûshi-ron kenkyû* (*Studies in Elementary Particle Theory*), which was founded in October 1948. One month after he arrived in Princeton, for example, Yukawa wrote that 'Here, Feyman's [sic] theory is popular among young people'. Yukawa translated what he had heard about Feynman's work – by way of Dyson's informal lectures at the Institute – into the familiar formalism of his Japanese colleagues.[21] Yet Dyson did not prepare any preprints of his first diagrammatic paper, written up so hastily upon his arrival at the Institute. Only after Dyson's first paper appeared in print (and after copies of the 1 February 1949 issue of the *Physical Review* arrived in Japan) did Tomonaga's group learn more of what Dyson had done. If Yukawa's early reports had not sufficiently stirred interest, the title of Dyson's paper probably would have caught the Tokyo group's attention: he announced his work under the rubric, 'The Radiation Theories of Tomonaga, Schwinger, and Feynman', and demonstrated the formal equivalence between Tomonaga's and Schwinger's approach with Feynman's still-unpublished methods. Around the same time, Yukawa sent a preprint of Dyson's second paper, which had become available in late January (Dyson, 1949b). Immediately, Tomonaga's group threw themselves into concerted study of Dyson's work (Kinoshita, 1988: 9; Nambu, 1988: 5).

Within months, Tomonaga's group began making use of Feynman's diagrams, based on what they had learned from Dyson's papers. Already primed by Koba and Takeda's work to appreciate a graphical method that could distinguish among complicated contributions to their perturbative calculations, they scrutinized Dyson's work with special care.[22] Even with all of the close scrutiny of Dyson's papers, however, and the additional news from Yukawa, the Tokyo theorists' initial appropriation of Feynman diagrams revealed several subtle differences from Dyson's own work. Consider, for example, Takeda's 1949 Letter to the Editor, which became the first published use of Feynman diagrams in *Progress of Theoretical Physics*. He returned to a familiar problem within QED: the infinities that arose in calculations from the incessant popping into and out of existence of virtual electron-positron pairs, a process that had been dubbed 'vacuum polarization'.[23] 'According to Feynman–Dyson's theory', Takeda briskly explained, one could write an expression for the vacuum polarization in the lowest perturbative order of approximation, based on a single simple Feynman diagram (see Figure 5a).

Next to this diagram, Takeda included a second diagram (Figure 5b), explaining that 'This just means to adopt an open loop having a mouth with small breadth Z_μ instead of a closed one'. He duly wrote down a modified mathematical expression for the vacuum polarization, taking into

FIGURE 5
(a), Feynman diagram for the lowest-order contribution to vacuum polarization; (b), Takeda's modified diagram for the same process. Reproduced from Takeda (1949: 573), with kind permission of the Japanese Physical Society.



account the 'open mouth' of his new diagram (Takeda, 1949: 573–74).[24] Takeda's diagrammatic maneuver here is striking, and no doubt would have appeared bizarre to Dyson or the other Institute postdocs. To Dyson, there was no such thing as an 'opened' closed loop. The diagram in Figure 5b no longer depicted virtual particles, precisely because the solid electron and positron lines no longer met at a single point. In Dyson's scheme, if the two lines never rejoined at a single point, that meant that the two virtual particles never repaid the energy they had borrowed, according to the Heisenberg uncertainty principle. To one steeped in Dyson's methods, in other words, Figure 5b depicted the interaction between a photon (wavy line) and two real electrons (solid lines), rather than a photon encountering a virtual electron-positron pair. As such, Figure 5b had nothing at all to do with vacuum polarization. Within Dyson's formalism, Takeda's accompanying mathematical expression for Figure 5b was therefore neither correct nor even relevant, since the diagram contributed to a wholly separate physical process.

The next paper in *Progress of Theoretical Physics* to make use of Feynman diagrams signaled a similar discomfiture with the new techniques. Fukuda Hiroshi and Miyamoto Yoneji, both working at Tokyo University, pressed Feynman diagrams into service for their study of various particle-decay processes. They worked beyond the lowest perturbative order, including the effects of the first round of perturbative corrections. Although they worked beyond lowest order, however, they did not begin their calculation with Feynman diagrams and use Dyson's rules to translate each diagram step-by-step into a mathematical expression. Instead, the young Tokyo theorists wrote out all of the relevant integrals in full – using Tomonaga's formalism, without the aid of any diagrams – and only later 'illustrated' the various complicated integrals with associated Feynman diagrams. They altogether ignored Dyson's formal rules for translating Feynman diagrams into equations (and thereby avoided some of Takeda's difficulties), choosing instead to write down the expressions the best way they knew how (Fukuda & Miyamoto, 1950).[25] Feynman diagrams had entered the Tokyo theorists' purview, but had hardly edged out their native routine.

The first papers published in *Progress of Theoretical Physics* that put Feynman diagrams to use in a recognizably Dysonian fashion came from Kinoshita Tôichirô. Kinoshita was already well versed in how to calculate using Tomonaga's formalism, as well as the pitfalls of attempting to perform complicated perturbative calculations without additional bookkeeping aids. In his earliest published work, submitted to the Japanese journal in July 1948, he had studied perturbative corrections to an electron's scattering in an external electromagnetic potential. Eight months later, he and his collaborators had to retract several of their printed conclusions upon realizing that they had overlooked an important term in their perturbative calculation. By April 1949, Kinoshita and his collaborators had thus re-done their full calculation with the aid of Koba and Tomonaga's arrow notation (Endô et al., 1948, 1949a, 1949b).[26] Just as the corrected calculation was sent off to the journal, Dyson's first paper arrived in Japan. As Kinoshita later recalled, he immediately made a careful study of it, and 'volunteered to present Dyson's paper at [Tomonaga's Tokyo] seminar, which quickly turned into a very lively free-for-all' (Kinoshita, 1988: 9). In preparing for his presentation before such an alert and vigorous group – and having himself just recently been chastened by the difficulties of perturbative calculations – Kinoshita probably scrutinized Dyson's paper even more closely than most of his peers had yet done. Nine solid months of work later, Kinoshita's diligence began to pay off, as he applied Feynman diagrams and Dyson's calculational techniques to a series of problems (Kinoshita, 1950a, 1950b).[27]

Soon theorists in Tokyo had more than just Kinoshita's example to guide them in their incorporation of Feynman diagrams and Dyson's associated techniques. On Oppenheimer's personal invitation, Tomonaga spent the academic year 1949–50 in residence at the Institute for Advanced Study. While in the USA, Tomonaga had the opportunity to learn first-hand about the fast-breaking developments. He heard Schwinger lecture several times, listened to Dyson, and learned in greater detail, month-by-month, some of the ins and outs of making calculations with Feynman diagrams – all in much greater detail than had been conveyed by Dyson's publications alone. While he was in Princeton, Tomonaga sent regular updates to his students and colleagues back in Japan, many of which were published in the mimeographed newsletter, *Soryûshi-ron kenkyû*, just as Yukawa's earlier letters had been.[28]

Dispersing the Diagrams throughout Japan

Over time, Feynman diagrams became a steady fixture of the Tokyo theorists' calculations and publications. Between 1949 and 54, 107 physicists working in Japan published a total of 97 diagrammatic papers in *Progress of Theoretical Physics*, with an additional 66 preprints appearing in *Soryûshi-ron kenkyû* between 1949 and 1952. Half of these authors were young theorists trained in Tomonaga's Tokyo seminar; their contributions accounted for more than half of the diagrammatic papers. The shared research topic, motivation, and tight-knit group structure (bolstered by Tomonaga's extended visit to the Institute for Advanced Study and his return to Tokyo in the summer of 1950) help explain why Tomonaga's group eventually mastered the new diagrammatic techniques and made such regular use of them. What about the scores of Japanese theorists working outside Tomonaga's circle? By 1954, papers that incorporated Feynman diagrams had been published by physicists working throughout Japan: all the way from Hokkaido in the north, to Osaka, Kyoto, Kanazawa, Hiroshima, Wakayama, and beyond. Hence we are faced with the same question as for physicists working in the USA: how did an esoteric technique, which had proven neither obvious nor easy to master upon first viewing, spread so quickly?

The first thing to note about the physicists in Japan who began to use Feynman diagrams is that, like their counterparts in the USA, they were almost exclusively young theorists. Their average time between obtaining undergraduate degrees and DSc degrees was 10.6 years; this period corresponds roughly to US physicists' graduate school and postdoctoral stages.[29] On average, the Japanese theorists began to use Feynman diagrams right in the middle of their post-undergraduate training: 4.4 years after obtaining their undergraduate degrees, just over 6 years before obtaining their DSc. Only three authors in Japan published their first diagrammatic papers after obtaining their DSc (Miyazima Tatsuoki, Husimi Kôdi, and Nakabayasi Kugao), and two more wrote their first diagrammatic papers in the same year as obtaining their DSc (Taketani Mitsuo and Utiyama Ryôyû). All five of these physicists began using Feynman diagrams while collaborating with younger diagram-users from Tokyo, or with colleagues who themselves had been tutored by the Tokyo group. All the rest first picked up the diagrams as young theorists, still in the midst of their training. As in the USA, the new techniques required extended practice, and older physicists simply did not re-tool.[30]

Consider next where the diagrams spread, and by what means. Nearly every diagrammatic paper published in Japan between 1949 and 54 can be traced back to extended personal communication with members of Tomonaga's circle. Three novel institutional rearrangements, all taking shape in 1948–50 – just when Feynman diagrams were introduced – helped to facilitate this communication with the Tokyo group, and hence to put the diagrams into broad circulation throughout Japan. First, the Elementary Particle Theory Group, founded in 1948, brought Japanese theorists together for regular, informal meetings, supplemented by the group's new newsletter, *Soryûshi-ron kenkyû.* The meetings facilitated collaborations extending between members of different universities. Tokyo theorists began to co-author diagrammatic papers with physicists at several other universities, including Osaka, Hokkaido, Shiga, Kyushu, and Wakayama universities; in each case, these papers were the first from physicists in any of these universities to make use of the diagrams. Acknowledgments in many other diagrammatic papers, written by theorists not working in Tokyo, further noted help received from the Tokyo group (Nakano et al., 1950; Matsumoto et al., 1951; Tokuoka & Tanaka, 1952).[31]

Second, the Elementary Particle Theory Group, the Japanese Ministry of Education, and some private companies introduced new fellowships to allow young physicists to spend time at different universities within Japan, in order to learn in person about new techniques or to polish older ones (Ôneda, 1988: 17; Kamefuchi, 1988: 128). Umezawa Hiroomi, for example, completed his undergraduate degree at Nagoya in 1947, working with Sakata Shôichi. He remained in Nagoya, where he quickly assumed prominence in Sakata's group. He became a prolific author, and advised several younger members of Sakata's group, obtaining his DSc from Nagoya in 1952. Yet he only began to make use of Feynman diagrams in December 1950: he won a traveling fellowship from the Chubu-Nippon

Press (based in Nagoya), which allowed him to spend the autumn 1950 semester in Tokyo. Since his parents lived in Tokyo, he was able to keep his costs low, so he used his fellowship to bring a younger Nagoya student, Kamefuchi Susumu, with him. Together they attended 'the famous Tomonaga Seminar that was taking place at a shabby building' in the city, as Kamefuchi later recalled. Thus they worked with Tomonaga's group for several months immediately after Tomonaga himself had returned from the Institute for Advanced Study. At the end of the semester, Umezawa and Kamefuchi returned home to Nagoya (Kamefuchi, 1988: 128; 2001: 10). By December 1950, Umezawa had added Feynman diagrams to his arsenal of calculating techniques, making steady use of the diagrams for the remainder of his career. Kamefuchi likewise turned to the diagrams once he began to publish some of his early research in *Soryûshi-ron kenkyû* and *Progress of Theoretical Physics*. Only after Umezawa returned to Nagoya and resumed his advising duties for many of Sakata's students did Feynman diagrams become a regular fixture of the Nagoya group's publications. By May 1952, Umezawa's influence had extended to neighboring universities: Goto Shigeo, working at nearby Gifu University, thanked Umezawa for 'valuable discussions' in Goto's first diagrammatic paper, the first such paper to appear from anyone at Gifu.[32]

Third, beginning in 1949, the US occupation authority's General Headquarters oversaw a tremendous expansion of the Japanese university system. The US authorities stipulated that there should be at least one national university in each prefecture of Japan, in imitation of the state-university system within the USA – a decree aimed at weakening the monopoly of the older imperial universities (Kaneseki, 1974: 230–32; Nakayama, 2001a: 37–39). The General Headquarters decree very quickly increased the circulation of Feynman diagram-users throughout Japan. With the creation of so many new universities came many new jobs for young physicists; the older pattern of Japanese academic life, in which professors almost always remained at the same universities in which they had been trained, began to break down. Ozaki Shoji and Ôneda Sadao, who had first learned about the diagrams from several Tokyo theorists late in 1949, moved to the newly-created Kanazawa University during the summer of 1950, and there began to train new recruits (Ôneda, 1988: 15–17). Takeda Gyô moved from Tokyo to Kobe University, where he continued to publish diagrammatic works.[33] But by far the most important shift came from young theorists moving from Tomonaga's Tokyo group to Osaka. Osaka City University became one of the earliest of the new universities built upon the General Headquarters plan, and it quickly became home to several of the Tokyo group's most prominent and prolific diagram-enthusiasts, including Nambu Yoichiro, Koba Zirô, Hayakawa Satio, Nishijima Kazuhiko, and Yamaguchi Yoshio. Fresh from their work with Tomonaga, these theorists submitted 12 diagrammatic papers to *Progress of Theoretical Physics* and fourteen additional reports to *Soryûshi-ron kenkyû* by the end of 1952. With their help, 11 other young theorists at

Osaka University and Osaka City University began to publish diagrammatic papers, beginning in May 1951. Thanks to the Tokyo transplants, nearly 30% of all the papers in *Progress of Theoretical Physics* and *Soryûshi-ron kenkyû* making use of Feynman diagrams between 1949 and 1954 appeared with Osaka by-lines. The Tokyo-turned-Osaka theorists likewise began to help theorists in neighboring Kyoto get up to speed with the new techniques (Katayama, 1950; Kita & Munakata, 1950).

By the early 1950s, the circulation of young theorists within Japan was supplemented by a series of personal exchanges between Japan and the USA. Younger theorists began to make the trip that only their elders Yukawa and Tomonaga had made before. Hayakawa Satio traveled to the Massachussetts Institute of Technology (MIT) and Cornell during 1950–51, sending regular updates in to *Soryûshi-ron kenkyû*; Nambu and Kinoshita both took up residence at the Institute for Advanced Study in 1952. The trips were revealing for both travelers and hosts: writing soon after his arrival in Boston, Hayakawa observed to a Japanese colleague that 'Schwinger is just Tomonaga's copy. Only he is strong. After all, Japan is the best in field theory, and in the US, only Feynman can match Japan'.[34] The following year, physicists from both countries had a chance to make such evaluations on their own, when Japan hosted its first international physics conference since the end of the war. Feynman, Murray Gell-Mann, Robert Marshak, Abraham Pais and dozens of their diagram-wielding colleagues from the USA traveled to Kyoto for the meeting, further solidifying what had already become a robust community of diagram users (Konuma, 1988: 25).[35]

Feynman Diagrams in the Soviet Union

Timing is Everything

Just as in Japan, international politics shaped the dispersion of Feynman diagrams in the Soviet Union. With the Soviet Union, the exigencies of world affairs cut at the same time, but the other way: immediately before Feynman diagrams were introduced, the Cold War made it impossible for physicists in the USA and the Soviet Union to exchange ideas in person or to discuss techniques informally. The separation between physicists in the USA and the Soviet Union was not new with the coming of the Cold War, although Soviet physicists' hopes had been raised at the end of World War II that a new era of international scientific exchanges might be opening. An international meeting was held in 1945 to celebrate the 220th anniversary of the Soviet (formerly Imperial) Academy of Sciences, with wide, positive news coverage throughout the West. Top Soviet theorists lobbied optimistically for an international conference to be held in 1947, but their plans were shelved as US–Soviet antagonisms grew during the course of 1946. Renewed calls went out from Party bureaucrats and Academy philosophers to purge physics of its over-dependence on Western sources. Several leading Soviet theorists were publicly upbraided for 'groveling before the West', for 'uncritically receiving Western physical theories and propagandizing them in our country', and for producing insufficiently Soviet textbooks. A meeting had been planned for mid-March 1949 – just 1 month after Dyson's first paper on Feynman diagrams had appeared in the *Physical Review* (Dyson, 1949a) – to carry these denunciations further and to 'clean house' in Soviet physics (Holloway, 1994: 26–28; Sonin, 1995; Kojevnikov, 1996: 43–48; Hall, 1999: 705–14).

The isolation that was so harshly re-imposed upon Soviet physicists by their government in the late 1940s deprived them of any chance for informal, face-to-face exchanges with physicists on the other side of the Iron Curtain. Because of this postwar isolation, Soviet theorists did not pick up and use Feynman diagrams in anything like the ways their colleagues in the USA and Japan did. Whereas young theorists (beyond Feynman and Dyson themselves) in these other countries had begun using the new techniques in print as early as autumn 1949, the first published uses of Feynman diagrams in the Soviet Union did not appear until 1952. Only 11 authors put the diagrams to use in the main Soviet journal between 1952 and 1954, publishing a total of 12 papers – a far cry from the exponentially rising flood of diagrammatic papers then filling journals such as the *Physical Review* and *Progress of Theoretical Physics*.

One might suspect that difficulties in receiving copies of the *Physical Review* led to Feynman diagrams' delayed appearance within Soviet publications.[36] Yet difficulties receiving the *Physical Review* cannot be the entire story behind the diagrams' relative absence. In fact, officers of the American Institute of Physics made special efforts to distribute copies of the *Physical Review* inside the Soviet Union, as their internal memoranda reveal, thinking that their flagship journal would contribute positive propaganda if nothing else. Official copies mingled with cheap pirated editions, which the Soviet government clandestinely produced before signing international copyright statutes. Soviet papers in the *Zhurnal eksperimental'noi i teoreticheskoi fiziki* (*Journal of Experimental and Theoretical Physics*) often cited the most up-to-date publications from the *Physical Review*.[37] The lack of diagram-usage thus was not due solely to any unusual delays in receiving Feynman's or Dyson's diagrammatic papers. At least some Soviet theorists did receive copies of these written instruction manuals in a timely fashion, and they were able to obtain them promptly precisely because they had the considerable resources of a classified research regime working for their benefit. But this came at the cost of enforced isolation from more casual research ties. What was lacking in the Soviet case was not written texts or formal publications, but the ancillary institutional support for informal, pedagogical exchange between physicists already 'in the know' about the new tools and those who had not yet learned how to use them.

From the H-bomb to Landau's Seminar

Deeply ensconced in the secret Soviet nuclear weapons program, Feynman diagrams made their initial, if halting, entry into Soviet physics. A small team of young theorists, including Vladimir Berestetskii, Aleksei Galanin, Boris Ioffe, and Aleksei Rudik – all disciples of Isaak Pomeranchuk in Moscow – were assigned to work on a specific task for the H-bomb project in the summer of 1950. In order to predict whether certain H-bomb designs could work in principle, the Soviet theorists needed to know how to calculate the flow of energy that would be carried away from the fusion reaction region by out-flying radiation. The escaping radiation would carry energy away from the interaction region, thereby cooling it down; the question was whether or not the radiation would carry energy away too quickly, robbing the thermonuclear reaction of its required heat and causing the bomb to fizzle. Unlike the details of the fusion reaction itself, this part of the calculation depended only on knowing how to calculate the scattering between high-energy photons (the emitted radiation) and electrons (in the material surrounding the fusion-reaction region). This was a problem in electrodynamics, not nuclear physics. Yakov Zel'dovich, famous for his rough-and-ready, back-of-the-envelope approach to calculation, had given a rough estimate for this radiation scattering, which was only trustworthy to within a factor of two or so. Yet the bomb-designers knew that this crucial quantity had to be calculated to a much higher accuracy – with an uncertainty of a few percent, at most – since the entire question of whether or not the H-bomb design would work hung on this delicate balancing of the energy. So Pomeranchuk's young charges were instructed to find some way to make this calculation in a more precise way (Ioffe, 2001: 25–28).[38]

Pomeranchuk had noticed the recently published papers on the new approach to QED by Schwinger, Feynman, and Dyson, but had not yet mastered the new techniques. He pointed his protégés to these works, and they scoured the papers intensely. One of the worked examples that Feynman included as an appendix to his long paper involved using the diagrams to calculate perturbative corrections to Compton scattering, corrections that would enter at the percentage level. This was precisely the calculation and the level of accuracy that Berestetskii, Galanin, Ioffe, and Rudik now needed to master, and in a hurry. A few weeks into their work, they learned to their surprise that Rudik had not been cleared for the top-secret work. The remaining three theorists threw themselves into trying to make sense of the new calculating techniques, based on Feynman's and Dyson's papers alone. After more than 1 year of full-time effort, they succeeded. They worked so hard to try to understand the ins and outs of the papers, in fact, that Galanin and Ioffe even made their own translations of the papers into Russian, hoping that the line-by-line scrutiny of the papers required for translation would pay dividends in their mastery of the diagrammatic tools. Berestetskii's close study of the papers similarly bore fruit in a lengthy review paper published in 1952 (Ioffe, 2001: 25–28).[39] With time and intense effort, in isolation, these three theorists learned how to put Feynman diagrams to work in the context of their assigned task. Just at the time that leading Soviet theorists were being lambasted in public for their narrow dependence upon Western physics, Pomeranchuk's students, under cover of the high-priority H-bomb program, smuggled in the new techniques and put them to work.

The details of Ioffe's, Galanin's, and Berestetskii's calculations, along with the specific motivation for undertaking them, were 'born secret', hardly the stuff they could discuss openly with colleagues in the Soviet Union, let alone with colleagues outside the country (Ioffe, 2001: 28–29).Yet the young theorists knew that the new diagrammatic techniques they had worked so hard to acquire had utility and interest far beyond this specific military application. As they were wrapping up their H-bomb calculations, Galanin published three long papers that made use of the new techniques (absent the original motivation for practicing how to use them), which he submitted to the *Zhurnal* beginning in late August 1951. His first three installments – the earliest publications in the country to make any use of the diagrams – presented many of the fruits of his group's close readings of Feynman's and (especially) Dyson's papers. Galanin's first paper – the longest of the three – tackled the exact same calculation that Dyson himself had used for introducing the diagrams in his own first paper: corrections to an electron's behavior in an external electromagnetic field. Galanin followed nearly all of Dyson's prescriptions carefully, including how to draw the Feynman diagrams themselves (Galanin, 1952a)[40] (see Figure 6).

Galanin's second paper applied the new formalism to the very problem that he, Ioffe and Berestetskii had calculated for their H-bomb work: perturbative corrections to Compton scattering. His third paper in this early series, submitted 15 July 1952, drew heavily upon recent work published in the *Physical Review* by both Murray Gell-Mann and Francis Low, and by Hans Bethe and Edwin Salpeter, applying Dyson's formalism and Feynman's diagrams to problems involving bound states (Galanin, 1952b, 1952c).[41]

Note Galanin's selective reading of Dyson's difficult papers: in none of his three papers did Galanin discuss Dyson's generalization of how to remove the infinities from any given order of approximation (the subject that occupied nearly all of Dyson's second paper, and which Dyson considered his most important contribution). Instead, during his months-long scrutiny of Dyson's and Feynman's published work, Galanin honed in on a select few of their worked examples, building his diagrammatic arsenal by focusing on those particular calculations that were most crucial

FIGURE 6
(a), From Dyson (1949a: 501), reproduced with kind permission of the American Physical Society; (b), from Galanin (1952b: 458).

a *b*

to the secret H-bomb work. Galanin and his compatriots needed to calculate real numbers to high accuracy for specific interactions between electrons and photons; from the welter of details in Feynman's and Dyson's publications, Galanin picked out and developed those elements of most immediate concern.

Galanin, just as surely as Dyson, knew that publishing formal research papers was only one way to spread new ideas and methods, and by no means the most efficient way. At the time, there was one prominent place where Soviet theorists (young and old) gathered to learn of the latest developments in their field: Lev Landau's weekly seminar, held every Thursday afternoon in the Moscow Institute for Physical Problems. Landau's seminar, much like Tomonaga's group in Tokyo, featured intense discussions in which young theorists took turns presenting the latest research from the international physics journals, or, occasionally, presenting some of their own work. Landau (or simply 'Dau' to his friends and students) kept tight control over what material would be fit for presentation in his weekly seminar (see especially Hall [1999: chapters 6–13] and Khalatnikov [1989a]). He also developed a reputation for interrupting speakers with cutting criticism whenever he thought that their presentations had wandered into 'pathology' – Landau's favorite term for physics that was not necessarily incorrect, but rather stale or overly pedantic. One of the long-time members of Landau's seminar compared it to a 'Cossack army': 'There was a sense of battle between [the speaker] and Landau, naturally of much interest to all those present, who were always very numerous, including staff of the Moscow and Dubna institutes as well as visitors from Leningrad, Kharkov, Kiev, and Novosibirsk' (Akhiezer, 1989: 51–52).[42]

One might have expected Landau to welcome the new diagrams and feature them in his influential seminar upon hearing about them from Galanin, Ioffe, and Berestetskii. Not only had he worked on QED himself, but for many years he had directed his students to master the means of making perturbative calculations as well. Reinforcing the interest in these assignments, Landau's group, and many Soviet theorists in addition, learned about the new experimental developments in QED from a lengthy review paper by Iakov Smorodinskii published in 1949. Although Smorodinskii's paper had been written too early to include any news of Feynman's or Dyson's diagrammatic work, it did highlight Hans Bethe's non-relativistic, approximate calculation of the Lamb shift, as well as Schwinger's and Tomonaga's early work on how to remove the infinities from QED (Berestetskii & Landau, 1949; Smorodinskii, 1949; Akhiezer, 1989: 44–46; Akhiezer, 1994: 36–37; Ioffe, 2002: 7–8). Beyond Smorodinskii's review, 42 papers on QED appeared in the Soviet *Zhurnal* between 1946 and 54 (accounting for nearly 30% of the journal's output in theoretical high-energy physics), while more than 100 other papers appeared on closely allied topics such as relativistic quantum mechanics, quantum field theory and field-theoretic studies of nuclear forces. QED was definitely on several Soviet theorists' agendas at the time.

Yet instead of embracing the new diagrams, Landau shunned them. Ioffe, Galanin, and Berestetskii, along with their teacher Pomeranchuk, tried several times to interest Landau in their newly discovered diagrammatic techniques. Each time, they were rebuffed by Landau, who found nothing sufficiently of interest in their hard-won squiggles. The little he heard about Feynman's and Dyson's techniques for removing the infinities from QED struck Landau – as they struck many other members of the European theoretical physics elite at the time – as little more than temporary trickery. A deeper conceptual overhaul was needed to place quantum field theory on a solid footing, Landau maintained; there was little sense fiddling with Feynman diagrams in the meantime (see Brown & Rechenberg, 1990: 67–68, 73–74).[43] As Ioffe recalled, 'Two attempts to present Feynman's papers at Landau's seminar failed: the speakers were thrown off the podium after 20 or 30 minutes of talking. Only the third attempt succeeded (if I remember correctly, this was in 1951 or even in 1952). But still he had no interest in these problems' (Ioffe, 2002: 10–11). Landau's disinterest in the diagrams did not dissipate quickly. As late as autumn 1954 – over 3 years after Galanin submitted his first diagrammatic papers to the *Zhurnal* – Landau barked at a young graduate student that it would be 'immoral' to chase such 'fashions' as Feynman diagrams (Dzyaloshinskii, 1989: 90).

After continuing to fail in their efforts to interest Landau in the new techniques, Pomeranchuk and his students decided to start their own weekly seminar. It was held in the same place and on the same day as Landau's seminar, starting 2 hours before Landau's larger seminar began. Ioffe became the secretary of the group, and gave the first presentation in the group's inaugural meeting on 1 October 1951, lecturing on Dyson's papers. Landau avoided the meetings, teasing the upstarts that they were wasting their time. Despite Landau's disapproval, however, Pomeranchuk's seminar began to attract more and more participants, and slowly news of the diagrammatic techniques began to spread (Ioffe, 2002: 11–12).[44]

First Contact

With the aid of Pomeranchuk's new seminar, plus a textbook published in 1953 by Berestetskii and Akhiezer – and despite Landau's active antipathy – a slow trickle of diagrammatic papers began to appear by Soviet physicists. By the end of 1954, the 12 research papers in *Zhurnal eksperimental'noi i teoreticheskoi fiziki* that made use of Feynman diagrams had been supplemented by a handful of additional brief notices in *Doklady Akademii Nauk SSSR* (*Proceedings of the Soviet Academy of Sciences*), many of them by Galanin, Ioffe, and Pomeranchuk themselves.[45] The diagrams really took off, becoming taken-for-granted tools among the Soviet theorists, only after physicists in the Soviet Union and in the USA began to meet again at conferences and workshops. Only then could they exchange news more freely and discuss techniques face-to-face. Just as in the USA and Japan, personal contact was the key.

No Soviet physicists were allowed to travel beyond the Warsaw Pact countries to participate in international conferences until after Stalin died in 1953; indeed, it took another 2 years before the first tentative exchanges between East and West began to take place. In the winter of 1954–55 Feynman and Dyson were invited to a small conference on QED to be held in Moscow that spring. Though both men expressed interest in participating, the Swedish theorist Gunnar Källén was the only Western physicist who managed to attend the conference, and diagrammatic techniques were far from the main agenda.[46] In August 1955, physicists from around the world gathered in Geneva for the 'Atoms for Peace' conference, at which they talked about possibilities for (civilian) nuclear power production. Immediately after the success of the Geneva meeting, calls rang out in both the USA and the Soviet Union to follow up on the 'good feelings' of the Geneva experiment by holding more international meetings (Anonymous, 1955a, 1955b).[47] Most significant for theoretical physicists was a series of exchanges that began in 1956. Three Soviet physicists attended the sixth annual 'Rochester Conference' on high-energy physics at the University of Rochester in April 1956.[48] The next month, 14 physicists from the USA visited Moscow for 2 weeks. Included among the US delegation to the May 1956 meeting were several of the world's earliest and most enthusiastic users of Feynman diagrams: not only Dyson himself, but also Jack Steinberger, Keith Brueckner, Robert Marshak, and Murray Gell-Mann. According to Luis Alvarez's diary of the trip, after the first week or so,

> both Russians and visitors have gotten quite used to being with each other, so it is no longer the case that everyone is consciously trying to be on his best behavior. We just acted naturally and had a wonderful time. There was lots of laughter and some hot arguments about physics between the theoreticians – just what goes on at any gathering of US physicists. (Alvarez, 1957b: 24–25; see also Alvarez, 1957a)

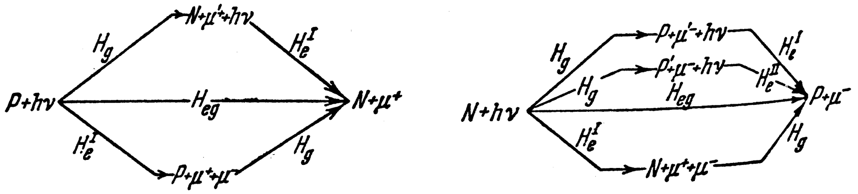
Jack Steinberger similarly told a newspaper reporter that 'the Americans and other foreigners mixed freely with Russian scientists' during the 2-week visit (Steinberger, quoted in Salisbury, 1956: 12).[49] Four months later, a delegation of Soviet theorists received clearance to attend an international conference on theoretical physics in Seattle; after the conference, they spent 2 days talking with Dyson at the Institute for Advanced Study before returning to Moscow. The following year, more Soviet physicists visited the USA to attend conferences and workshops.[50]

After the re-establishment of informal, personal contact between theorists in the two countries, Soviet theorists began to take up Feynman diagrams at a fast clip. The editors of the *Zhurnal* received more diagrammatic submissions during July 1956 – several weeks after the lengthy Moscow conference – than any previous month. The papers continued to pour in, at more than twice the rate at which diagrammatic papers had been submitted before the first visits between Soviet and US physicists. The number of authors contributing the papers likewise grew at over twice the earlier rate. Perhaps more important, the number of first-time diagrammatic authors also shot up soon after the first visits, nearly doubling from nine (between July 1955 and June 1956) to 17 (between July 1956 and June 1957) – the single largest increase between 1951 and 1959.[51] Reinforcing the new trend, many Soviet physicists welcomed the arrival of a new textbook by the Moscow-based mathematical physicists Nicolai Bogoliubov and Dmitri Shirkov, published just over a year after the first visit by the American physicists.[52]

More changed than just the rate at which Soviet theorists published diagrammatic papers. During the early 1950s, before Soviet physicists were allowed to fraternize with colleagues in the West – and before Feynman diagrams became a staple tool on Soviet soil – Soviet theorists had hardly been idle or uncreative. Instead, when they wanted to study various problems in, for example, the scattering of nuclear particles, they developed their own diagrammatic methods. Whereas several groups within the USA and Japan had already begun to use Feynman diagrams for these kinds of calculations – cavalierly dissociating Feynman diagrams from the specific set of rules that Dyson had worked so hard to pull together for the diagrams' 'proper' use – Soviet theorists turned to their own doodles instead.[53] Consider, for example, the line-drawings in Figure 7, introduced in a 1950 paper to keep track of the various ways in which photons, nucleons and mesons could interact. Just as the Japanese theorists Koba Zirô and Takeda Gyô had invented their own line-drawings to help keep track of perturbative terms before learning about Feynman diagrams (see Figures 3 and 4), so too did these Soviet theorists create their own helpful pictorial mnemonic aids.

Unlike the Japanese case, however, the home-grown Soviet diagrams were not edged out overnight upon the arrival of Feynman diagrams. In fact, they coexisted for some time with Feynman's diagrams, sometimes appearing in the same paper. Soviet theorists would only invoke Feynman diagrams when they were completing calculations closely modeled on Dyson's prescriptions; when picturing various interaction possibilities, they chose to stick with their countrymen's diagrams instead. Examples of the Soviet reaction diagrams, akin to Figure 7, could be found in the Soviet *Zhurnal* as late as 1957.[54] Soviet physicists only began to use Feynman diagrams in place of these kinds of reaction diagrams – in a more casual way, not tied to specific perturbative calculations – after they had begun to meet with their US counterparts, for whom these looser appropriations of

FIGURE 7
Reaction diagrams for keeping track of various particle interactions. From Baldin & Mikhailov (1950: 1058).



the diagrams had already become old hat.[55] In ways that no amount of written texts had done beforehand, the acts of talking with and watching their visiting US colleagues deploy Feynman diagrams for tasks similar to their own convinced several Soviet theorists to begin putting Feynman diagrams to work.

Skill-Like Knowledge in Theoretical Physics

Feynman diagrams infiltrated the rising generation of US theorists thanks largely to Freeman Dyson's direct, personal tutelage, accompanied by a cascade of newly trained recruits circulating out from the Institute for Advanced Study in Princeton. In Japan, meanwhile, Tomonaga's Tokyo group had been primed to tackle Feynman diagrams more than any other group of physicists outside the USA. They were already world-class experts on the general topic of QED and its problems, and they had logged several years practicing the finer points of making complicated perturbative calculations. They had even invented their own notations and line-drawing schemes to keep track of the many terms involved in such calculations. Prompted in part by Yukawa's early letters from the Institute for Advanced Study about Dyson's work, and by the manifestly shared purpose between the Koba–Takeda transition diagrams and Dyson's reports of Feynman diagrams, Tomonaga's group worked hard to master Dyson's papers as soon as they arrived.[56] Learning to wield the tools from printed recipes alone, however, proved remarkably difficult, as witnessed by Takeda's 'opened' closed loops and Koba's 'illustrative' early examples of Feynman diagrams. Only after nearly a year of dedicated study did some young theorists (such as Kinoshita) get up to speed in making Dysonian calculations. Tomonaga's intense, 1-year-long visit to the Institute for Advanced Study, followed by his return to Tokyo in the summer of 1950, solidified the transfer. The new techniques, and the skills required to use them, only began to travel beyond Tomonaga's tight-knit circle thanks to several well-timed contingencies: the appearance of a new mimeographed newsletter, the sudden expansion of the national university system and the inauguration of new fellowships. Politics and institutions, in other words, aligned just in time to foster effective pedagogy.

Politics likewise shaped pedagogy in the Soviet Union, where more than a year of high-pressure work on a secret military project was required before three young theorists had learned how to complete diagrammatic calculations much the way Feynman and Dyson did. Lacking a robust pedagogical environment in which to spread the new techniques, however, Berestetskii's, Galanin's, and Ioffe's extended efforts to master the diagrammatic approach nearly came to naught. Only when they took matters into their own hands, establishing their own training center to rival Landau's, did they begin, slowly, to put the diagrams into circulation. Only with the return of informal contact with their US colleagues did Soviet physicists pick up the diagrams at anything resembling theorists' pace in other countries. Absent the institutions of personal contact and pedagogical inculcation, Soviet theorists had little motive to ask how else the diagrams might come in handy. The Soviets did not see Feynman diagrams as a calculational panacea, often preferring to stick with their own reaction diagrams instead.

The spread of theoretical tools such as Feynman diagrams thus shows many similarities to other kinds of technology transfer; skills and tool-use play just as critical a role in theoretical sciences as in laboratory and field sciences. Yet the skills involved in theoretical and experimental realms are not quite the same. Dyson had written his early papers in part to serve as recipe books, making as explicit as he could the step-by-step rules needed for undertaking diagrammatic calculations. Feynman similarly had included several lengthy appendices to his own first diagrammatic paper, hoping that the explicit examples would help readers get up to speed with the new tools. With the aid of these textual instructions, something could be learned about the diagrams, even without the intense face-to-face training sessions and the postdoc cascade that put the diagrams into circulation within the USA – difficult and rare as such textual transmission proved to be. The spread of these paper tools was not the all-or-nothing affair that is so often painted in analyses of the experimental sciences: no infinite epistemic barriers separated theorists in Japan and the Soviet Union from their peers in the USA.

Paper tools such as Feynman diagrams proved even more malleable and ripe for local appropriation than pieces of physical equipment usually do. Experimentalists working with transversely excited atmosphere (TEA) lasers – to return to one of the more familiar examples of skills-transfer within an experimental realm – had one easy way to check whether they had successfully mastered the skills needed to build and use their instrument: when they flipped the switch, the machine either lased or it didn't. Other types of skills and tools, such as recipes for protocols in biological laboratories or the model organisms so common within them, possess no such simple 'look and see' tests, forcing practitioners to devise more elaborate ways to standardize procedures and outcomes.[57] Along this spectrum, Feynman diagrams and similar paper tools seem the most malleable and plastic of all, their use the most open-ended. Feynman diagrams are nothing more than representations on paper; they always require someone to interpret them and put them to work. In the process of acquiring the skills and tools – which can only happen within a particular intellectual context and pedagogical infrastructure – the tools are often changed as well: adoption almost always means adaptation. Nor are the tools the only items changed in the process; the tool-users must be fashioned to work with the tool. That nearly the only physicists in all three countries to pick up the diagrams were young theorists, still in the midst of their training, points most directly to this final lesson: paper tools and their users must be fashioned together, as part of the same pedagogical process.

Notes

We are grateful to Michael Lynch and three anonymous referees for helpful suggestions; to Sameer Shah for research assistance; and to the Spencer Foundation for financial support (grant no. 200200081) during the course of this research.

1. The literature on tacit knowledge, local practices, replication and material culture in experimental science has grown to be quite large. See especially Collins (1992 [1985]), Shapin & Schaffer (1985), Schaffer (1989, 1992), Clarke & Fujimura (1992), Kohler (1994), Pickering (1995), Galison (1997), Jackson (2000), and Delamont & Atkinson (2001). See also Polanyi (1958, 1967) and Lave (1988). Bruno Latour and others have examined metrology – the establishment of measurement standards – in similar terms. See Latour (1987: 247–57), O'Connell (1993), and Timmermans & Berg (1997).
2. For a seminal account of the philosophical motives behind the move to practices, see Hacking (1983). In addition to the literature on practices listed earlier, for the field sciences, see also Kuklick & Kohler (1996).
3. Other historians and sociologists have highlighted the importance of Soviet examples for pursuing questions of how science and technology develop and spread, given the enforced mutual isolation of Soviet and Western experts during much of the Cold War. See Graham (1998) and Collins (2001). Andrew Warwick has charted a similar example of the interplay of geopolitics with the spread of theoretical techniques in his study of general relativity during World War I; see Warwick (2003: chapter 9).
4. On QED during the interwar period, see especially Schweber (1994: chapters 1–2), Pais (1986: chapters 15–180), and Miller (1994).
5. See also John Wheeler's mimeographed notes from the Pocono meeting, a copy of which is available in the Niels Bohr Library of the American Institute of Physics, College Park, MD, USA, call number MP156. As in any quantum-mechanical calculation, this integral represented the probability amplitude; its absolute square would yield the probably for two electrons to scatter in this way. Feynman integrated over both x_5 and x_6 because the emission and absorption events could occur anywhere in space and time. The integral thus also covered the case in which the electron on the left emitted a photon at x_5, which was later absorbed by the electron on the right at x_6. Even after performing these integrations, this expression is only one-half of the one-photon term: because electrons are indistinguishable, Feynman next explained that one must include a similar integral describing the case in which the incoming electron on the left ended up, after scattering, as the outgoing electron on the right, and vice versa. In the integral, s_{56} is the relativistically invariant space–time distance between the points x_5 and x_6. The '+' labels on K and δ indicate that these functions refer only to positive energy states. We have dropped factors of i, along with the electron-labels a and b that Feynman attached to the K terms and the Dirac matrices γ_μ in his corresponding equation 4, and written d^4x in place of $d\tau$ for the four-dimensional volume element, but otherwise followed his notation. See also Schweber (1986), Mehra (1994: chapters 12–13), and Schweber (1994: chapter 8) for further details on the evolution of Feynman's work in 1947–49.
6. On Feynman's presentation of the diagrams at the 1948 Pocono meeting, see Feynman (1966), Pais (1986: 459), and Schweber (1986, 1994: chapters 4 and 8). Feynman published his new method in a pair of papers in 1949 (Feynman, 1949a, 1949b).
7. Dyson's letters to his family from this period have been preserved; see in particular Dyson to his parents, 25 June 1948, 2 July 1948, 9 July 1948, 14 September 1948, 30 September 1948, and 4 October 1948; in Professor Dyson's possession, Princeton, NJ, USA. See also Schweber (1994: chapter 9).
8. On the citations to Feynman's and Dyson's papers, see *Science Citation Index* (1961–), s.v. 'Dyson, Freeman', and 'Feynman, Richard'. On the 1951 notes' widespread distribution, see, for example, Stan Cohen, Don Edwards, and Carl Greifinger, form letter dated 9 January 1952, and Rebekah Young to S. Cohen, 15 January 1952, in Department of Physics records, University of California at Berkeley, Folder 3:31, collection number CU-68, Bancroft Library, Berkeley, CA, USA. See also Kaiser (2005: chapter 3).

9. On the sudden jump in theorist postdoc positions at the Institute for Advanced Study, see 'Director's Report', in the Board of Trustees meeting minutes of 15 April 1948, p. 4, in Institute for Advanced Study archives, Box 10; and minutes from Board of Directors meeting, 4 May 1951, p. 3, in Institute for Advanced Study archives, Box 11, Princeton, NJ, USA. See also Stern (1961: chapter 11) and Regis (1987: 137–40).
10. Freeman Dyson to his parents, 26 September 1948 and 10 October 1948; Freeman Dyson interview with David Kaiser, 8 January 2001, Princeton, NJ, USA; Wolfgang Pauli to Abraham Pais, 26 May 1949, in Pauli (1993: 655). The results from some of these collaborative calculations appeared in Watson & Lepore (1949) and Karplus & Kroll (1950).
11. See, for example, the mimeographed lecture notes by Dyson (1951, 1954a, 1954b) and Rohrlich (1953). Cf. several sets of unpublished lecture notes from Feynman's courses at Cornell and Caltech, in which he taught the new diagrammatic techniques: Feynman (1949c, 1950, 1951, 1953). On the cascade of diagram-wielding postdocs emerging from the Institute for Advanced Study, see Kaiser (2005: chapter 3).
12. Information on diagram-usage described in this paragraph is based on page-by-page counts of the papers in the *Physical Review* that either included explicit Feynman diagrams or talked about the diagrams in words; the manual search was then checked with keyword searches in the electronic archived version of the journal. Data on authors comes from Markworth (1961), Cattell (1960), Institute for Advanced Study (1980), Bilboul (1976), and *Index to Theses Accepted for Higher Degrees in the Universities of Great Britain and Ireland* (1951–). See also Kaiser (2000a: chapter 5; 2005: chapter 3).
13. Fritz Rohrlich to C.W. Ufford, 13 October 1952, in Fritz Rohrlich papers, 1948–94, Box 23, Folder, 'Correspondence, 1946–54', Syracuse University Archives, Syracuse, NY, USA.
14. Henry Stapp interview with David Kaiser, 21 August 1998, Berkeley, CA, USA.
15. Tomonaga's and Schwinger's similarity in approach and notation is not without explanation: although they worked in complete mutual isolation during the war and afterward, they both drew strongly upon the interwar work by people such as Heisenberg and Pauli. Tomonaga had studied with Heisenberg in Leipzig in 1937–39, before returning to Japan with the outbreak of World War II; he and several other Japanese theorists continued to study Heisenberg's papers intensely. Although Heisenberg had not, of course, found a solution to the problem of QED's infinities, both Tomonaga's and Schwinger's programs grew out of careful study of the same starting point. See Darrigol (1988: 3, 25–26), Maki (1988), and Schweber (1994: chapters 6 and 7).
16. J. Robert Oppenheimer, telegram to Tomonaga Sin-itiro, 14 April 1948, as reprinted in *Soryûshi-ron kenkyû* 1, no. 2 (1948): 61; Oppenheimer to Tomonaga, 28 May 1948, as reprinted in *Soryûshi-ron kenkyû* 1, no. 2 (1948): 147. Tomonaga's brief paper was published as Tomonaga (1948).
17. On the rivalry between Tomonaga's group and that of Sakata Shôichi in Nagoya, see Darrigol (1988: 21–23), Hayakawa (1988: 56–57), Itô (1988: 61–62), Kamefuchi (2001: 7–12), and Schweber (1994: 267–68). On the competition between groups in Japan and the USA, see for example, Tanikawa (1947: 530), which criticized Bethe & Oppenheimer (1946) for having overlooked key terms in the perturbative analysis.
18. Note that Koba and Takeda worked in terms of spatial-momentum three-vectors, rather than relativistic energy-momentum four-vectors.
19. On Nambu's close working relationship with Koba, see Nambu (1988).
20. It is likely that Dyson had been shown Koba and Takeda's letter by Yukawa Hideki, who was then in residence with Dyson at the Institute for Advanced Study.
21. Hideki Yukawa to Shôichi Sakata, 30 October 1948, as reprinted in *Soryûshi-ron kenkyû* 2, no. 3 (1949): 61–62.
22. See, for example, the 'homegrown' user's manual written by two young theorists working with Tomonaga: Fukuda & Itô (1949).
23. Based on Heisenberg's uncertainty principle, 'virtual' particles can borrow energy from the vacuum, as long as they pay back the borrowed energy sufficiently quickly. In order

to conserve electric charge, virtual electrons can be created along with equal numbers of positrons, so that the total electric charge of the virtual particles remains zero. The creation and annihilation of such pairs of virtual particles – known as 'vacuum polarization' – gave rise to one of the types of infinities that plagued QED. See Schweber (1994: chapter 2).

24. In his alternate calculation, Takeda assigned the argument of the electron's propagator to be $(x - y + Z/2)$, while the positron's propagator carried the argument $-(x - y - Z/2)$. He then expanded his alternate integral for the vacuum polarization current to lowest order in Z. Takeda was inspired in part by Werner Heisenberg's work from the late 1930s, in which he had introduced a universal 'fundamental length' – some finite, smallest distance closer than which no objects could pass. Heisenberg had introduced the idea in an effort to evade some of QED's infinities. Heisenberg himself had quietly dismissed his own 'fundamental length' long before Takeda's paper, and no other physicists in Japan or elsewhere seem to have pursued this line further.
25. This brief Letter to the Editor announced results of the full calculations, presented in Fukuda et al. (1950). The Letter did not contain any explicit details of the calculation; in the longer paper, the means of calculation were laid out in full, in which it becomes clear that the theorists wrote down their integrals first, using Tomonaga's formalism, and only later appended the Feynman diagrams as illustrations. For example, after writing down a particular integral, they wrote that 'The decay process $\tau^+ \rightarrow \pi^+ + \gamma$ is visualized by Feynman's diagram in [the first diagram of their paper]' (Fukuda et al., 1950: 353.)
26. The group probably turned to the Koba–Tomonaga arrow notation, rather than to the Koba–Takeda transition diagrams, because they were only trying to calculate two-photon-exchange terms, for which the arrow notation proved sufficient.
27. Early reports on this research appeared in Kinoshita & Nambu (1949, 1950a); they extended this research in Kinoshita & Nambu (1950b). To this day, Kinoshita holds the record for having calculated the highest-order perturbative corrections to electromagnetic quantities: during the 1980s, he and his collaborator, W.B. Lindquist, calculated eighth-order corrections to an electron's magnetic moment (that is, up to and including the exchange of four photons), a calculation involving 891 distinct Feynman diagrams. For an update and review, see Hughes & Kinoshita (1999).
28. Tomonaga Sin-itiro to Koba Zirô, 18 January 1950, as reprinted in *Soryûshi-ron kenkyû* 2, no. 1 (1950): 205–07; Tomonaga to Miyazima Tatsuoki, 11 February 1950, as reprinted in *Soryûshi-ron kenkyû* 2, no. 2 (1950): 11–13; Tomonaga to Koba, 12 February 1950, as reprinted in *Soryûshi-ron kenkyû* 2, no. 2 (1950): 3–4; Tomonaga to Masao Kotani, 10 March 1950, as reprinted in *Soryûshi-ron kenkyû* 2, no. 2 (1950): 10–11; and Tomonaga to Kotani, 14 May 1950, in *Soryûshi-ron kenkyû* 2, no. 3 (1950): 205–06.
29. At the time, most Japanese physicists completed undergraduate degrees (roughly equivalent to a master's degree from most US universities), and then joined the research group of a senior professor. The next formal degree that Japanese physicists might obtain would be the Doctor of Science degree, which would be granted on the basis of research already completed; few if any obtained a PhD degree. In terms of physicists' lifecycles, the DSc functioned more like the culmination of postdoctural work than like a PhD. See Kaneseki (1974: 231).
30. Information on undergraduate and DSc degrees comes from a variety of sources: Kaneseki (1974: 222–29), *Nihon Hakushi Roku* (1985), *Nihon Butsuri Gakkai Meibo* (1956, 1963), and *Gakushikai Shimeiroku* (1940–). Undergraduate information has been collected for 86% of the diagrammatic authors in Japan, and DSc information on 79%.
31. One year after Watari collaborated with Matsumoto, Matsumoto's young colleague at Shiga University, Shintomi Taro, submitted a diagrammatic paper: Shintomi (1952). See also the acknowledgments in Ozaki (1950), Ogawa et al. (1951), Koba, Mugabayashi & Nakai (1951), Ôneda (1951a, 1951b), Koba et al. (1950a, 1950b, 1951), Utiyama et al. (1952), Takahashi & Umezawa (1952), Minami et al. (1952), and Iso (1952). On interuniversity exchanges within Japan, see also Konuma (1988: 24–27).

32. Eight diagrammatic papers and five additional preprints from Nagoya theorists were submitted after Umezawa's return. Kamefuchi thanked Umezawa for help in his early diagrammatic papers: Kamefuchi (1951, 1952). Goto Shigeo thanked Umezawa in his first diagrammatic paper: Goto (1952).
33. Ônedu et al. (1950) acknowledged the aid of Tokyo theorists Fukuda Hiroshi, Miyamoto Yoneji, and Hayakawa Satio in introducing them to Feynman diagrams. S. Hori at Kanazawa, in turn, thanked both Ozaki and Ôneda in his first diagrammatic papers: Hori (1951, 1952a, 1952b). Takeda Gyô's diagrammatic papers from Kobe include Takeda (1952a, 1952b, 1952c, 1952d).
34. Hayakawa Satio to Kobayashi Minoru, 2 September 1950, as reprinted in *Soryûshi-ron kenkyû*, 2, October (1950: 165–67). Translation by Kenji Ito. Hayakawa wrote many other letters to Japanese colleagues from both MIT and Cornell, which were reprinted in *Soryûshi-ron kenkyû*, 2, 29 August (1950: 178–87), *Soryûshi-ron kinkyû* 2, October (1950: 158–60, 165–70), and *Soryûshi-ron kenkyû* 3, 5 February (1950: 211–19). See also Nambu (1988), Kinoshita (1988), and the panel discussion reprinted in Brown et al. (1988: 41).
35. See also the correspondence in Hans A. Bethe papers, Folder 10:49, collection no. 14-22-976, Division of Rare and Manuscript Collections, Cornell University Library, Ithaca, NY, USA; and in the Robert E. Marshak microfiche collection, folder 'IUPAP', microfiche 122, call no. M366, Physics–Optics–Astronomy Library, University of Rochester, Rochester, NY, USA.
36. Boris Ioffe mentions such difficulties in general: Ioffe (2002).
37. Samuel Goudsmit to Karl K. Darrow, 3 January 1951, in Samuel A. Goudsmit papers, Folder 51:51, call number AR30260, Niels Bohr Library, American Institute of Physics, College Park, MD, USA; see also E.M. Webster to Eilen Neuberger (Publications Manager, AIP), 12 March 1953, in the same folder. On the pirated copies of the *Physical Review*, see Mayer (1990: 34). A paper submitted to *Zhurnal eksperimental'noi i teoreticheskoi fiziki* on 5 June 1950 included citations to several papers in the *Physical Review* from 1949 and 1950, including one paper that had been published in the 15 May 1950 issue; a note added in proof to this same *Zhurnal* paper cited a paper from the 15 August 1950 issue of the *Physical Review.* See Baldin & Mikhailov (1950: 1063). Similarly, a *Zhurnal* paper that was submitted on 29 August 1951 cited a *Physical Review* paper that had been published in the 1 June 1951 issue: Galanin (1952b: 470).
38. On the importance of understanding the radiation reaction in the early Soviet H-bomb work, see also Gorelik (1999: 98–99). On Pomeranchuk, see Okun' (1988) and Josephson (2000: 216–17).
39. Ioffe remembers that he had already begun trying to understand Schwinger's, Feynman's, and Dyson's papers on his own during the spring of 1950, but that it had been difficult, since 'At that time nobody in Moscow was proficient in these new QED methods' (Ioffe, 2002: 9). Feynman worked through lowest-order corrections to Compton scattering in Feynman (1949b: 787–89). Galanin's translation of Feynman (1949a, 1949b) appeared in a 1951 pamphlet along with Ioffe's translation of Dyson (1949b) in *Problemy sovremennoi fiziki: Sborniki sokrashchennykh perevodov i referatov inostrannoi literatury* (*Problems of Modern Physics: Collections of Abbreviated Translations and Abstracts of Foreign Literature*), series 3, issue 11 (1951); Berestetskii's lengthy review appeared as Berestetskii (1952).
40. Galanin published a brief summary of his work in Galanin (1951).
41. Cf. Gell-Mann & Low (1951) and Salpeter & Bethe (1951).
42. Landau's Moscow seminar became legendary even in its day, and has been described by many of his former students and colleagues. See the other essays in Khalatnikov (1989a), some of which also appeared in abridged form in *Physics Today*: Khalatnikov (1989b), Ginzburg (1989), and Akhiezer (1994). See also Ioffe (2002) and Hall (1999: chapter 13).
43. Cf. Kragh (1990: 183–88), Schweber (1994: 595–605) and Cao (1997: 203–04, 214–17).

44. Beginning in early 1954, Landau finally relented and began to learn something about Feynman diagrams from Ioffe and Galanin, as well as from two of Landau's own students, Alexei Abrikosov and Isaak Khalatnikov; the latter were themselves among the earliest recruits to the diagrammatic techniques from Ioffe's, Galanin's and Pomeranchuk's new seminar. The work resulted in a series of short reports by Landau, Abrikosov, and Khalatnikov published in *Doklady Akademii Nauk SSSR* (*Proceedings of the Soviet Academy of Sciences*). These papers appear in translation in Ter Haar (1965: 607–25). Soon after they were completed, Landau admitted to a friend: 'This is the first work where I could not carry out the calculations myself' (quoted in Khalatnikov [1989b: 39]). Several years later, Landau began to make more idiosyncratic use of Feynman diagrams in his campaign to overthrow quantum field theory. See Landau (1955, 1960), Brown & Rechenberg (1990: 67–76), Gross (1990: 97–100), and Kaiser (1999).
45. In addition to Galanin (1951) and the Landau–Abrikosov–Khalatnikov papers from 1954, see also Skorniakov (1953), Akhiezer & Polovin (1953), Ioffe (1954a, 1954b), Zel'dovich (1954a, 1954b), Galanin et al. (1954), and Klepikov (1954). See also Akhiezer & Berestetskii (1953).
46. For the invitations and responses of Feynman, Dyson, Abdus Salam, and others, see the folder 'Material konferentsii po kvantovoi elektrodinamike i teorii elementarnykh chastits (1955)', Archive of the Russian Academy of Sciences, f. 471, op. 1 (1947–60), d. 210a. See also Schwartz (1955). The only record of the conference is the report of Zharkov (1955).
47. See also Fermi (1957) and Holloway (1994: 352–54).
48. See Freeman Dyson to his parents, 4 April 1956; Anonymous (1956a); Rosenbaum et al. (1956); Polkinghorne (1989: 60).
49. See also Freeman Dyson to his parents, 15 May 1956. The May 1956 Moscow visit received much attention from the press: Anonymous (1956b); Raymond (1956); see also Rosenbaum et al. (1956) and Alvarez (1957a, 1957b).
50. On the Soviet physicists' September 1956 visit to Seattle and the Institute for Advanced Study, see William Phillips to Eyvind Wichmann, 9 August 1956, and Verna Hobson (assistant to J. Robert Oppenheimer), 'Memorandum to File re: Visit of Russian Physicists', 27 September 1956, in Institute for Advanced Study archives, Folder 'General: Russian Physicists' Visit to Institute, Sept. 1956', and previous correspondence in the same folder; and Freeman Dyson to his parents, 17 October 1956. On their visit to Stanford the following year, see Anonymous (1957) and Davies (1957a, 1957b).
51. Between July 1951 and June 1956, the number of diagrammatic papers submitted to *Zhurnal eksperimental'noi i teoreticheskoi fiziki* rose at an average rate of 4.7 papers per year, whereas the number of submitted papers rose at a rate of 10.0 papers per year between July 1956 and June 1959. Similarly, the total number of authors contributing diagrammatic papers to the *Zhurnal* between July 1951 and June 1956 rose at an average rate of 4.6 authors per year, whereas the total number of authors rose at a rate of 10.5 per year between July 1956 and June 1959. Based on page-by-page counts in *Zhurnal eksperimental'noi i teoreticheskoi fiziki* of papers that either used Feynman diagrams explicitly or discussed them in the text.
52. Bogoliubov and Shirkov's textbook was first cited in *Zhurnal eksperimental'noi i teoreticheskoi fiziki* in a paper submitted in July 1957 (*Science Citation Index*, s.v. 'Bogoliubov, N.N'.) The book was soon published in translation as Bogoliubov & Shirkov (1959 [1957]). Bogoliubov and Shirkov, both based at the Steklov Mathematics Institute of the Soviet Academy of Sciences in Moscow, had earlier written a long review paper making use of Feynman diagrams, published before the first visits between US and Soviet physicists (Bogoliubov & Shirkov, 1955a, 1955b, 1955c).
53. Examples of US physicists' widening uses and interpretations of Feynman diagrams, beyond Dyson's original prescriptions, are discussed in Kaiser (2000a, 2000b, 2002, 2005).

54. See Zel'dovich (1954), Pontecorvo (1956: 136–37), Kulakov (1957: 478), and Nelipa (1958: 983).
55. Consider, for example, the work of Lev Okun', a young theorist working at the Moscow Laboratory of Thermotechnical Studies. Between January 1956 and August 1957, Okun' published 14 papers (most of them brief Letters to the Editor) on the decays of new particles, recently discovered in Soviet and US particle accelerators. Even though he co-authored some of his papers with people such as Pomeranchuk and Ioffe, he never made any use of Feynman diagrams for these non-perturbative calculations. Most of Okun's work focused on selection rules among possible decay processes in the simplest approximation, independent of perturbative corrections. Only after visiting with several US theorists, who had been using Feynman diagrams as a supplement for the same kinds of calculations as Okun' had been making, did Okun' begin to make some use of the diagrams. Compare, for example, Okun' (1956) with his first diagrammatic paper: Okun' (1958).
56. As several sociologists have noted recently, one can often aid in the transfer of tacit knowledge and craft skills by deploying 'second-order measures of skill': explicit instructions about how the skills are relevant, for what they are to be used, and how long it often takes to learn and/or use the new techniques. In this analysis, such explicit discussion rarely suffices to supplant the informal, tacit knowledge built up from personal communication and embodied practice, but the second-order measures can nonetheless help reduce the time needed to pick up the new skills. Yukawa's letters might well have functioned in this way, drawing the Japanese community's attention to Dyson's work and emphasizing that it was relevant to their own program of research – and hence deserving of extra-special scrutiny. Cf. Pinch et al. (1996).
57. Cf. Collins (1992 [1985]), Jordan & Lynch (1992), Kohler (1994), Fujimura (1996), and Creager (2002). We are grateful to an anonymous referee for suggesting these sorts of comparisons.

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