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Reviews (continued)

Peter Galison, *How Experiments End* (Chicago, IL & London: The University of Chicago Press, 1987), 288 pp., £31.95/$39.95, £11.95/$15.95 pbk. ISBN 0-226-27914-6 (-27915-4 pbk).

Peter Galison's book contains an impressively detailed analysis of three episodes in the history of modern science. The first deals with a sequence of experiments on gyromagnetic effects — for example will the rotation of an iron bar cause it to become magnetized, and conversely will magnetization cause rotation? These experiments stem from Ampère's idea that magnetism is caused by tiny circulating electric currents, later identified with orbiting electrons. In 1915, Einstein worked on this subject. Galison's critical commentary on the design and conduct of these experiments shows the truly enviable level at which he is able to engage with the technical content of the work. He also brings out some interesting analogies between Einstein's experimental concerns and his earlier work in the Swiss federal patent office, when he was called upon to evaluate competing claims about magnetic compasses. The second case deals with the cosmic ray experiments leading to the discovery of an elementary particle, the mu-meson, in the 1930s. The third case concerns the discovery (in the early 1970s) again in elementary particle physics, of a decay process called 'weak neutral currents'. These studies sit between chapters of a more general character which are designed to set the work in the context of recent discussions in the philosophy and sociology of science.

Throughout the book a number of different aims are pursued in parallel. Partly the intention is to draw attention to the sheer importance of experiment itself. Along with a number of other

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Social Studies of Science (SAGE, London, Newbury Park and New Delhi), Vol. 21 (1991), 186–89

writers, Galison feels that experiment as such has been treated as the poor relation to theory in our models of knowledge. Thus he points out how experimenters build up their own traditions and pre-occupations that can operate independently of variations in theory. For example, experimentalists pursued their search for gyromagnetic effects over a period of time that saw radical change in scientific theory. Again, Galison wants to exhibit the internal complexity of experimental practice. Experimental traditions can grow up around specific kinds of apparatus, and this can even influence the kind of evidence that is given credibility. Galison instances the different techniques for detecting elementary particles, contrasting the visual methods using bubble chambers with the statistical methods using electronic counters. He finds that scientists using the former tend to be convinced by photographs purporting to show the tracks of particles they are looking for; while scientists using the latter are prone to dismiss this evidence (on the grounds that anything can happen once), and base their confidence on large numbers of events and statistical models.

Galison also exhibits the massive changes that have taken place in the process of experimentation. He begins with small, table-top experiments and ends with the giant, modern accelerators whose construction involves a major feat of civil engineering. The single experimenter who constructs his own apparatus, and who knows its foibles and idiosyncrasies, has given way to the research team characterized by a marked division of labour and even a hierarchical management structure. Nevertheless Galison argues that certain fundamental things stay the same. Good experiment always has been and always will be a rational encounter with an external reality, and it will always be beset by the same methodological problems. In particular, there is the endless problem of separating the effect that is meant to be under study from other known and unknown processes in nature that mimic it. How, for example, is the physicist to tell the difference between a neutron-induced event and a neutrino-induced event? For those looking for weak neutral currents the former is mere noise, while only the latter is a real signal; but the discrimination is problematic. Only when we see how experimenters convince themselves and others that they have made this discrimination can we see how an experiment ends — that is, how it comes to be seen as having established a fact, rather than being a mere artefact.

When do experiments end? Here the argument of the book begins to falter. Confident and impressive as he is in his handling of technical

detail, the author's response to more general epistemological issues is far less convincing. Galison's answer is encapsulated in a metaphor: an experiment ends when it can 'stand up in court' (276–77). This means that when all the objections that scientists can think up have been answered, then it is likely to be accepted. The questions and potential objections, of course, derive from the practices and results of other experimenters. All of this seems plausible enough. The worry comes when Galison seeks to differentiate this answer from the remarkably similar ones given by current sociologists of knowledge. Galison offers his book as a reply to these sociologists. The whole idea is to make an advance on sociological accounts which treat the background of knowledge and judgement conditioning the reception of an experiment as a matter of consensus, interest and negotiation. But how does Galison's work mark any advance? He is certainly aware that his courtroom model is a model of a social process, so what is new?

Galison's quarrel with sociologists of knowledge is that they 'denigrate the role of nature' (10). They fail to see experiment as 'the encounter of reason with the world' (278). The great difference, then, lies in the implicit claim that here is a form of realism that contrasts with sociological idealism. The question that must now be addressed is how convincingly Galison makes out this claim. Unfortunately (for the book) the argument is weak. Consider, as an example, Galison's discussion of Barnes. We are told that 'Barnes argues that it is wrong to describe the scientist as gaining knowledge through the contemplation of an external nature' (10). The reference is duly given to Barnes' *Interests and the Growth of Knowledge* (Routledge & Kegan Paul, 1977), 2. What could be more damning? Here we seem to have a sociologist doing exactly what Galison claims they do — namely, denigrating the role of (non-social) nature. The imputation, however, is false. In the passage referred to, Barnes is doing something quite other than denigrating the role of nature: he is denigrating the role of contemplation. He is denying that scientists get their knowledge through the *contemplation* of external nature. The maintenance of a body of knowledge, Barnes goes on, 'is not just a matter of how it relates to reality, but also of how it relates to the objectives and interests a society possesses by virtue of its historical development' (2). No reader of Galison could infer the presence of these features of Barnes' position from the report given of it. 'Not just *A* but also *B*' has been glossed as 'Not *A*, just *B*'.

Galison has produced a fine and impressive book, but one that is

sadly marred by misdirected polemic of the kind that I have illustrated. The care taken over scientific details is not matched by a corresponding care over the work of his fellow investigators in the field of science studies. Alas, this will be the side of the book that will attract attention, because it will confirm many people's preconceived opinions. Galison's work will become a stick to beat the sociology of knowledge, and will be seen as rescuing science from all manner of imaginary threats. I fear that, in the present climate of opinion, the charges that the author makes against sociology are all too likely to 'stand up in court'.

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