On Experimentation

The Uses of Experiment. Studies in the Natural Sciences. DAVID GOODING, TREVOR PINCH, and SIMON SCHAFFER, Eds. Cambridge University Press, New York, 1989. xviii, 481 pp., illus. $80.

Theory, the product of intellectual work, has always been more prestigious than practice, the result of manual work. Even Louis Althusser's strictures about "theoretical practice"—a purely verbal strategy to go beyond this dichotomy—were to no avail: this hierarchy of values is everywhere present. Among historians, philosophers, and sociologists of science it has led not only to an emphasis on theory at the expense of experiments but, more important, to an implicit subordination of the latter to the former. There are countless historical, philosophical, and (more recently) sociological studies of the theories of relativity, quantum mechanics, and evolution and of their respective heroes (Einstein, Bohr, Heisenberg, Darwin, and the like) but few detailed accounts of the experimental practices associated with these bodies of knowledge: the Kaufmann, Lummer and Pringsheim, and Frank-Hertz experiments and breeding practices have all been eclipsed behind grand theories despite the fact that these experiments were often (though not always) at their origin. The absolute limit of this tendency was probably attained when the historian Alexandre Koyré denied the reality of most of Galileo's experiments.

Fortunately, various authors have recently begun to question this tradition, and the book under review is one more nail in the coffin of a long dominant but truncated vision of science. Comprising 14 essays, mostly case studies by historians and philosophers of science, *The Uses of Experiment* shows that "the uses of experimental instruments are many and various." Ranging from the study of Newton's prisms to the testing of missile accuracy, the book also offers a variety of analytical approaches, from the now "standard" constructivism to the more traditional, rationalist philosophy.

Concentrating on experimental practices does not necessarily lead to the negation of the role of concepts and theories. As W. D. Hackmann notes in his paper on scientific instruments as aids to discovery, the Leyden jar, devised in 1745, helped in the formulation of new concepts about electrostatic action and the design of new electrical devices, which in turn led to new discoveries. Likewise, David Gooding's study of Faraday's work on magnets stresses the role of experiments in the formation of visual representations of the magnetic field and as a source of concepts used in the construction of theory.

The question of style of experimental research is addressed by Peter Galison and Alexi Assmus, who analyze how C. T. R. Wilson invented the cloud chamber, which became, under Rutherford's direction, the primary research tool of the Cavendish Laboratory. Immersed in what Galison and Assmus called a "mimetic tradition" that attempted to reproduce natural phenomena, Wilson was trying to reproduce clouds in a bottle. Starting with a version of the dust-chamber apparatus developed by the Scottish engineer John Aitken to study cloud formation, he transformed the apparatus under the influence of the Cavendish analytical approach characterized by its search for fundamental laws. Once the apparatus was appropriated by the analytical tradition, Wilson, more at ease in mimetic research, returned to his studies of clouds and left to others the task of seeing tracks of elementary particles. The question of the migration of an instrument from one context to another and of how such a migration influences its conceptualization is also addressed by J. A. Bennett, who sketches how the mercury column became a barometer.

Another approach to the question of the uses of experiments is exemplified by Simon Schaffer, who attempts to understand how an instrument becomes a standard, taken-for-granted resource. He follows in great detail Newton's experiments with the prism and how Newton continuously modified the descriptions of his experiments and the physical characteristics of his prisms in order to counter criticisms by scientists on the Continent who could not reproduce his results regarding the properties of the spectrum of light. Allowing no place for the possible reality of the observed facts, Schaffer's analysis of the controversy over Newton's theory of colors suggests that Newton's victory only came after he "took power over the key resources of experimental philosophy." After 1710, this authority allowed "the distribution of influential texts and instruments stamped with the imprimatur of collective assent." Once this had occurred, those who were still unable to reproduce Newton's results were simply dismissed as incompetent or using bad prisms.

The relativism (one could also say culturalism) implicit in some papers has been useful as a strategic reaction against a simplistic realist position, but it is now time to move beyond these two unidimensional poles. An interesting step in that direction was taken by the French philosopher Gaston Bachelard, who in his book *La formation de l'esprit scientifique* published in 1938 (and in many subsequent books) defended a middle position "between the realists and the nominalists, between the positivists and the formalists, between the partisans of facts and the partisans of signs." Instead of simply (and simplistically) replacing things with words, Bachelard's constructivist rationalism suggests a more complex but more fruitful path toward a proper understanding of scientific practice. Though not influenced by Bachelard, Andy Pickering, often perceived as a relativist, presents a sociological model of scientific practice that explicitly takes into consideration the resistance offered by the "material world." This "pragmatic realism" exemplified with the case of Morpurgo's experiments on fractionally charged quarks, is an interesting *tertium quid* to the positivism and nominalism denounced by Bachelard.

The study of scientific discourses can be helpful in elucidating the rhetorical use of experiments and the evolution of forms of presentation, as R. H. Naylor shows in his study of Galileo's experimental discourse. It cannot, however, replace the study of material practices: it is one thing to do an experiment and another to talk about it to convince people. As the work of G. E. R. Lloyd has shown, Greek thinkers, physicians in particular, were already very good at pointing to the importance of empirical observations to undermine their competitors' theory, though they themselves rarely made the observations or rudimentary experiments they suggested. So it is dubious to predict that, simply because "language relates both to scientific ideas and also to the practice of science," the study of scientific rhetoric "transcends the outmoded division between internal and external history," as Geoffrey Cantor suggests in his paper on the rhetoric of experiment. To believe so is only to confuse (and conflate) experimental reports and experimental practices, which are distinct though the latter need the former to become public.

Whereas in the 17th century, experiments were discussed among small groups of practitioners through their contacts, correspondence, and publications, James Secord shows that the development of the steam press in the first decade of the 19th century transformed the production of newspapers and periodicals and made possible their diffusion at a very low price, thus making information accessible to the masses. Newsworthy scientific information changed the rules of the diffusion and validation of knowledge. Secord's detailed analysis of the evolution of the controversy and of the fate of the acari, the insect allegedly created by Crosse, is not without similarity with the recent cold fusion affair, and reminds us that newspapers can become important actors in scientific debates.

Political actors can also play a role in fixing the use of experiments. As John Krige shows in his paper on the negotiations surrounding Britain's decision to join CERN (the European Organization for Nuclear Research), the decision process was far from being linear. Contingent factors and competing interests and styles of doing physics played a central role in determining an outcome that was not logically fixed at the start. In a study of debates over the accuracy of nuclear missiles, Donald Mackenzie extends the analysis of controversies to the case of technological testing. His constructivist approach suggests the important conclusion that test-ban treaties could have important effects on the development of nuclear technology.

Philosophers of science use experiments as ingredients in their theory of scientific change and in arguments against competing theories. John Worrall usefully deconstructs the myth that the famous observation of a diffraction white spot at the center of a disc's shadow served as a crucial experiment in the acceptance of the wave theory of light to argue that prediction of novel facts is not a criterion for accepting a new theory. Using different case studies, Allan Franklin describes ten "epistemological strategies" used by scientists to convince their colleagues of the validity of their results and argues that they are not conventional cultural practices but compelling rational arguments. Thomas Nickles attacks the hypothetico-deductive method by suggesting that an inductivist methodology gives experiments a more active role in the justification of knowledge. Pointing to the tendency of recent historical and sociological research to emphasize the local and contingent character of scientific knowledge, Nickles concludes his paper by asking a question that will have to be addressed: how is delocalization, decontextualization of scientific results possible?

The richness and diversity of the questions raised in *The Uses of Experiment* make this book an important contribution to the unending debate on the nature of science. However, focusing alternatively on theory and experiments should not lead to the neglect of the important question of the relation between mathematics and experiment: How do numbers emerging from these two sources come to match?

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Energy Efforts

Synthetic Fuel Technology Development in the United States. A Retrospective Assessment. MICHAEL CROW, BARRY BOZEMAN, WALTER MEYER, and RALPH SHANGRAW, JR. Praeger, Westport, CT, 1988. xiv, 175 pp. $39.95.

In 1973, the United States discovered it had become dangerously dependent on petroleum imports from the Middle East. The federal government had intermittently funded several projects that promised to produce liquid fuels from American coal. Why have such attempts failed? The authors of this book seek to answer this question by combining the methodologies of technology assessment and the history of technology. The question is important and their chosen method is appropriate, but Crow *et al.* unfortunately limit their analysis to one family of oil-from-coal processes, the direct liquefaction of coal by hydrogen (DCL). The generally more promising processes of indirect liquefaction by way of coal-based carbon monoxide are not considered at all.

We are given a modest but competent history of the development of DCL processes. The book's account of the H-Coal process, taken as a "baseline case," is a good illustration of the damaging effect of on-off funding. The process was introduced by Hydrocarbon Research, Inc. (HRI) in 1955, as a spin-off of its earlier H-Oil process. The federal Office of Coal Research underwrote the construction of a demonstration unit in 1965 but canceled its support in 1967 to concentrate on an alternative oil-from-coal process. HRI was able to get financial support from ARCO and later Ashland Oil. The federal government reentered the project after the 1973 oil shortage and provided over 80% of the funding. A pilot plant was completed in 1980, but the operation was suspended two years later when Ashland pulled out, citing falling oil prices as the main reason. The analysis given by Crow *et al.* shows that H-Coal would be uneconomic whatever the price of natural oil. The cost of synthetic oil increases in step with that of natural oil, as a result of the energy inputs required. It is also clear that the project was dogged by poor management, not least because its operation was usually divided between two firms and its oversight was shared by various federal agencies.

Given such difficulties, do we have to look any farther to explain the failure of synfuels in the United States? Only if we agree with the authors' concluding remark that the "ultimate need for synthetic fuels seems a certainty." They insist that synfuels projects have been burdened with misplaced expectations. Instead of being compelled to become competitive in price with natural oil, which they describe as a "moving target," synthetic liquid fuel should be regarded as a long-term safeguard against oil embargoes and shortages. (They note that South Africa, which has a compelling need to create a domestic source of liquid fuels, has avoided the pitfalls of the American efforts and has, in technological terms, succeeded.) Crow *et al.* argue that DCL should be treated not as a conventional private-sector technology but as a government-sheltered pioneering technology.

These conclusions reveal the limitations of the intense focus on DCL technology. Crow *et al.* have not attempted to demonstrate that synfuels will in fact be required in the future. Nor do they seek to strengthen their argument by drawing supporting evidence from the development of other pioneering technologies, bar a passing reference to the early history of computers. Consideration of the development of other synthetics and the alternatives to synfuels reveals factors that Crow *et al.* overlook.

The United States successfully developed synthetic rubber, which now accounts for around 70% of virgin rubber consumption. Why has synthetic rubber succeeded where synfuels have not? The successful technology was identified at an early stage and quickly taken, with federal support, to commercialization. The federal government controlled the price of rubber during the crucial development period, and synthetic rubber soon became competitive in price with natural rubber. Rubber was always imported from areas outside American control and hence was more subject to supply disruptions and price fluctuations. Furthermore, styrene-butadiene copolymer rubber (GR-S) offered certain advantages, notably wear resistance, over natural rubber. Synthetic rubber thus displays four features characteristic of a successful synthetic: rapid commercialization, competitive cost, advantages over the natural product, and acceptability to consumers of the natural product. Synthetic indigo and nylon, to name only two