Tools without Theories

Review of *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics*, by David Kaiser. University of Chicago Press. 2005.

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Feynman diagrams have been a part of the physicist's toolkit for over 50 years. David Kaiser's masterful book uses the introduction, dispersion, and modification of these diagrams to trace two aspects of postwar physics. Physicists used diagrammatic techniques to tackle many of the central problems facing particle physics during this era, ranging from renormalization in QED to the study of the strong interaction. Kaiser recounts how the diagrammatic techniques shaped theorists' understanding of and approaches to these problems. He also uses the Feynman diagrams as a test particle to probe the changing institutional structure of physics, locating force centers (research institutes and departments), lines of force propagating out from them (in the form of peripatetic postdocs), and other types of interaction (such as textbooks). This dual vision results in an engaging labor history of physics. The book paints a detailed picture of the context, pedagogy and practice of particle physics from the late 40s to the 60s. The evocative descriptions of the physicists and their institutional settings are backed by exhaustive archival work, and Kaiser's mastery of the relevant physics is clear. Kaiser's sure hand in presenting technicalia, his eye for telling details, and his lively prose style combine to make this book a joy to read. Overall it is an impressive and engrossing book. After a brief synopsis, I will turn to critical comments regarding Kaiser's assessment of diagrammatic reasoning and his advocacy of a new historiography.

The diagrams got off to a bad start: Feynman's first public presentation of his curious line drawings to a meeting in 1948 was a failure, as Kaiser recounts. None of his listeners recognized that the diagrams could streamline the tedious calculation of term-by-term series expansions in QED. Yet within a few years, a growing group of theorists had become proficient diagrammers. Part I traces the introduction and dispersion of the diagrams. Kaiser carefully describes the web of personal contacts that made rapid spread of the diagrams possible. The center of the web was the Institute for Advanced Study at Princeton, where Freeman Dyson coached a group of postdocs in the diagrammatic methods after he had produced a set of explicit rules for diagram use and showed how they related to terms in a perturbative expansion in QED. The web reached far beyond Princeton, and Kaiser tracks the reception and development of diagrammatic techniques in Britain, Japan, and Soviet Union. Kaiser presses two main themes in recounting these developments. First, he argues that there was almost no pedagogy at a distance; physicists became proficient with diagrams, if at all, almost exclusively by personal contact and collaboration with the cognescenti. The "tacit knowledge" embodied by diagrammatic calculations was conveyed by personal interactions, and not via published articles or other texts. Second, the diagrams and their uses changed significantly within different institutional contexts, as the international case studies illustrate particularly clearly.

Parts II and III turn to the question of what the physicists were doing with these simple line drawings. For Dyson the diagrams made it possible to prove that QED is renormalizable, and their role was limited to that of a bookkeeping device used to keep track of terms in long perturbation expansions in QED. Some of the earliest applications of the diagrams were to calculate quantities such as the Lamb shift to ever higher orders in QED, following Feynman and Dyson's lead. But, as Kaiser emphasizes, the diagrams were also used in meson theory (covering interactions between nucleons and the slew of newly discovered "mesons," intermediate in mass between protons and electrons), where perturbation methods did not apply. (This led Feynman to warn Fermi, "Don't believe any calculation in meson theory which uses a Feynman diagram!" (p. 201)) Theorists continued to use diagrams even when they were no longer tied down to terms in a pertubation expansion; Marshak et al. used diagrams to study the possible interaction Hamiltonians for meson-nucleon interactions, and Golstone and others developed diagrammatic methods in many body physics. This extension of diagrammatic techniques to new problems continued in the period covered in part III. During this time, aspiring young physicists could turn to a variety of textbooks to learn diagrammatic techniques, and Kaiser describes the pedagogical and publishing trends these books exemplify. The main focus of part III is Geoffrey Chew's S-matrix program. This approach to the strong interaction built on the study of scattering amplitudes using complex analysis, and Chew pushed for treating the S-matrix autonomously without appealing to the "sterile" ideas of quantum field theory. On Kaiser's account, diagrammatic techniques provided the scaffolding for this new approach, and he emphasizes the role of Feynman-like diagrams in the study of dispersion relations, Regge poles, and the like. Alongside this story of diagrammatic improvisation, Kaiser gives an evocative account of Chew as the charismatic leader of the S-matrix program. Kaiser conveys a rich sense of Chew's context, from pedagogy to politics, although I did not find that the discussion of Chew's general philosophical convictions and commitment to democracy in various senses shed much light on the idea of "nuclear democracy" in the S-matrix program.

Hopefully this brief synopsis provides some sense of the sweep of the book, and I will now turn to two critical points. First, in assessing diagrammatic reasoning Kaiser uses ideas taken from art history (Gombrich, Goodman) and science studies (Latour). These tools strike me as unilluminating. In particular, to my mind the crucial question is not whether the diagrams are construed realistically (Kaiser's focus in Chapter 10, in particular), but rather what role the diagrams play in practice. Are the diagrams essential to calculations, or are they in principle eliminable, granted that they play an important heuristic role? In Euclidean geometry, for example, one can make a case that the diagrams themselves play an essential role, given that various argumentative steps depend upon features of the diagrams. Given the lack of explicit continuity principles before Hilbert, geometers had to appeal to diagrams to establish, for example, that two lines drawn in the diagram would intersect. Dyson clearly did not take the Feynman diagrams to be ineliminable in this strong sense; they were merely an easy way to generate terms in a perturbation expansion, but they added nothing above what was already in the mathematics. As Kaiser recounts, Feynman did not share this view, and many subsequent physicists argued that the diagrams precede the mathematics (e.g., Veltman and 't Hooft, discussed on p. 269). The problem with assessing such claims is that it is difficult to isolate the diagrams' contribution. Kaiser consistently emphasizes the importance of diagrams, but it was often unclear to me whether the diagrams per se – as opposed to an underlying non-diagrammatic mathematical technique – had a particularly important role. For example, in the discussion of the dispersion relation techniques and "polology" (Chapter 8) Kaiser puts Feynman-like diagrams in the spotlight. However, it seems plausible to see the main innovation in these approaches as coming from the application of complex analysis and analytical continuation of the scattering amplitudes rather than from the use of diagrams. Kaiser also addresses the status of diagrams on the basis of pedagogical priority: the diagrams typically appear in textbooks long before the derivation of Dyson's rules tying them to the formalism. It is not clear to me what such an argument establishes – pedagogy need not recapitulate conceptual priority. If the diagrams are essential, then it is natural to ask whether their use can lead to a stable practice, where different practitioners reliably reach the same conclusions of diagrammatic calculations. Tackling this question seems to be a promising way of assessing the resilience of diagrammatic reasoning, as opposed to Kaiser's focus on physicist's tendency to reify the diagrams.

The second criticism relates to Kaiser's arguments in the final chapter against a history of physics based on the categories of theory construction and selection. Kaiser's account does not focus on the conflict between "theories" during this period. He further argues that "theories" are not to be found in physics textbooks or in the pages of journals; indeed, theories have "vanished" and the attempts of historians to create them as convenient units of analysis are misguided. On reading this argument, I felt as if a magnificent tour of Gaudi's architectural workshop had ended with a confident declaration that *La Sagrada Familia* doesn't exist. This claim only makes sense if one accepts one or both of two dubious assumptions: (1) that the building has to be complete to count as existing, and (2) that the existence claim has a restricted domain (e.g., the interior of the workshop). As far as I can understand it, Kaiser's argument seems to be based on two analogous assumptions regarding theories. The first is that philosophers treat theories as finished products with clear empirical content and logical structure. But this mistakes an aspiration for a prerequisite, and philosophers of science are well aware that scientists typically have only partial understanding of the structure and content of current theories. The second assumption is that theories are to be individuated in terms of calculational techniques. But this restricts the existence question inappropriately; theories should be individuated on the basis of their empirical content, and looking for theories among calculational practices is looking in the wrong place. Surely there is a sense in which Newton's gravitational theory is distinct from a theory with an inverse cube force law, even if the same techniques are used to deduce the consequences. On the other hand, it would be bizarre to claim that Newton's gravitational theory was a different theory when partial differential equations were first used in its study. There is much more to be said on these issues, but Kaiser's argument does not convince me that historians concerned with theory choice and selection have been chasing a will-o-the-wisp.

To sum up, Kaiser's book exemplifies a new historiography focused on pedagogy and "paper tools," with one of the most ubiquitous paper tools in contemporary physics as its focus. Unlike Kaiser, I take this approach to complement rather than replace a history of theory construction and selection. But in any case, this book amply illustrates that this approach can lead to an enlightening history of science, especially when executed with the knowledge and skill Kaiser brings to the task.