Review

Reviewed Work(s): How Experiments End by Peter Galison

Review by: Andy Pickering

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Peter Galison. *How Experiments End.* xii + 330 pp., illus., figs., app., bibl., index. Chicago/London: University of Chicago Press, 1987. $14.95 (paper).

This book is an important contribution to new-wave studies of experiment in science. At its heart are three case studies in the history of modern physics. The first concerns measurements between 1915 and 1925 of the gyromagnetic ratio or "g-factor" of the electron; the second explores the discovery of the muon as it emerged sometime around 1937 from a decade of cosmic-ray experiments; and the third analyzes the discovery of the weak-neutral current, credited to the group of elementary-particle experimenters working around the Gargamelle bubble chamber at CERN in 1973. Peter Galison has published shorter accounts of these studies before, but much new material is added here, mainly in the form of contextual additions to already well-contextualized accounts. This heavy contextualization is what makes *How Experiments End* both interesting and important. It is, for instance, fascinating to learn in detail how Albert Einstein's thoughts on zero-point energy in quantum theory intersected with his patent-office work on gyrocompasses in the conception, execution, and interpretation of his measurement of the g-factor of the electron. More generally, heavy contextualization is integrally related to Galison's understanding of the nature of scientific experiment and its product.

Galison regards experimentation as a process, extended in time, that aims at and ends in persuasive argument sustaining empirical fact. His historical studies seek to tease out what is involved in the construction of such argument. To paraphrase his conclusion, Galison arrives at a vision of scientific culture as radically heterogeneous, comprising many disparate elements—skills, material resources and practices, interpretative models, high theory, and so on—all evolving quasi-independently on a variety of time scales: this is why thick description is historically necessary. And, if I read him correctly, he sees persuasive argument as the product of the skillful assembly of a range of such elements into a coherent package. To give just one example, speaking of the discovery of the muon he notes that "when the two groups . . . each decided to end their experiment, they were at once rendering a verdict on instruments, experiments, high-level theories, and specific models" (p. 131). All of these disparate elements, then, were made to hang together and reinforce one another in pointing to the existence of the new particle.

In emphasizing the importance of such achieved coherence in sustaining facts and marking the end points of experimental practice, Galison reinforces and elaborates a theme that is becoming familiar in modern analyses of experiment. The strength of *How Experiments End* is, I think, the wealth of interesting thought and documentation that it brings to this theme. But the book has its weaknesses, too. These stem, it seems to me, from an unresolved tension between Galison the historian and Galison the physicist. Galison the historian speaks of the situatedness of persuasion: particular arguments are persuasive for particular actors in particular contexts. This emphasis—symbolized (and simplified) throughout the book in the differing significance attached by different physicists to statistical analyses and singular "golden events"—points eventually to a radically sociological understanding of empirical knowledge in science.

Against this, however, Galison's language continually betrays the physicist's instinct that the facts are not social at all. All of the studies presented here touch upon controversies over matters of empirical fact, with one stated result eventually being displaced by another, but Galison's treatment of the competing positions is often far from symmetric. He has the knack of describing the last persuasive argument in a controversy as being so persuasive as to be, actually, unarguable, while ironicizing earlier arguments in passages like this: "*With hindsight,* we may note that the two physicists used the *wrong* cosmic ray particle . . . produced in a process which *does not occur* . . . and then invoked an absorption law that *is* also incomplete. *Nonetheless* Millikan and Cameron were able three times to produce a match between their theory and their experimental data to one part in a hundred" (pp. 84–85, emphasis added). This retrospective-realist literary undoing of all persuasive arguments except the last one goes unjustified in the text and adds nothing to our understanding of how experiments end. It serves, rather, to close off inquiries that might profitably be pursued. But this stylistic quirk is a minor blemish: *How Experiments End* is a major historical work on an exciting topic.

ANDY PICKERING

Michael Lynch. *Art and Artefact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory.* (Studies in Ethnomethodology.) xvi + 317 pp., app., bibl., index. London/Boston/Melbourne: Routledge & Kegan Paul, 1985. $39.95.

This is the sort of book that gets sociologists a bad name. Deservedly so if it is judged by the construction and the prose; undeservedly and unfortunately, however, if the substance of the work is ignored. Ten years ago, armed only with a Garfinkel and a tape recorder, Michael Lynch penetrated the interior of a neurobiological laboratory in California. In his sojourn with the people of the lab, Lynch selected two areas of their daily life for scrutiny: first, how they made electron micrographic representations of a putative neural phenomenon, axon sprouting; second, how in everyday conversation they reached agreement about the configuration of nature. The result is a study of scientific work and talk.

The strength of this book is Lynch's single-minded commitment to an ethnomethodological account of life in the laboratory. That is, in his interpretation of lab work Lynch has gone ritually deaf to the appeals his scientists made to scientific method in order to explain their results. Instead, he has sought to relate the "discoveries" and "failures" they produced to the ongoing or situational rationality associated with particular pieces of laboratory work. In the instance of axon sprouting, Lynch chose the microscopical "artifact" to explore how, in everyday situations, a natural and a social order were distinguished (i.e., continuously manufactured) by the laboratory workers. In the "discovery" of the sprouting axon Lynch shows how, by the use of conventions, various features in the tissues were exposed, selected, and, through a series of increasingly complex operations, elevated to a level of representational clarity. Lynch shows, however, that as they did this the lab people behaved as though the representations they had made were not manufactured by a conventional process at all. They called these representations natural—in the same way, perhaps, that we designate photographs as simple copies of reality. As he puts it, "It is as though the complex of theories, techniques, and instruments which were involved in disclosing such entities became transitively invisible in the way a clear pane of glass is unnoticed by the gaze that sees through it" (p. 83). The machine became the mirror of nature, as it were. On the other hand, Lynch shows that the lab folk had a quite different language for designating features in a representation as artifactual. When they did this they made an appeal to the instrumental (and thus to the social) complex involved in their creation.

In the second half of the book Lynch eavesdrops on the laboratory workers as they negotiate agreement among themselves as to the meaning of various events and appearances. Then he describes the rather different language in which the lab workers reported these negotiated outcomes as discoveries. The problem with this book is that most of the text should be in the footnotes and most of the footnotes in the text. As a stout ethnomethodologist and not a laboratory worker, Lynch has buried his reasoning and not used it to cover his tracks and legitimate his findings. The result is a text replete with tedious reportage interspersed with a dense language whose meaning is only elucidated by the footnotes. These, by contrast, are clear and theoretically informative, and can virtually be read as chapters on their own.

CHRISTOPHER LAWRENCE

Henk Zandvoort. *Models of Scientific Development and the Case of Nuclear Magnetic Resonance.* (Synthese Library Studies in Epistemology, Logic, Methodology, and Philosophy of Science, 184.) xiii + 305 pp., figs., bibl., index. Dordrecht/Boston/Lancaster: D. Reidel, 1986. (Distributed in North America by Kluwer Academic Publishers, Norwell, Mass.) Dfl 115, $49.50, £34.50. (Photo-offset from typescript.)

Nuclear magnetic resonance (NMR) in bulk matter was discovered independently by Edward Purcell and Felix Bloch and their respective teams in late 1945 and early 1946. By 1952 this discovery had been carried into several distinct lines of productive physical research, including the theory of spin-lattice relaxation, resonance-line shapes in both liquids and solids, NMR